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ANNUAL REPORT

OF THE

BOARD OF REGENTS

OF THE

SMITHSONIAN INSTITUTION,

SHOWING

THE OPERATIONS, EXPENDITURES, AND CONDITION
OF THE INSTITUTION

TO

JULY, 1889.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1890.

213636

FIFTY-FIRST CONGRESS, FIRST SESSION.

Concurrent resolution adopted by the House of Representatives May 27, 1890, *and by the Senate, June* 17, 1890.

Resolved by the House of Representatives (*the Senate concurring*), That there be printed of the Report of the Smithsonian Institution and National Museum for the years ending June 30, 1888, and June 30, 1889, in two octavo volumes for each year, 16,000 copies; of which 3,000 copies shall be for the use of the Senate, 6,000 for the use of the House of Representatives, and 7,000 for the use of the Smithsonian Institution.

LETTER

FROM THE

SECRETARY OF THE SMITHSONIAN INSTITUTION,

ACCOMPANYING

The annual report of the Board of Regents of the Institution to the end of June, 1889.

SMITHSONIAN INSTITUTION,
Washington, D. C., July 1, 1889.

To the Congress of the United States:

In accordance with section 5593 of the Revised Statutes of the United States, I have the honor, in behalf of the Board of Regents, to submit to Congress the annual report of the operations, expenditures, and condition of the Smithsonian Institution for the year ending June 30, 1889.

I have the honor to be, very respectfully, your obedient servant,

S. P. LANGLEY,
Secretary of Smithsonian Institution.

Hon. LEVI P. MORTON,
President of the Senate.
Hon. THOMAS B. REED,
Speaker of the House of Representatives.

ANNUAL REPORT OF THE SMITHSONIAN INSTITUTION TO THE END OF JUNE, 1889.

SUBJECTS.

1. Proceedings of the Board of Regents for the session of January, 1889.

2. Report of the Executive Committee, exhibiting the financial affairs of the Institution, including a statement of the Smithson fund, and receipts and expenditures for the year 1888–'89.

3. Annual report of the Secretary, giving an account of the operations and condition of the Institution for the year 1888–'89, with statistics of exchanges, etc.

4. General appendix, comprising a selection of miscellaneous memoirs of interest to collaborators and correspondents of the Institution, teachers, and others engaged in the promotion of knowledge.

CONTENTS.

Page.
Resolution of Congress to print extra copies of the Report.................... II
Letter from the Secretary, submitting the Annual Report of the Regents to Congress.. III
General subjects of the Annual Report IV
Contents of the Report.. V
  List of illustrations.. IX
Members *ex officio* of the Establishment.................................... XI
Regents of the Smithsonian Institution XII
JOURNAL OF THE PROCEEDINGS OF THE BOARD OF REGENTS............... XIII
    Stated meeting, January 9, 1889 XIII
REPORT OF THE EXECUTIVE COMMITTEE for the year ending June 30, 1889 ...
    Condition of the fund July 1, 1889.. XIX
    Receipts for the year.. XIX
    Expenditures for the year XX
    Sales and repayments XX
    Appropriations for international exchanges................................ XXI
      Details of expenditures of same XXI
    Appropriations for North American Ethnology XXII
      Details of expenditures of same.. XXIII
    Appropriation for Smithsonian Building repairs, and expenditures........ XXIV
    Appropriations for the National Museum XXIV
      Details of expenditures of same XXV
    General summary XXXI
    Income available for ensuing year XXXII
ACTS AND RESOLUTIONS OF CONGRESS relative to the Smithsonian Institution, National Museum, etc., for 1889 XXXIII

REPORT OF THE SECRETARY.

THE SMITHSONIAN INSTITUTION 1
    The Board of Regents.. 1
        Changes of members of the Board.. 1
    Finances 1
        Just claims on the Government.. 2
        Present total endowment of the Institution............................ 2
        Balance on hand July 1, 1889... 3
        Appropriations committed to the care of the Institution.............. 3
        Estimates for the next fiscal year, 1889-'90........................... 4
        Museum appropriations transferred to the Institution.................. 5
    Buildings 5
        Additional Museum building urgently needed............................ 5

Page

THE SMITHSONIAN INSTITUTION—Continued.

Buildings—Continued.

Recent accessions of material very large 5
Storage room quite insufficient 6
Fire-proofing of the west wing greatly desired 7
Research 7
Astro-physical observatory contemplated 7
Valuable apparatus lent to the Institution 8
Explorations 8
Investigations in Northern Africa by Mr. T. Williams 8
Investigations in Thibet by Mr. W. W. Rockhill 9
Collections from Egypt by Dr. James Grant Bey 9
Expected collections from Indians of Hoopa Reservation by Jeremiah Curtin 9
Expected collections from Russia and Finland 9
Also from Alaska 10
Publications 10
Classes of publication 11
Museum publications no longer included in the Miscellaneous Collections 12
General Appendix to the Annual Smithsonian Report a source of expense 12
A change in the character of the papers undertaken 12
Distribution of Smithsonian publications 13
A small portion reserved for sale 14
Facilities offered to others in publication 14
Smithsonian exchange system 15
Death of the curator, Dr. J. H. Kidder 16
Mr. William C. Winlock appointed his successor 16
Magnitude of the exchange operations 16
Cost of the exchanges to the Institution 17
Claim for increased appropriations 18
Estimate of amount required 18
Charge of 5 cents per pound made to the Departments 18
Its discontinuance recommended 18
Estimate of $27,500 for the fiscal year 1889–'90 19
Only one-third of Government publications received for transmission. 19
Comparison of present and proposed plan 19
Delay resulting from insufficient appropriations 20
Convention between the United States and other powers 20
Ocean steamers granting favor of free transportation 21
Library 21
Separate halls desired in the new Congressional Library Building, for the Smithsonian Library 22
Temporary quarters for the same suggested 23
Improvement in the reading room of the Institution 23
Efforts to increase the number of periodicals by exchange 23
Lists of scientific periodicals furnished by correspondents 25
Total addition of books received during the past year 25
Department of living animals 25
Gifts of birds and quadrupeds 25
Inconvenience resulting from limited accommodations 26
Total number of living specimens received during the year 26

Page.

THE SMITHSONIAN INSTITUTION—Continued.
- Zoological Park 27
  - Report by the Committee on Public Buildings and Grounds 27
  - Amendment introduced to the District of Columbia bill 30
  - This finally passed and made a law March 2 31
  - Land on Rock Creek selected for the purpose 31
- Miscellaneous 32
  - Assignment of rooms for scientific work 32
  - Toner lecture fund 32
  - Grants and subscriptions 33
  - Privilege of the floor of the House of Representatives 33
  - Smithsonian grounds 33
  - American Historical Association 33
  - Stereotype plates 33
  - Temporary shed for astro-physical observations 33
  - Reception 34
  - Correspondence 34

UNITED STATES NATIONAL MUSEUM 34
- Classified service of the Museum 34
- Schedule of officers and employés 36
- Increase of the collections 39
- Tabular statement from 1882 to 1889 40
- Catalogue entries 42
- Principal accessions to the collections 42
- Co-operation of Departments and Bureaus of the Government 45
- Photographic exhibit 45
- Distribution of duplicate specimens 45
- Accessions to the Museum Library 46
- Publications of the Museum 47
- Students 49
- Special researches 50
- Meetings and lectures 50
- Visitors 50
- Personnel 51
- Explorations 51
- Centennial Exposition of the Ohio Valley and Central States 51
- Marietta Centennial Exposition 53
- Injury to the collections by frequent transportation 54
- Appropriation required to maintain and improve the collections 54

BUREAU OF ETHNOLOGY 55
- Field work 55
  - Mound explorations 55
  - General field studies 56
- Office work 60
  - Linguistic map of North America in preparation 60
  - Various linguistic researches in progress 61
- List of publications of the Bureau 65
  - Annual reports 65
  - Contributions 65
  - Introductions 65
  - Bulletin 65

NECROLOGY 66
- Dr. Jerome H. Kidder 66
- Mr. James Stevenson 67

Page.
Appendix to Secretary's Report 69
I. Publications of the year 69
II. Report on international exchanges 73
III. Report on the Library 83

GENERAL APPENDIX.

Advertisement 87
The National Scientific Institutions at Berlin, by Albert Guttstadt 89
Hertz's Researches on Electrical Oscillations, by G. W. de Tunzelmann 145
Repetition of Hertz's Experiments, etc., by Frederick T. Trouton 191
Progress of Meteorology in 1889, by George W. Curtis 205
How Rain is Formed, by H. F. Blanford 287
On Aerial Locomotion, by F. H. Wenham 303
On the Movements of the Earth's Crust, by A. Blytt 325
Time-keeping in Greece and Rome, by F. A. Seely 377
Botanical Biology, by W. T. Thiselton-Dyer 399
Elementary Problems in Physiology, by J. S. Burdon Sanderson 423
On Boscovich's Theory, by Sir William Thomson 435
The Modern Theory of Light, by Oliver J. Lodge 441
Michelson's Recent Researches on Light, by Joseph Lovering 449
Photography in the Service of Astronomy, by R. Radau 469
The Life-work of a Chemist, by Sir Henry E. Roscoe 491
Memoir of Heinrich Leberecht Fleischer, by A. Müller 507
Memoir of Gustav Robert Kirchoff, by Robert von Helmholtz 527
On Heredity, by Sir William Turner 541
Anthropology in the Last Twenty Years, by Rudolph Virchow 555
Scandinavian Archæology, by Ingwald Unset 571
Progress of Anthropology in 1889, by Otis T. Mason 591
The Last Steps in the Genealogy of Man, by Paul Topinard 669
The State and Higher Education, by Herbert B. Adams 695
The Molecular Structure of Matter, by William Anderson 711
Aluminum, by H. C. Hovey 721
Alloys of Aluminum, by J. H. Dagger 725
The Eiffel Tower, by G. Eiffel 729
The Eiffel Tower, by William A. Eddy 736
The Great Terrestrial Globe at the Paris Exhibition of 1889 745
Geographical Latitude, by Walter B. Scaife 749

LIST OF ILLUSTRATIONS.

Page.

Hertz's Electrical Researches:

Fig. 1 147
Fig. 2 148
Fig. 3 150
Fig. 4 151
Figs. 5, 6 154
Fig. 7 155
Fig. 8 163
Fig. 9 166
Fig. 10 169
Fig. 11 173
Fig. 12 185

Trouton's Electrical Experiments:

Fig. 1 191
Fig. 2 193
Fig. 3 195
Fig. 4 196

Page.

Trouton's Electrical Experiments—Continued.

Fig. 5 198
Fig. 6 200
Fig. 7 201

Progress in Meteorology:

Fig. 1 241

Aerial Locomotion:

Figs. 1, 2 319
Fig. 3 320
Figs. 4, 5, 6 321

Movements of the Earth's Crust:

Fig. 1 352

The Terrestrial Globe at Paris Exposition:

Fig. 1 746
Fig. 2 747

INDEX to the volume 795

THE SMITHSONIAN INSTITUTION.

MEMBERS EX OFFICIO OF THE "ESTABLISHMENT.

(January, 1889.)

GROVER CLEVELAND, President of the United States.
JOHN J. INGALLS, President of the United States Senate *pro tempore.*
MELVILLE W. FULLER, Chief-Justice of the United States.
THOMAS F. BAYARD, Secretary of State.
CHARLES S. FAIRCHILD, Secretary of the Treasury.
WILLIAM C. ENDICOTT, Secretary of War.
WILLIAM C. WHITNEY, Secretary of the Navy.
DON M. DICKINSON, Postmaster-General.
AUGUSTUS H. GARLAND, Attorney-General.
BENTON J. HALL, Commissioner of Patents.

REGENTS OF THE INSTITUTION.

(List given on the following page.)

OFFICERS OF THE INSTITUTION.

SAMUEL P. LANGLEY, *Secretary.*

Director of the Institution, and of the U. S. National Museum.

G. BROWN GOODE, *Assistant Secretary.*

WILLIAM J. RHEES, *Chief Clerk.*

REGENTS OF THE SMITHSONIAN INSTITUTION.

By the organizing act approved August 10, 1846 (Revised Statutes, Title LXXIII, section 5580), "The business of the Institution shall be conducted at the city of Washington by a Board of Regents, named the Regents of the Smithsonian Institution, to be composed of the Vice-President, the Chief-Justice of the United States [and the Governor of the District of Columbia], three members of the Senate, and three members of the House of Representatives, together with six other persons, other than members of Congress, two of whom shall be resident in the city of Washington, and the other four shall be inhabitants of some State, but no two of the same State."

REGENTS FOR THE YEAR 1889.

The Vice-President of the United States:

JOHN J. INGALLS (elected President of the Senate *pro tem.* February 26, 1887)

The Chief-Justice of the United States:

MELVILLE W. FULLER, elected Chancellor and President of the Board January 9, 1889.

	Term expires.
United States Senators:	
JUSTIN S. MORRILL (appointed February 21, 1883)	Mar. 3, 1891.
SHELBY M. CULLOM (appointed March 23, 1885, and Mar. 28, 1889)	Mar. 3, 1895.
RANDALL L. GIBSON (appointed Dec. 19, 1887, and Mar. 28, 1889)	Mar. 3, 1895.
Members of the House of Representatives:	
SAMUEL S. COX (appointed Jan. 5, 1888, died Sept. 10, 1889)	Dec. 26, 1889.
JOSEPH WHEELER (appointed January 5, 1888)	Dec. 26, 1889.
WILLIAM W. PHELPS (appointed January 5, 1888)	Dec. 26, 1889.
Citizens of a State:	
HENRY COPPÉE, of Pennsylvania (first appointed Jan. 19, 1874)	Dec. 26, 1891.
NOAH PORTER, of Connecticut (first appointed Jan. 26, 1878)	Mar. 3, 1890.
JAMES B. ANGELL, of Michigan (first appointed Jan. 19, 1887)	Jan. 19, 1893.
ANDREW D. WHITE, of New York (first appointed Feb. 15, 1888)	Feb. 15, 1894.
Citizens of Washington:	
JAMES C. WELLING (first appointed May 13, 1884)	May 13, 1890.
MONTGOMERY C. MEIGS (first appointed December 26, 1885)	Dec. 26, 1891.

Executive Committee of the Board of Regents.

JAMES C. WELLING, *Chairman.* HENRY COPPÉE. MONTGOMERY C. MEIGS.

JOURNAL OF PROCEEDINGS OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION.

WASHINGTON, *January* 9, 1889.

The stated annual meeting of the Board of Regents of the Smithsonian Institution was held this day at half-past 10 o'clock a. m.

Present: Chief-Justice **MELVILLE W. FULLER**, Hon. J. J. **INGALLS**, Hon. J. S. **MORRILL**, Hon. S. M. **CULLOM**, Hon. R. L. **GIBSON**, Hon. S. S. **COX**, Hon. W. W. **PHELPS**, Hon. **JOS. WHEELER**, Dr. **HENRY COPPÉE**, Dr. **JAMES C. WELLING**, General M. C. **MEIGS**, and the Secretary, Mr. S. P. **LANGLEY**.

On motion of Mr. Morrill, Mr. Ingalls was called to the chair.

Excuses for non-attendance were read from Dr. **NOAH PORTER** and Dr. J. B. **ANGELL**, and the Secretary stated that Dr. A. D. **WHITE** was out of the country.

The journal of proceedings of the Board of the regular annual meeting of January 11 and the special called meeting of March 27, 1888, was read and approved.

The Secretary stated that since the last annual meeting the death had occurred of one of the most distinguished and useful members of the Board, Dr. **ASA GRAY**, and it was proper that some expression be made by the Board in regard to the loss it had sustained.

Dr. Coppée, in a few eulogistic remarks on the late Dr. Gray, portrayed his character and particularly his active usefulness as a Regent, and thought the expression of the feeling of every one of his associates should be placed upon the permanent records of the Institution. On his motion, it was

Resolved, That a committee of three be appointed, of which the Secretary shall be chairman, to prepare and record in our proceedings a resolution expressing the sentiments of the Board upon the loss of Professor Gray.

The Chair appointed Prof. S. P. Langley, Dr. Coppée, and Dr. Welling as the committee, which subsequently reported the following:

THE LATE DOCTOR ASA GRAY.

It is rarely indeed that the departure from this life of any man produces so profound and so general a sense of personal loss as has followed the death of our friend, Dr. Asa Gray. His associates in the

Board of Regents, his companions in scientific research, and the great body of younger men who looked up to him as their master, have all been made to realize that something has gone from the world which can ill be spared, and that their own lives have lost a part of that which made up their fullness.

Upon the Smithsonian Institution his loss falls with particular weight, since his active interest in its welfare is almost continuous with its existence, for he was one of the Committee of the American Academy of Arts and Sciences, the report of which upon the "plan proposed for the organization of the Smithsonian Institution," rendered in 1847, has exercised so active an influence upon the subsequent history of this establishment.

Appointed a Regent in January, 1874, to succeed Prof. Louis Agassiz, his efficient and active interest in the welfare of this Institution has been one of its most valuable possessions, and it is with deeper feeling than formal resolutions of regret unsually convey that we now endeavor to express some part of our sense of irreparable loss.

Dr. Gray's scientific reputation, while literally world-wide, was naturally greatest in his own country, for it is he who has made the botanical world acquainted with probably nearly three-fourths of the forms that grow on this northern continent; and in this country, where everything was referred to his Harvard Herbarium and to his judgment and classification, as the final court of appeal, he occupied a unique position as priest and pontiff of American botany. His botanical labors are otherwise too familiar to need rehearsal here, but it is not perhaps so generally known that he was an honored sponsor at the birth of the Darwinian Theory. In this constant correspondence with its illustrious author, Dr. Gray elicited the frequent expression of an admiration as hearty as it was sincere;* and in Europe as well as in this country our friend was recognized rather as the colleague than as the disciple of the great English naturalist.

As another distinguished botanist has said of him, in speaking on this same subject, "Wherever it was known that Asa Gray saw nothing sinister, nothing dangerous, in the teachings of Darwin, those teachings were stripped of all their terrors. The impossibility that such a man, so eminent in science, so clear in his conceptions, so pure in his morals, and so steadfast in his faith, could pass judgment upon a work that he had not thoroughly examined, or favor a doctrine that could be productive of evil, was apparent to all who knew him, and to the full extent of Dr. Gray's wide influence throughout the world, the works of Charles Darwin were stricken from the *index expurgatorius* and admitted into the family circle as safe books for all to read.

Rather with the desire that a permanent record shall be made of the appreciation in which this Board holds its departed associate than in any expectation that formal action can adequately express its sense of the great loss that we personally feel, and that this Institution has experienced, your committee submits the following resolutions:

---

* "I said in a former letter that you were a lawyer, but I made gross mistake. I am sure that you are a poet,—no, I will tell you what you are: a hybrid, a complex cross of lawyer, poet, naturalist, and theologian! Was there ever such a monster seen before?" (Darwin to Gray, September 10, 1860.)

Whereas the members of the Board of Regents of the Smithsonian Institution have been called upon to mourn the death of their distinguished colleague, the late Dr. Asa Gray, who has been actively interested in the welfare of the Institution from its beginning, and who held for fifteen years the office of Regent, with great advantage to the Institution: Therefore, be it

Resolved, That with a high appreciation of Dr. Gray's most eminent labors in the development of all scientific truth, and especially in the advancement and popularization of the study of botany; with a grateful sense of the service he has rendered to the Smithsonian Institution, and with reverence for his pure life, we record our admiration of the Christian character in which the truths of science were all seen in the same light that shone on a life of steadfast faith.

Resolved, That we mourn not only the great investigator, the teacher and the associate, whose single mind found outward expression in a manner so well remembered in its simple and indefinable charm, but that above all we grieve for the loss of a friend.

Resolved, That this preamble and the resolutions be spread on the minutes of the Board in respectful tribute to the memory of our venerated colleague, and that a copy be transmitted to his family in token of the share we take in their bereavement.

The Secretary stated that having learned from the widow of Dr. Gray that she needed about eighty copies of the second part of the "Flora of North America," by her husband, which had been published by the Smithsonian Institution, to complete the sets in her possession and render them available, he had ventured in the name of the Regents to furnish these desired volumes, and had taken the occasion to express their continued interest in the result of the labors of their late colleague; for which Mrs. Gray had asked him to express her very sincere thanks.

The chairman announced the election by joint resolution of Congress, approved by the President February 15, 1888, of Dr. Andrew B. White, of the State of New York, as Regent for the term of six years, to fill the vacancy occasioned by the death of Dr. Gray.

The chair then announced as the next business in order, the election of Chancellor.

On motion of Mr. Cox, Chief-Justice Melville W. Fuller was unanimously elected Chancellor of the Institution.

Mr. Fuller, in accepting the office, after thanking the members of the Board for the compliment, expressed his desire to promote the objects of the Institution, in whose welfare, he was well aware, the late chancellor, Chief-Justice Waite, had such great interest, and he earnestly hoped that he should be able to discharge his duties with as much fidelity and success.

Dr. Welling, chairman of the Executive Committee, presented its annual report for the year ending June 30, 1888; which was read and accepted.

On motion of Mr. Cox it was—

Resolved, That the income of the Institution for the fiscal year ending June 30, 1890, be appropriated for the service of the Institution, to be expended by the Secretary, with the advice of the executive committee, upon the basis of the operations described in the last annual report of said committee, with full discretion on the part of the Secretary as to items of expenditures properly falling under each of the heads embraced in the established conduct of the Institution.

The Secretary presented his annual report, which in accordance with the rules of the Board had been printed and distributed in advance to the members. He expressed his readiness to make additional explanations or remarks in regard to any part of the operations of the Institution.

Mr. Cullom inquired as to the Zoological Park, and the prospect of its establishment. He expressed great interest in the project and hoped it would speedily be realized.

The Secretary briefly urged the importance to science of the measure, as the means of rescuing from speedy extinction some of the animals which formerly inhabited this continent in vast numbers, and expressed his fear that if the land was not now secured (which in its natural state was pre-eminently fitted for the Park) within a year, so-called "improvements" would entirely destroy its character and adaptability.

General Meigs stated that thirty years ago he had pointed out to the Government the desirability of securing the Rock Creek region for a public park, and the land could then have been procured for an insignificant sum.

After a general expression of opinion by the Regents in favor of the proposed Zoological Park, the members of the Board in the Senate and House were requested to urge the passage of the bill by Congress as speedily as possible.

The Secretary stated that a reference had been made at the last annual meeting of a bill introduced in the Senate December 12, 1887, for the erection of a bronze statue of the late Professor Baird. This bill had passed the Senate unanimously February 9, 1888, and was referred in the House to the Committee on Library, which had not made a report.

Mr. Cox stated that if the bill came up for action in the House he had no doubt it would be favorably acted on.

The Secretary made the following remarks:

The Smithsonian contribution to the Library of Congress now consists of over a quarter of a million titles, forming a collection of its kind absolutely unequaled in the world, created mainly out of the Smithsonian income and practically a donation to the General Government. Further, nearly a quarter of the Smithsonian yearly income is indirectly devoted to the increment of this great collection.

It had been hoped that this collection would have been kept in a hall distinct from other books in the Library of Congress, but the exigencies of the demand on the Librarian have caused it not only to be crowded into insufficient space, but in an inaccessible room, so that the collection is not seen and in no way recalls the source of its contribution, and to the general public its very existence is unknown.

In the new Library of Congress building adequate space will presumably be provided for its preservation and increase, but if it seems fit to the Regents that a distinct hall or halls shall be devoted to it, and that they shall also in their construction and decoration not only be worthy of the contents, but recall that the collection is due to the Smithsonian fund, the following resolution is submitted:

Resolved, That since the Smithsonian deposit now numbers over 250,000 titles, and is still increasing at the cost of the Institution, it is, in the opinion of the Regents, desirable that in the new building for the Library of Congress sufficient provision shall be made for its accommodation and increase in a distinct hall or halls, worthy of the collections, and such as, while recalling to the visitor the name of Smithson, shall provide such facilities for those consulting the volumes as will aid in his large purpose of the diffusion of knowledge among men.

On motion of General Meigs, the resolution was adopted.

The Secretary called the attention of the Board to the act recently passed by Congress (approved by the President, January 4, 1889), to incorporate the American Historical Association, and providing that said association shall report annually to the Secretary of the Smithsonian Institution its proceedings, etc., who at his discretion shall communicate the same to Congress, and further authorizing the Regents of the Institution to receive on deposit the collections, papers, etc., of the said association.

On motion of Mr. Cullom, it was

Resolved, That the American Historical Association be and hereby is permitted to deposit its collections, manuscripts, books, pamphlets, and other material for history in the Smithsonian Institution or in the National Museum, in accordance with the provisions of the act of incorporation, and that the conditions of said deposit shall be determined by the Secretary, with the approval of the executive committee.

On motion of Mr. Cullom, the Board adjourned *sine die.*

REPORT OF EXECUTIVE COMMITTEE OF THE BOARD OF REGENTS OF THE SMITHSONIAN INSTITUTION.

(For the year ending 30th of June, 1889.)

To the Board of Regents of the Smithsonian Institution :

Your Executive Committee respectfully submits the following report in relation to the funds of the Institution, the appropriations by Congress for the National Museum and other purposes, and the receipts and expenditures for the Institution and the Museum for the year ending June 30, 1889 :

Condition of the fund July 1, 1889.

The amount of the bequest of James Smithson deposited in the Treasury of the United States, according to the act of Congress of August 10, 1846, was $515,169. To this was added by authority of Congress (act of February 8, 1867) the residuary legacy of Smithson and savings from annual income and other sources, $134,831. To this $1,000 have been added by a bequest of James Hamilton, $500 by a bequest of Simeon Habel, and $51,500 as the proceeds of the sale of Virginia bonds owned by the Institution, making in all, as the permanent Smithsonian fund in the United States Treasury, $703,000.

Statement of the receipts and expenditures of the Smithsonian Institution July 1, 1888, *to June* 30, 1889.

RECEIPTS.

Cash on hand July 1, 1888		$4,809.23	
Interest on fund July 1, 1888	$21,090.00		
Interest on fund January 1, 1889	21,090.00		
		42,180.00	
			$46,989.23
Cash from sales of publications		431.82	
Cash from repayments of freight, etc		3,328.71	
			3,760.53
Total			50,749.76

EXPENDITURES.

Building:		
Repairs, care, and improvements	$2,896.11	
Furniture and fixtures	1,147.09	
		$4,043.20
General expenses:		
Meetings	212.00	
Postage and telegraph	387.71	
Stationery	707.98	
General printing	602.11	
Incidentals (fuel, gas, stable, etc)	2,118.11	
Library (books, periodicals, etc)	1,350.33	
Salaries	18,820.74	
		24,198.98
Publications and research:		
Smithsonian Contributions	$99.22	
Miscellaneous Collections	4,240.14	
Reports	1,034.20	
Laboratory	6.68	
Apparatus	1,842.62	
Explorations	329.21	
Museum	868.05	
		8,420.12
Literary and scientific exchanges		2,329.99
Total expenditures		$38,992.29
Balance unexpended June 30, 1889		$11,757.47

The cash received from sales of publications, repayments for freight, etc., is to be credited on items of expenditure, as follows:

Postage and telegraph		$0.67
Incidentals		81.00
Library (books, periodicals, etc.)		55.20
Salaries		745.00
Smithsonian Contributions	$91.03	
Miscellaneous Collections	316.68	
Smithsonian reports	24.11	
		431.82
Museum		257.32
Exchanges		2,189.52
		$3,760.53

The net expenditures of the Institution for the year ending June 30, 1889, were therefore $35,231.76, or $3,760.53 less than the gross expenditure, $38,992.29, as above given.

In addition to the aggregate of $18,820.74 paid for salaries as shown in the above statement, the following amounts were paid for *salaries* or compensation for services:

For building	$1,500.00
For exchanges	187.50
For library	413.36
	2,100.86

All the moneys received by the Smithsonian Institution from interest, sales, refunding of moneys temporarily advanced, or otherwise, are deposited with the Treasurer of the United States to the credit of the Secretary of the Institution, and all payments are made by his checks on the Treasurer of the United States.

Your committee also presents the following statements in regard to appropriations and expenditures for objects intrusted to the care of the Smithsonian Institution by Congress:

INTERNATIONAL EXCHANGES.

Appropriation by Congress for the fiscal year ending June 30, 1889, "for expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employés," fifteen thousand dollars. (Sundry civil act, October 2, 1888; public 307, p. 27) $15,000 00

Expenditures during the fiscal year 1888–'89.

Salaries or compensation:	
1 curator, nine months eight days, at $175 per month	$1,621.67
1 curator, one month seventeen days, at $208.33 per month	322.58
I clerk, twelve months, at $150 per month	1,800.00
1 clerk, six months, at $110 per month; 1 clerk, six months, at $100 per month	1,260.00
1 clerk, six months, at $80 per month; 1 clerk, six months, at $75 per month	930.00
1 clerk, nine and one-half months, at $75 per month	712.50
1 clerk, six months, at $70 per month; 1 clerk, six months, at $60 per month	780.00
1 clerk, six months, at $75 per month; 1 clerk, six months, at $65 per month	840.00
1 clerk, twelve months, at $60 per month	720.00
1 clerk, three and one-half months, at $65 per month	227.50
1 messenger, six months, at $30 per month; 1 messenger, two and one-half months, at $25 per month; 1 messenger, three and one-half months, at $20 per month	312.50
1 packer, twelve months, at $75 per month	900.00
1 packer, twelve months, at $50 per month	600.00
1 laborer, 6 months, at $40 per month	240.00
1 agent (Germany), six months	500.00
1 agent (England), twelve months	500.00
1 translator (special)	5.00
Total salaries or compensation	12,271.75
General expenses:	
Freight	1,327.42
Packing-boxes	512.00
Printing	177.92
Postage	130.00
Binding records	97.50
Date stamps and stencils	86.75
Furniture and fixtures	106.36

Stationery, wrapping paper, twine, and miscellaneous supplies	$268.50	
Total expenditure		$14,978.20
Balance unexpended July 1, 1889		21.80
Balance remaining July 1, 1888		50.17

The cost of the international exchange system since July 1, 1886, has been as follows:

Fiscal year 1886–'87	$14,683.11
Fiscal year 1887–'88	15,113.46
Fiscal year 1888–'89	17,329.99
Total cost	$47,126.56

For the payment of this expense the Smithsonian Institution has received the following sums:

Fiscal year 1886–'87:	
From Congress	10,000.00
From other sources	696.48
Fiscal year 1887–'88:	
From Congress	12,000.00
From other sources	205.75
Fiscal year 1888–'89:	
From Congress	15,000.00
From other sources	2,189.52
Total receipts	40,091.75
Showing a balance due the Institution, July 1, 1889, of	$7,034.81

As this amount has been expended by the Smithsonian Institution in carrying on the system of exchanges over and above the amounts heretofore appropriated for its support by Congress, your committee respectfully recommends that Congress be requested to make appropriation to re-imburse the Smithson fund.

Your committee also refers to the last report of the Secretary, which states that up to 1880, inclusive, the Institution had paid $92,386.29 for exchanges, of which it is estimated that more than two-thirds were on Government account, for which the Government paid nothing whatever. Since the year 1880 the service has cost $96,065.85, of which the Government has paid $57,500, leaving nearly $40,000 of the cost to be borne by the Smithsonian Institution, and this exclusive of the rent of the rooms, which represents about $3,000 a year in addition.

NORTH AMERICAN ETHNOLOGY.

Appropriation by Congress for the fiscal year ending June 30, 1889, "for the purpose of continuing ethnological researches among the American Indians under the direction of the Secretary of the Smithsonian Institution, including salaries or compensation of all necessary employés" (Sundry civil act, October 2, 1888; public 307, p. 27.)	$40,000.00

The actual conduct of these investigations has been continued by the Secretary in the hands of Maj. J. W. Powell, Director of the Geological Survey.

The following is a classified statement of all expenditures made during the last fiscal year from this appropriation:

Classification of expenditures (A).

(*a*) Salaries or compensation:	
1 ethnologist, per annum	$3,000.00
2 ethnologists, at $2,400 per annum	4,800.00
2 ethnologists, at $1,800 per annum	3,600.00
1 ethnologist, at $1,800 per annum, seven months	1,050.00
1 ethnologist, at $1,500 per annum, three months	375.00
4 assistant ethnologists, at $1,200 per annum	4,800.00
1 assistant ethnologist, at $1,200 per annum, ten months	1,000.00
1 assistant ethnologist, at $1,200 per annum, six months	600.00
1 assistant ethnologist, $1,500 per annum, two months	250.00
1 assistant ethnologist, at $1,500 per annum, six months	750.00
1 assistant ethnologist, per annum	1,000.00
2 assistant ethnologists, at $720 per annum	1,440.00
2 copyists, at $600 per annum	1,200.00
1 modeller, at $600 per annum	592.52
1 messenger, at $600 per annum	600.00
	25,057.52
Unclassified and paid by day, job, or contract	4,488.68
Total salaries or compensation	$29,546.20
(*b*) Miscellaneous:	
Travelling expenses	3,243.45
Transportation of property	128.05
Field supplies	47.00
Instruments	16.00
Laboratory material	95.60
Photographic material	44.20
Books for library	202.39
Stationery and drawing material	59.36
Illustrations for report	114.00
Office furniture	92.50
Office supplies and repairs	218.75
Correspondence (telegrams)	4.17
Specimens	500.00
	34,311.67
Bonded railroad accounts settled by Treasury	61.19
Total	$34,372.86

Reclassified by subject-matters (B).

Sign language and picture writing	4,863.68
Explorations of mounds, eastern portion of the United States	7,426.13
Researches in archæology, southwestern portion of the United States	4,343.11
Researches, language of North American Indians	12,013.26
Salaries, office of the Director	2,790.00
Illustrations for report	515.85
	34,311.67
Bonded railroad accounts settled by Treasury	61.19
Total	$34,372.86

RECAPITULATION (1888-'89).

By appropriation for North American ethnology		$40,000.00
To amount expended to June 30, 1889, as per foregoing detailed statement of expenditures	$34,311.67	
To amount of bonded railroad accounts settled by Treasury	61.19	
	34,372.86	
Balance on hand from this appropriation to meet outstanding liabilities	5,627.14	
		40,000.00

SUMMARY (1887-'89).

It appears from the last report of the committee that, at the close of the fiscal year ending June 30, 1888, the balance then on hand of previous appropriations for this object was	7,847.08
Amount credited to appropriation because of disallowance by the Comptroller	17.00
Appropriation by Congress, October 2, 1888	40,000.00
Total available for the year ending June 30, 1889	47,864.08
Expended during the year ending June 30, 1889	34,372,86
July 1, 1889, balance to meet outstanding liabilities	13,491.22
Which balance on hand is deposited as follows:	
With disbursing clerk	4,847.92
With special disbursing agent	600.00
In the United States Treasury	8,043.30
	13,491.22

SMITHSONIAN BUILDING REPAIRS.

Appropriation by Congress "for urgent and necessary repairs to central and western portions of the Smithsonian Institution building" (sundry civil act, March 3, 1887. Public, 148, p. 4)		$15,000.00
Expended to July 1, 1888		12,719.96
Balance July 1, 1888, as per last report		2,280.04
Expenditures from July 1, 1888, to June 30, 1889:		
Paints and painting	$1,525,51	
Carpenters and miscellaneous work	53.71	
Architects	700.00	
		2,279.22
Balance deposited in United States Treasury to credit of the appropriation, to close the account, July 1, 1889		.82

NATIONAL MUSEUM.

PRESERVATION OF COLLECTIONS, JULY 1, 1888, TO JUNE 30, 1889.

Appropriation by Congress for the fiscal year ending June 30, 1889, "for the preservation, exhibition, and increase of the collections from the surveying and exploring expeditions of the Government, and from other sources, including salaries or compensation of all necessary employés" (sundry civil act, October 2, 1888. Public, 307, page 28)	$125 000.00

Classification of expenditures:		
Salaries or compensation	$108,495.66	
Supplies	3,759.79	
Stationery	1,580.43	
Specimens	2,891.74	
Books	1,087.05	
Travel	580.41	
Freight and cartage	2,409.58	
Total expenditures to June 30, 1889		120,804.66
Balance July 1, 1889		4,195.34
Disallowance on a bill for travelling expenses		3.00
Balance July 1, 1889, to meet outstanding liabilities		$4,198.34

ANALYSIS OF SALARIES AND COMPENSATION PAID FROM THE APPROPRIATION FOR PRESERVATION OF COLLECTIONS, 1888-'89—JULY 1, 1888, TO JUNE 30, 1889.

[All these persons were employed by the month or day, and many for part of the year only.]

Direction:		
Assistant Secretary Smithsonian Institution, in charge U. S. National Museum, three months at $300; nine months at $333.33 per month		$3,899.97
Scientific staff:		
3 curators (per month) at	$200.00	
2 curators (per month) at	175.00	
1 curator (per month) at	166.66	
2 curators (per month) at	150.00	
1 acting curator (per month) at	150.00	
1 curator (per month) at	125.00	
1 curator (per month) at	100.00	
3 assistant curators (per month) at	133.33	
1 assistant curator (per month) at	50.00	
1 assistant (per month) at	115.00	
1 assistant (per month) at	100.00	
1 collector (per month) at	100.00	
1 aid (per month) at	87.50	
2 aids (per month) at	75.00	
1 aid (per month) at	60.00	
1 aid (per month) at	58.33	
3 aids (per month) at	50.00	
1 aid (per month) at	40.00	
		32,000.55
Clerical staff:		
1 chief clerk (per month) at	175.00	
1 corresponding clerk (per month) at	158.33	
1 registrar (per month) at	158.33	
1 disbursing clerk (per month) at	100.00	
1 draughtsman (per month) at	83.33	
1 assistant draughtsman (per month) at	30.00	
1 clerk (per month) at	110.00	
2 clerks (per month) at	100.00	
1 clerk (per month) at	90.00	
1 clerk (per month) at	87.50	

Clerical staff—Continued.		
1 clerk (per month) at	$80.00	
1 clerk (per month) at	75.00	
2 clerks (per month) at	60.00	
1 clerk (per month) at	58.33	
4 clerks (per month) at	50.00	
1 typewriter (per month) at	45.00	
2 copyists (per month) at	55.00	
4 copyists (per month) at	50.00	
2 copyists (per month) at	48.33	
1 copyist (per month) at	46.66	
1 copyist (per month) at	45.00	
5 copyists (per month) at	40.00	
1 copyist (per month) at	35.00	
2 copyists (per month) at	30.00	
		$27,136.27
Preparators:		
1 artist (per month) at	110.00	
1 photographer (per month) at	158.33	
1 taxidermist (per month) at	80.00	
1 taxidermist (per month) at	70.00	
1 assistant taxidermist (per month) at	60.00	
1 assistant taxidermist (per month) at	35.00	
1 modeller (per month) at	125.00	
1 modeller (per diem) at	4.00	
1 preparator (per month) at	100.00	
1 preparator (per month) at	80.00	
1 preparator (per month) at	75.00	
1 preparator (per month) at	65.00	
1 preparator (per month) at	60.00	
1 preparator (per month) at	50.00	
		13,482.24
Buildings and labor:		
1 superintendent of buildings (per month) at	137.50	
1 assistant superintendent of buildings (per month) at	85.00	
17 watchmen (per month) at	50.00	
4 skilled laborers (per month) at	50.00	
1 skilled laborer (per diem) at	2.00	
1 laborer (per month) at	45.00	
7 laborers (per month) at	40.00	
18 laborers (per diem) at	1.50	
1 laborer (per month) at	25.00	
1 laborer (per diem) at	1.25	
1 attendant (per month) at	40.00	
2 attendants (per month) at	35.00	
1 attendant (per month) at	30.00	
2 cleaners (per month) at	30.00	
1 cleaner (per diem) at	1.00	
1 messenger (per month) at	45.00	
1 messenger (per month) at	35.00	
3 messengers (per month) at	25.00	
1 messenger (per month) at	20.00	
1 messenger (per month) at	15.00	
		30,019.23

Temporary help:		
Copyists	$390.19	
Preparators	255.11	
Laborers	1,033.73	
		$1,679.03
Special contract work		278.37
Total		$108,495.66

SUMMARY—PRESERVATION OF COLLECTIONS, 1889.

Direction—Assistant Secretary	3,899.97
Scientific staff	32,000.55
Clerical staff	27,136.27
Preparators	13,482.24
Buildings and labor	30,019.23
Temporary labor	1,679.03
Special contract work	278.37
Total paid for services	$108,495.66

FURNITURE AND FIXTURES, JULY 1, 1888, to JUNE 30, 1889.

Appropriation by Congress for the fiscal year ending June 30, 1889, "for cases, furniture, fixtures, and appliances required for the exhibition and safe keeping of the collections of the National Museum, including salaries or compensation of all necessary employés" (sundry civil act, October 2, 1888, Public 307, p. 28)	$40,000.00

Classification of expenditures.

Salaries or compensation:		
1 engineer of property, at $150 per month		$1,800.00
1 clerk (per month) at	$58.33	
1 copyist (per month) at	58.33	
1 copyist (per month) at	55.00	
2 copyists (per month) at	40.00	
1 copyist (per month) at	30.00	
		2,274.96
1 cabinetmaker (per diem) at	3.00	
1 carpenter (per diem) at	3.50	
6 carpenters (per diem) at	3.00	
2 carpenters (per diem) at	2.00	
		7,179.75
1 painter (per diem) at	2.50	
1 painter (per month) at	50.00	
2 painters (per diem) at	2.00	
		1,918.73
2 laborers (per month) at	50.00	
1 laborer (per month) at	40.00	
6 laborers (per diem) at	1.50	
		2,954.75
1 cleaner (per month) at	30.00	360.00
Extra temporary help:		
6 carpenters (per diem) at	3.00	
10 laborers (per diem) at	1.50	
1 painter (per diem) at	3.00	
1 copyist (per month) at	40.00	
		1,176.11
		17,664.30

Materials, etc.			
Exhibition cases	$7,933.35		
Designs and drawings for cases	170.00		
Drawers, trays, boxes	832.03		
Frames, stands, blocks, miscellaneous woodwork	1,966.92		
Glass	989.19		
Hardware and tools	1,253.83		
Cloth, plush, cotton (lining for cases and screens)	98.54		
Glass jars	17.70		
Chemicals, photo supplies, and instruments	590.21		
Lumber	1,966.83		
Paints, oil, varnish, brushes	861.61		
Office furniture, desks, mats, etc	356.57		
Chairs (for hall)	154.50		
Metal work, iron, brass, tin, etc	1,076.07		
Slate, brick, stone, plaster	395.11		
Rubber goods, hose, etc	421.88		
Fire-proof safe for disbursing clerk	412.12		
Travelling expenses	16.02		
		$19,512.48	
Total expenditure July 1, 1888–June 30, 1889			$37,176.78
Balance July 1, 1889, to meet outstanding liabilities			2,823.22

HEATING AND LIGHTING, ETC., JULY 1, 1888, TO JUNE 30, 1889.

Appropriation by Congress for the fiscal year ending June 30, 1889, "for expense of heating, lighting, and electrical and telephonic service for the National Museum" (sundry civil act, October 2, 1888, Public 307, p. 28)	$12,000.00	
Appropriation by Congress "for expenses of heating the U. S. National Museum for the fiscal year ending June 30, 1889" (deficiency act, March 2, 1889, Public 153, p. 5.)	1,000.00	
		13,000.00

Classification of expenditures.

Salaries or compensation:			
1 engineer, at $120 per month	$1,440.00		
1 chief fireman and machinist, at $65 per month.	130.00		
5 firemen, at $50 per month	2,950.00		
1 telephone clerk, at $55 per month	660.00		
1 telegraph clerk, at $40 per month	255.00		
		5,435.00	
General expenses:			
Coal and wood	4,188.43		
Gas	1,111.11		
Telephones	600.16		
Electric work	47.24		
Rental of call-boxes	110.00		
Heating repairs	418.73		
		6,475.67	
Total expenditure July 1, 1888–June 30, 1889			11,910.67
Balance July 1, 1889, to meet outstanding liabilities			$1,089.33

OTHER MUSEUM APPROPRIATIONS.

The balances remaining of the following appropriations, as stated in the last report of the committee, were carried under the action of Revised Statutes, section 3090, by the Treasury Department, to the credit of the surplus fund, July 1, 1889:

Preservation of collections, 1886	$1.66
Preservation of collections, 1887	.02
Furniture and fixtures, 1886	45.05
Furniture and fixtures, 1887	74.97
Preservation (armory), 1886	7.64
Heating and lighting, 1887	18.54

PRESERVATION OF COLLECTIONS, 1887-'88.

The balance of $10,345.05 remaining July 1, 1888, of this appropriation, as stated in the last report of the committee, has been expended, during the year 1888-'89, as follows:

Balance remaining July 1, 1888		$10,345.05
Salaries or compensation	$982.09	
Supplies	818.67	
Stationery	487.36	
Specimens	6,758.94	
Books	289.25	
Travel	163.66	
Freight	802.39	
Expenditure		10,302.36
Balance July 1, 1889		42.69

The classification of the total expenditure of this appropriation is as follows:

Amount appropriated		$116,000.00
Expended:		
Salaries	$97,493.52	
Supplies	3,427.05	
Stationery	2,279.56	
Specimens	8,797.59	
Books	789.61	
Travel	986.51	
Freight	2,183.47	
Total expenditure		115,957.31
Balance July 1, 1889		42.69

FURNITURE AND FIXTURES, 1887-'88.

The balance reported July 1, 1888, of $1,716.96, of this appropriation has been expended during the past year as follows:

Balance July 1, 1888		$1,716.96
Glass	$648.05	
Drawers, trays, boxes, etc	208.87	
Hardware and fittings	259.03	

Glass jars	$51.20	
Chemicals and apparatus	24.34.	
Lumber	184.71	
Paints and oil	32.00	
Office furniture	275.00	
Plumbing, tin, lead, etc	15.05	
Expenditure		$1,698.25
Balance July 2, 1889		18.71

The following is a classification of the total expenditure of the appropriation:

Amount appropriated			$40,000.00

EXPENDITURE.

Salaries or compensation:			
Engineer of property, clerks, and copyists	$3,970.00		
Carpenters	7,807.75		
Painters	2,020.00		
Laborers	4,926.04		
Cleaners	480.00		
		$19,203.79	
Materials, etc.:			
Exhibition case frames	$7,383.44		
Designs and drawings for cases	305.00		
Glass	3,438.16		
Drawers, trays, boxes, etc	804.01		
Hardware and fittings for cases	1,133.94		
Iron brackets	126.30		
Cloth, cotton, felt (lining for cases)	420.24		
Glass jars and containers for specimens	274.49		
Chemicals and apparatus	402.67		
Lumber	2,325.69		
Tools	191.68		
Paints and oils	781.99		
Office furniture and other fixtures	2,059.75		
Plumbing, tin, lead, etc	904.59		
Slate, tiles, etc	29.50		
Brushes, brooms, pitchers, etc	111.47		
Paper	49.50		
Travelling expenses	35.08		
		20,777.50	
Total expenditure			39,981.29
Balance			18.71
Credit by disallowance on travel account			3.25
Balance July 1, 1889			$21.96

HEATING, LIGHTING, ETC., 1887-'88.

The balance reported last year (July 1, 1888), of $755.89 has been expended during 1888-'89, as follows:

Balance July 1, 1888		$755.89
Gas	$155.89	
Telephones	183.00	

Electric work	$143.30	
Rental of call-boxes	20.00	
Heating repairs	250.00	
Expenditure		$752.19
Balance July 1, 1889		3.70

The total expenditures of this appropriation have been as follows:

Amount appropriated			$12,000.00

EXPENDITURE.

Salaries or compensation:			
Engineer	$1,440.00		
Telegraph and telephone clerks	1,140.00		
Firemen and machinists	3,473.36		
		$6,053.36	
Supplies:			
Coal and wood	3,014.08		
Gas	950.98		
Telephones	771.65		
Electric work	436.50		
Rental of call-boxes	130.00		
Heating repairs	639.73		
		5,942.94	
Total expenditures			11,996.30
Balance July 1, 1889			$3.70

RECAPITULATION.

The total amount of the funds administered by the Institution during the year ending 30th of June, 1889, appears, from the foregoing statements and the account books, to have been as follows:

SMITHSONIAN INSTITUTION.

From balance of last year, July 1, 1888		$4,809.23	
From interest on Smithsonian fund for the year		42,180.00	
From sales of publications	$431.82		
From repayments for freight, etc	3,328.71		
		3,760,53	
Total			$50,749.76

Appropriations committed by Congress to the care of the Institution.

International exchanges:		
Balance, 1888	$50.17	
For 1888–'89	15,000.00	
		$15,050.17
North American ethnology:		
Balance, 1888	$7,847.08	
For 1888–'89	40,000.00	
		47,847.08
Smithsonian building repairs:		
Balance, 1888		2,280.04
Preservation of collections:		
Balance, 1888	10,345.05	
For 1888–'89	125,000.00	
		135,345.05

Furniture and fixtures:			
Balance, 1888	$1,716.96		
For 1888-'89	40,000.00		
		$41,716.96	
Heating, lighting, etc.:			
Balance, 1888	755.89		
For 1888-'89	13,000.00		
		13,755.89	
Total			$255,995.19
Grand total			$306,744.95

The committee has examined the vouchers for payments made from the Smithsonian income during the year ending 30th June, 1889, all of which bear the approval of the Secretary of the Institution, or, in his absence, of the assistant secretary as acting Secretary, and a certificate that the materials and services charged were applied to the purposes of the Institution.

The committee has also examined the accounts of the "international exchanges," and of the "National Museum," and finds that the balances above given correspond with the certificates of the disbursing clerk of the Smithsonian Institution, whose appointment as such disbursing officer was accepted, and his bonds approved, by the Secretary of the Treasury.

The quarterly accounts-current, the vouchers and journals, have been examined and found correct.

The abstracts of expenditures and balance sheets under the appropriation for "North American ethnology" have been exhibited to us; the vouchers for the expenditures, after approval by the Director of the Bureau of Ethnology, are paid by the disbursing clerk of said Bureau, and after approval by the Secretary of the Smithsonian Institution are transmitted to the accounting officers of the Treasury Department for settlement. The disbursing officer of the Bureau is accepted as such and his bonds approved by the Secretary of the Treasury. The balance available to meet outstanding liabilities on 1st July, 1889, as reported by the disbursing clerk of the Bureau, is $13,491.22.

Statement of regular income from the Smithsonian fund, to be available for use in the year ending June 30, 1890.

Balance on hand June 30, 1889	$11,757.47
Interest due and receivable July 1, 1889	21,090.00
Interest due and receivable January 1, 1890	21,090.00
Total available for year ending June 30, 1890	$53,937.47

Respectfully submitted.

JAMES C. WELLING.
HENRY COPPÉE.
M. C. MEIGS.

WASHINGTON, *October* 15, 1889.

ACTS AND RESOLUTIONS OF CONGRESS RELATIVE TO THE SMITHSONIAN INSTITUTION, NATIONAL MUSEUM, ETC.

(In continuation from previous reports.)

[Fiftieth Congress, second session, 1888–'89.]

INTERNATIONAL EXCHANGES.

INTERNATIONAL EXCHANGES—SMITHSONIAN INSTITUTION: For expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employees, fifteen thousand dollars.

(Sundry civil appropriation act. Approved March 2, 1889. Statutes, XXV, p. 952.)

NAVAL OBSERVATORY: For payment to Smithsonian Institution for freight on Observatory publications sent to foreign countries, one hundred and thirty-six dollars.

(Legislative, executive, and judicial appropriation act. Approved February 26, 1889. Statutes, XXV, p. 733.)

UNITED STATES PATENT OFFICE: For purchase of books, and expenses of transporting publications of patents issued by the Patent Office to foreign Governments, three thousand dollars.

(Legislative, executive, and judicial appropriations act. Approved February 26, 1889. Statutes, XXV, p. 737.)

WAR DEPARTMENT: For the transportation of reports and maps to foreign countries through the Smithsonian Institution, one hundred dollars.

(Sundry civil appropriation act. Approved March 2, 1889. Statutes XXV, p. 970.)

UNITED STATES GEOLOGICAL SURVEY: For the purchase of necessary books for the library, and the payment for the transmission of public documents through the Smithsonian exchange, five thousand dollars; in all four hundred and three thousand dollars.

(Sundry civil appropriation act. Approved March 2, 1889. Statutes XXV, p. 960.)

NORTH AMERICAN ETHNOLOGY.

For the purpose of continuing ethnological researches among the American Indians, under the direction of the Secretary of the Smithsonian Institution, including salaries or compensation of all necessary employees, forty thousand dollars.

(Sundry civil appropriation act. Approved March 2, 1889. Statutes XXV, p. 952.)

NATIONAL MUSEUM.

Heating and Lighting: For expense of heating the United States National Museum for the fiscal year ending June thirtieth, eighteen hundred and eighty-nine, one thousand dollars.

(Act to supply deficiencies. Approved March 2, 1889. Statutes XXV, p. 909.)

Heating and Lighting: For expense of heating, lighting, and electrical and telephonic service for the National Museum, twelve thousand dollars.

Preservation of Collections of the National Museum: For the preservation, exhibition, and increase of the collections from the surveying and exploring expeditions of the Government, and from other sources, including salaries or compensation of all necessary employees, one hundred and forty thousand dollars.

Furniture and Fixtures of the National Museum: For cases, furniture, fixtures, and appliances required for the exhibition and safe-keeping of the collections of the National Museum, including salaries or compensation of all necessary employees, thirty thousand dollars.

Postage: For postage-stamps and foreign postal-cards for the National Museum, one thousand dollars.

(Sundry civil appropriation act. Approved March 2, 1889. Statutes XXV, pp. 952, 953.)

Public Printing and Binding for the National Museum: For printing labels and blanks for the use of the National Museum, and for the "Bulletins," and annual volumes of the "Proceedings" of the Museum, ten thousand dollars.

(Sundry civil appropriation act. Approved March 2, 1889. Statutes XXV, p. 979.)

Fish Commission: For altering and fitting up the interior of tl Armory Building, on the Mall, city of Washington, now occupied as a hatching station, for the accommodation of the offices of the United States Fish Commission, and for general repairs to said building, including the heating apparatus, and for repairing and extending the outbuildings, seven thousand dollars, or so much thereof as may be necessary, the same to be immediately available and to be expended under the direction of the Architect of the Capitol; and for the purpose above named the Secretary of the Smithsonian Institution is hereby required to move from the second and third stories of this building all properties except such as are connected with the workshops hereinafter named, under his control; and the workshops now in the second story of said building shall be transferred to and provided for in the third story thereof. And the Architect of the Capitol is hereby directed to examine and make report to Congress at its next regular session as to the practicability and cost of constructing a basement story under the National Museum Building.

(Sundry civil appropriation act. Approved March 2, 1889. Statutes XXV, p. 953.)

ZOOLOGICAL PARK.

Sec. 4. For the establishment of a zoological park in the District of Columbia, two hundred thousand dollars, to be expended under and in accordance with the provisions following, that is to say:

That in order to establish a zoological park in the District of Columbia, for the advancement of science and the instruction and recreation of the people, a commission shall be constituted, composed of three persons, namely: The Secretary of the Interior, the president of the board of Commissioners of the District of Columbia, and the Secretary of the Smithsonian Institution, which shall be known and designated as the commission for the establishment of a zoological park.

That the said commission is hereby authorized and directed to make an inspection of the country along Rock Creek, between Massachusetts avenue extended and where said creek is crossed by the road leading west from Brightwood crosses said creek, and to select from that district of country such a tract of land, of not less than one hundred acres, which shall include a section of the creek, as said commission shall deem to be suitable and appropriate for a zoological park.

That the said commission shall cause to be made a careful map of said zoological park, showing the location, quantity, and character of each parcel of private property to be taken for such purpose, with the names of the respective owners inscribed thereon, and the said map shall be filed and recorded in the public records of the District of Columbia; and from and after that date the several tracts and parcels of land embraced in such zoological park shall be held as condemned for public uses, subject to the payment of just compensation, to be determined by the said commission and approved by the President of the United States, provided that such compensation be accepted by the owner or owners of the several parcels of land.

That if the said commission shall be unable to purchase any portion of the land so selected and condemned within thirty days after such condemnation, by agreement with the respective owners, at the price approved by the President of the United States, it shall, at the expiration of such period of thirty days, make application to the supreme court of the District of Columbia, by petition, at a general or special term, for an assessment of the value of such land, and said petition shall contain a particular description of the property selected and condemned, with the name of the owner or owners thereof, and his, her, or their residences, as far as the same may be ascertained, together with a copy of the recorded map of the park; and the said court is hereby authorized and required, upon such application, without delay, to notify the owners and occupants of the land and to ascertain and assess the value of the land so selected and condemned by appointing three commissioners to appraise the value or values thereof, and to return the appraisement to the court; and when the values of such land are thus ascertained, and the President shall deem the same reasonable, said values shall be paid to the owner or owners, and the United States shall be deemed to have a valid title to said lands.

That the said commission is hereby authorized to call upon the Superintendent of the Coast and Geodetic Survey, or the Director of the Geological Survey to make such surveys as may be necessary to carry into effect the provisions of this section; and the said officers are hereby authorized and required to make such surveys under the direction of said commission.

(Appropriation act to provide for expenses of the government of the District of Columbia, etc. Approved March 2, 1889. Statutes xxv, p. 808.)

AMERICAN HISTORICAL ASSOCIATION.

CHAP. 20.—AN ACT to incorporate the American Historical Association.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That Andrew D. White, of Ithaca, in the State of New York; George Bancroft, of Washington, in the District of Columbia; Justin Winsor, of Cambridge, in the State of Massachusetts; William F. Poole, of Chicago, in the State of Illinois; Herbert B. Adams, of Baltimore, in the State of Maryland; Clarence W. Bowen, of Brooklyn, in the State of New York, their associates and successors, are hereby created in the District of Columbia a body corporate and politic, by the name of the American Historical Association, for the promotion of historical studies, the collection and preservation of historical manuscripts, and for kindred purposes in the interest of American history and of history in America. Said association is authorized to hold real and personal estate in the District of Columbia so far only as may be necessary to its lawful ends to an amount not exceeding five hundred thousand dollars, to adopt a constitution, and to make by-laws not inconsistent with law. Said association shall have its principal office at Washington, in the District of Columbia, and may hold its annual meetings in such places as the said incorporators shall determine. Said association shall report annually to the Secretary of the Smithsonian Institution concerning its proceedings and the condition of historical study in America. Said Secretary shall communicate to Congress the whole of such reports, or such portion thereof as he shall see fit. The Regents of the Smithsonian Institution are authorized to permit said association to deposit its collections, manuscripts, books, pamphlets, and other material for history in the Smithsonian Institution or in the National Museum, at their discretion, upon such conditions and under such rules as they shall prescribe.

(Approved, January 4, 1889, Statutes XXV, p. 640.)

SPECIAL MEETING OF THE REGENTS.

WASHINGTON, D. C., *November* 18, 1887.

A special meeting of the Board of Regents of the Smithsonian Institution was held this day at the Institution at half past 10 o'clock A. M.

Present, Hon. MORRISON R. WAITE, Chief Justice of the United States, Chancellor of the Institution; Hon. JOHN J. INGALLS, President of the Senate of the United States; Hon. JUSTIN S. MORRILL, Hon. SHELBY M. CULLOM, Hon. WILLIAM L. WILSON, Prof. ASA GRAY, Prof. HENRY COPPÉE, Dr. JAMES C. WELLING, Gen. MONTGOMERY C. MEIGS, Prof. JAMES B. ANGELL.

The Chancellor stated that the present meeting had been called in accordance with the provisions of the act of Congress organizing the Institution, at the request of three of the Regents which had been made to the Acting Secretary in the following communication:

SIR: At a meeting of the Executive Committee of the Board of Regents of the Smithsonian Institution, November 3, 1887, the following preamble and resolutions were adopted:

Whereas, the death of Professor Baird, the honored Secretary of the Smithsonian Institution, occurred at a time in the last summer when from the absence of certain Regents in Europe, and from the dispersion of others in different parts of the country, it was found impracticable to summon the Board of Regents in extraordinary session, that it might take appropriate action in the premises under the immediate pressure of that deplorable event; and

Whereas, the time has now come when such an extraordinary meeting is practicable, and is believed to be required alike by the proprieties and by the possible exigencies of the situation resulting from the lamented death of the late Secretary: Therefore be it

Resolved, That the Acting Secretary of the Institution be requested to call a special meeting of the Board of Regents to be held on Friday, November 18, at 10:30 A. M.

JAMES C. WELLING.
HENRY COPPÉE.
M. C. MEIGS.

The Chancellor read the following letter from Dr. Noah Porter, one of the Regents:

YALE COLLEGE, *November* 14, 1887.

DEAR SIR: I had made all necessary arrangements to be present at the meeting of the Regents which has been called for the 18th instant, when I was summoned to respond to another engagement of long standing, the time for which was fixed on the same day. I regret that I can not be present at Washington as it would give me very great satisfaction to honor the memory of our late distinguished Secretary for the singular fidelity, forecast, and devotion with which he has discharged the manifold duties of this office, and the eminent success which has crowned his enterprising labors. Under his administration the Smithsonian Institution has enlarged its sphere of usefulness and activity and has established itself most firmly in the confidence and esteem of the American people. The direct services which the late Secretary rendered to the wealth and welfare of the American people through his connection with the Fish Commission and the honor which he gained for his country abroad are too well known to need any comment, while his personal simplicity and integrity are above all praise.

Very respectfully,

NOAH PORTER.

S. P. LANGLEY, Esq.,
Acting Secretary of the Smithsonian Institution.

The Chancellor, Chief Justice Waite, then made the following remarks:

GENTLEMEN OF THE BOARD OF REGENTS: It is my sad duty to announce to you the death of Spencer Fullerton Baird, LL. D., the Secretary of the Institution, at Wood's Holl, Mass., on the 19th day of August last. Professor Baird was appointed by the lamented Professor Henry, while Secretary of the Institution, on the 5th of July, 1850, under the authority of this Board, to the office of Assistant Secretary "in the department of natural history, to take charge of the Museum, and to render such other assistance as the Secretary may require." He entered at once on the performance of his duties, and until the death of Professor Henry, nearly 28 years afterwards, filled his place with great ability, and to the entire satisfaction of his distinguished chief and of the Regents.

Professor Henry died on the 13th of May, 1878, and on the 17th of the same month Professor Baird was unanimously chosen his successor as Secretary of the Institution. From that day until he died he was faithful to every duty of his high office, and devoted himself untiringly to giving effect to the will of our munificent founder by the "increase and diffusion of knowledge among men."

As his death occurred when some of you were absent in Europe, and others away in different parts of this country, it was found impracticable to get an extraordinary meeting of the Board to take action upon the deplorable event at that time. We have now met for that purpose and I invite your special attention to the subject.

Senator Justin S. Morrill moved that Prof. S. P. Langley be appointed to fill the vacancy in the office of Secretary created by the death of Professor Baird.

It having been represented that the Executive Committee had prepared a minute of proceedings to be submitted to the Board, and that paper having been called for, it was read by the chairman, Dr. J. C. Welling:

"The Executive Committee beg leave respectfully to represent that in the preamble and resolution accompanying the call of the Acting Secretary for the present extraordinary meeting of the Board of Regents, they suppose themselves to have sufficiently set forth the reasons why this call has been so long delayed; the reasons which dictate the expediency of holding an extraordinary meeting at the present time, and therefore the objects which may properly engage the attention of the Board in view of the proprieties and exigencies of the situation resulting from the lamented death of the late Secretary.

Cherishing for the late Professor Baird the profound regard inspired by his talents, by his great attainments, by his life-work in the cause of science, and by his distinguished services to the Smithsonian Institution, and not doubting that this sentiment is shared by every member of the Board, your committee have thought that it was due alike to the memory of the departed Secretary whom we all held in highest honor, and to our own sense of the loss which the scientific world in common with this Institution has sustained in his death, that we should proceed, at the earliest practicable day, to take that appropriate action in the premises which is dictated by our intimate official and personal relations with the departed Secretary, and by a sincere desire on our part to testify and record our heartfelt admiration of the great and good man whose death we deplore.

With regard to any exigencies, actual or contingent, resulting from the death of the late Secretary, it does not need to be said that first in order and first in importance stands the electing of a Secretary. Though the transactions had by the Board at the last annual meeting, in the appointment of the Assistant Secretary, who is now the Acting Secretary of the Institution, may have simplified the solution of this problem so far as *we* are concerned, yet there are obvious considerations of delicacy which, in the case of a sensitive and refined nature like that of the eminent man in question, must preclude him from acting with official freedom, and with a full sense of executive authority, until the mind of the Board shall have been definitely declared with regard to the succession in this most responsible office; and in the mean time he naturally shrinks from doing aught in his office which may seem to conclude the final action of the Board in the premises.

As to any possible exigencies which may have arisen in consequence of the multiplied engagements of the late Secretary, who, besides his duties as the executive officer of the Smithsonian Institution, was also charged with the direction of the U. S. National Museum, of the Bureau of Ethnology, and of the U. S. Fish Commission, we beg leave to say that certain important questions of future policy, deeply concerning the prosperity of the Institution and the cause of American science, may possibly be thrust upon the Board at this juncture in a way to call for careful consideration, if not for immediate decision.

It is known to us all that Prof. Joseph Henry, the first Secretary and the organizer of the Smithsonian Institution, entertained the settled opinion that its operations "should be mingled as little as possible with those of the Government;" that the funds of the Institution, being specifically devoted by the terms of Smithson's bequest to a prescribed object, should not be diverted to other objects, and that consequently the activities of the Secretary should not be engrossed by other engagements which, from their nature or from the administrative cares incident to their management, might be judged to impair the distinctive singleness and highest efficiency of the Institution in laboring for "the increase and diffusion of knowledge among men." He a'so held that the necessity laid upon the Institution of making annual appeals to Congress for the sdpport and extension of adjuncts not essential to the conduct of its own special operations is a necessity which should be avoided as far as practicable in the interests of a dignified and single-minded administration of the Smithson trust; and hence he thought it desirable that some more definite distinction should be made between the Smithsonian Institution and the National Museum, if on the whole it should be judged best to retain them under a common jurisdiction. His own judgment inclined in favor of their entire separation. In the presence of additional engagements so vast, multiform, and important as those involved in the conduct of the Fish Commission, it is obvious that these opinions of Professor Henry would have gained an added emphasis.

The late Secretary, Professor Baird, while acquiescing in the strict views of Professor Henry with regard to the precise terms of the Smithsonian bequest, and while faithfully working, within the proper sphere of the Smithsonian Institution, on the general lines laid down by his predecessor, did not, it is presumed, entirely share Professor Henry's opinions as to the reflex influence and effect exerted by the adjuncts in question upon the normal function and legitimate fame of the Smithsonian Institution. Endowed with a wonderful capacity for administrative detail, and capable of inspiring his subordinates with enthusiam in their work and with loyalty to their official chief, he doubtless saw in these manifold adjuncts of the Institution only so many auxiliaries to its beneficent design ("the increase and diffusion of knowledge among men"), and therefore only so many additional accessories to its usefulness and glory.

Set as your committee are to execute the will of the Regents and not at all to define the scope or policy of the Institution, it would obviously be impertinent on our part to essay any prejudgments on the questions that may be raised by the existing attitude of the Institution considered in the kind or degree of its relations to the National Museum, to the Bureau of Ethnology, and to the Fish Commission. The former two of these adjuncts are parts and parcel of our jurisdiction, while the latter from its inception was placed under the responsible management of the late Secretary, and is now under the direction of Assistant Secretary Goode.

But while we can not venture on any definitions of policy (all questions of policy having been left by us in abeyance), we may properly recall to the recollection of the Board one great leading principle which has prevailed in the administration of the Institution from its beginning down to the present day; that principle is, that the Secretary is charged with plenary power in his office, and therefore with an entire and undivided responsibility for the right and proper administration of the Smithson trust. That trust gives to him the reason of his official being, and it is conferred by the Regents, without restrictions of their own, because of the confidence reposed in the ability, integrity, and discretion of the Secretary. Hence any change of policy which should require a division of responsibility because of a multiplicity or heterogeneity of operations, would work an entire change in the theory of our administration, would break up the continuity of our history, and might seriously jeopard the efficiency of the Institution by marring its harmony and unity. This harmony and this unity of operations would therefore seem to require the establishment of a permanent and definite line of policy to be pursued by the Institution as far as possible without break and without chasm because of changes occurring in its executive head.

It is obvious that anything like a fundamental revision and reconstitution of the proper work and proper relations of the Institution recurring periodically at the death of each Secretary would be fraught with serious detriment to its usefulness and to its fame. But if the specific nature and at the same time the *ensemble* of its general operations can be maintained, it would seem that those operations may receive any addition or undergo any extension which shall be found compatible with prudent and efficient administration under a single head How far, therefore, the ties which now bind the Institution to the National Museum, to the Bureau of Ethnology, and to certain scientific aspects of the Fish Commission, should be tightened or loosened is a question of expediency to be determined by a careful analysis and a deliberate weighing of all the elements involved in the problem set before us—that is, by considering and judging how far each and all of these adjuncts may be made ancillary to the proper work of the Smithsonian Institution under the conduct of a single responsible executive officer.

It is with these general convictions, and with the view of bringing more definitely before you the subject-matters which would seem to call for deliberation at this extraordinary session, that we venture to submit the following resolutions to your consideration, some of which, it will be seen, are suggested as mere starting points for discussion:

1. *Resolved*, That a committee of three Regents be appointed to draft resolutions expressive of the exalted admiration cherished by the Board for the late Spencer F. Baird, LL. D., our gratitude for the long, faithful, and abundant labors which he performed in the service of this Institution, our reverence for his memory, and our profound sense of the loss which the cause of science has sustained in his lamented death.

2. *Resolved*, That this Board do now proceed at once to the election of a Secretary to fill the vacancy created by the death of Professor Baird, and that the rights, powers, and duties of the Secretary thus elected, as well as his salary and emoluments, shall be the same as those prescribed by the existing regulations.

3. *Resolved*, That the newly appointed Secretary is hereby requested to make report in writing at the coming annual meeting, on any changes which may seem to him desirable in the organization of the Smithsonian Institution considered in its relations to the National Museum, to the Bureau of Ethnology, and to such scientific aspects of the Fish Commission as he may deem germane to the proper theory of the Institution, and which shall be capable of reduction under its wise and efficient administration—that is, to consider and report how far the existing relations between all or any of these adjuncts and the Smithsonian Institution should be increased, altered, diminished, or abolished in order the better to promote the original and organic design of the Institution as established by Congress.

4. *Resolved*, That a committee of three shall be designated by the Chair, to be composed of one Regent appointed from the Senate, one Regent appointed from the House of Representatives, and one Regent appointed from the States, whose duty it shall be to investigate and consider all the questions that may be suggested by the nature or extent of the relations now subsisting between the Smithsonian Institution and any or all of the other objects and adjuncts which are now more or less definitely and completely under its administration, or under the personal administration of its Assistant Secretary; that the said committee, in maturing their views, be invited freely and frankly to acquaint themselves with the opinions and judgments of the Secretary, who, to this end, is hereby requested to communicate to the said committee, in the first instance, any recommendations which he shall submit in pursuance of the preceding resolution; and, finally, that the said committee be instructed to report to the Board at the annual meeting appointed to take place on the 18th of January next, a digest of any additional plans, policies or methods of administration which they shall judge expedient in order to meet any adjustment of relations that shall seem to be required by the best interests of the Institution committed to our charge."

The first resolution in the foregoing series was then taken up for consideration, and on motion of Dr. Gray it was adopted.

Messrs. Gray, Ingalls, and Welling were appointed a committee to draft resolutions in honor of the late Secretary, and that committee, through its chairman, Dr. Gray, reported the following preamble and series of resolutions:

Whereas in the dispensation of Divine Providence, the mortal life of SPENCER FULLERTON BAIRD was ended on the 19th of August last, the Regents of the Smithsonian Institution, now at the earliest practicable moment assembled, desire to express and record their profound sense of the great loss which this Institution has thereby sustained, any which they personally have sustained, and they accordingly resolve—

1. That in the lamented death of Professor Baird the Institution is bereaved of its honored and efficient Secretary, who has faithfully and unremittingly devoted to its service his rare administrative abilities for thirty-seven years; that is, almost from the actual foundation of the

establishment, for the last nine years as its chief executive officer, under whose sagacious management it has greatly prospered and widely extended its usefulness and its renown.

2. That the National Museum, of which this Institution is the administrator, and the Fish Commission, which is practically affiliated to it—both organized and in a just sense created by our late Secretary—are by this bereavement deprived of the invaluable and unpaid services of their indefatigable official head.

3. That the cultivators of science, both in this country and abroad, have to deplore the loss of a veteran and distinguished naturalist, who was from early years a sedulous and successful investigator, whose native gifts and whose experience in systematic biological work served in no small degree to adapt him to the administrative duties which filled the later years of his life, but whose knowledge and whose interest in science widened and deepened as his opportunities for special investigation lessened, and who accordingly used his best endeavors to promote the researches of his fellow naturalists in every part of the world.

4. That his kindly disposition, equable temper, singleness of aim, and unsullied purity of motive, along with his facile mastery of affairs, greatly endeared him to his subordinates, secured to him the confidence and trust of those whose influence he sought for the advancement of the interests he had at heart, and won the high regard and warm affection of those who, like the members of this Board, were officially and intimately associated with him.

5. That without intruding into the domain of private sorrow the Regents of the Institution would respectfully offer to the family of their late Secretary the assurance of their profound sympathy.

6. That the Regents invite the near associate of the late Secretary, Professor Goode, to prepare a memorial of the life and services of Professor Baird for publication in the ensuing annual report of the Institution.

The resolutions were seconded by Dr. Coppée, who made the following remarks:

Mr. Chancellor, I rise to second the resolutions.

As I have been to some extent associated with Professor Baird as Regent since 1874, when I found him here as Assistant Secretary of the Smithsonian Institution, to which post he was appointed in 1850, it may be proper that I should ask your patience while I add a single word to the eloquent tribute of just eulogium offered to his memory in the resolutions of Professor Gray and the committee.

When the distinguished Professor Henry was called to his rest and reward in 1878, amid tokens of grief in yonder Capitol, there was a hearty concurrence of voices in the Board of Regents to appoint Professor Baird to the vacant place. At that time, sir, it seemed, in contradiction of the maxim of the French philosopher, that he was a necessary man. His large scientific scope, his great knowledge and success as a specialist in natural history just when that branch of science needed particular attention to meet its expanding claims, his wonderful industry, his intimate acquaintance with the system and the details of the Institution, his thorough and brotherly sympathy with its scientific workers, and, withal, his great and increasing reputation, formed, in the view of the Regents, the strongest grounds for his appointment. Without making comparisons, he was eminently worthy to succeed our earlier and illustrious scientist and Secretary.

Earnest, courteous, painstaking and exact, he allowed the Institution to suffer no detriment at his hands. It is specially significant of his unremitting care for it, that, last year when he was suffering from nervous prostration, in his eagerness to provide for its future welfare he asked the Board to appoint an assistant, who should aid him in his onerous labors, and who, in the event of his permanent disability or death, should assume the government of the Institution until the Board of Regents could take action.

Sir, the sad necessity came far too soon. It has called us together to-day to mourn his loss, recall his virtues and merits, and fill his vacant place.

The Smithsonian Institution, which had but one Secretary before him, will in the flight of time have many. Let me conclude by expressing my conviction that among them there will not be a more excellent Secretary than he, nor a nobler character than that of Spencer Fullerton Baird.

The resolutions were then unanimously adopted by a rising vote.

The second of the foregoing resolutions was then adopted, and immediately thereupon Senator Morrill renewed the nomination of Professor Langley as Secretary of the Smithsonian Institution.

The motion was seconded by Dr. Welling.

In rising to second the motion, Dr. Welling said that he had it in charge from Professor Langley to make to the Board on his behalf a certain representation which seemed to him (Professor Langley) to be due in order that the pending question might be considered with entire candor and freedom on all sides. Dr. Welling said that it was well understood that Professor Langley had been nominated by the late Secretary as an assistant secretary of the Institution because of the eminent ability he had shown and the distinguished reputation he had already gained as an original investigator in an important branch of physical science. The achievements which Professor Langley had made in astronomical physics were of a nature to shed luster on his name and do high honor to American science. It would be a great loss to the cause of science and a great loss to the best interests of this Institution if the capacity for original research thus demonstrated by Professor Langley should be smothered by the mere drudgery of official cares and administrative details. It might be proper to state that Professor Langley had brought himself to entertain the proposition now pending before the Board only after much misgiving on his own part, and after much earnest remonstrance on the part of the friends who knew him best as a scientific worker, and who feared that in accepting this office, dignified and inviting as it is, he might be making a mistake for the interests of science and for himself by sacrificing even higher duties and foregoing even higher honors than those awaiting him as director of this Institution.

Now however that the question of the succession in the office of the Secretary had been precipitated at an earlier date than we all had expected when he was chosen an assistant secretary, Professor Langley held that it was due to the Board and due to himself that he should frankly state the understanding with which he had finally brought himself to the belief that it was his duty to accept the office of Secretary if it should be conferred upon him by the Board. This understanding was that while, if called to such a responsible trust, he must needs give with all fidelity and with all conscientiousness the full measure of time, thought, and care which shall seem to be required by the Institution and by its adjuncts, he did not construe this obligation as precluding the possibility of sometimes giving to himself that physical rest and mental diversion which should come to every man who is burdened with the discharge of an exacting office. Professor Langley had doubtless observed that the first Secretary of the Institution, Professor Henry, had sought such rest and such diversion in the change of labor brought to him by the chairmanship of the Light-House Board, and in the performance of this function we all knew that Professor Henry had done good work for the cause of science (as witness his researches in sound and in the economies of light-house illuminants), and therefore a work which had redounded to the honor of the Smithsonian Institution.

Professor Langley had also observed, we may presume, that the late Secretary, with the approval of this Board, had engaged in great and useful labors connected with the Fish Commission, and that hence in our judgment there was no incompatibility in the pursuit by our Secretary of certain labors extraneous to the immediate precincts of the Institution, if they could be pursued without detriment to its best efficiency and to the full development of its capacity for usefulness. It was in this view that Professor Langley begged leave to represent that he, too, might sometimes wish to find rest and refreshment in a change of labor from the ordinary routine of official administration in connection with the Institution, and he would naturally look for such rest and refreshment in the further pursuit of his favorite scientific researches, so far, and only so far, as that pursuit could be made consistent with his paramount duty to the Smithsonian Institution.

Dr. Welling then added that, speaking for himself as a member of the Board, he felt free to express the conviction that these "leisure labors" would serve to enhance the title of Professor Langley to the Directorship of an Institution which had for its object "the increase and diffusion of knowledge among men;" and while the statement thus made at the instance of Professor Langley might have seemed to be required by an honorable frankness on his part, the Board would be likely to find in this frankness a further ground of confidence in the high sense of honor and duty which he would bring to the discharge of his responsible office. We might therefore trust with the full assurance of faith that the Institution in his case, as in the case of his distinguished predecessors, would be only the gainer by such intervals of rest as he might seek in the interest of his health, and by such vicissitudes of labor as he might seek in the interest alike of this Institution and of his chosen studies. Such intervals of rest, or at least such variety of labor, were especially necessary to a man who is placed under stress and pressure of heavy administrative cares, like those devolved on the Director of this Institution, and the Board had in the character of Professor Langley the best possible guaranty that he could be freely trusted to decide all such questions of duty according to a delicate and conscientious sense of right.

The Board then proceeded to ballot for the election of Secretary. Ten votes were cast, all of which were found to be for Professor Langley, who was thereupon declared by the Chancellor to be duly elected as Secretary of the Smithsonian Institution.

After some discussion upon the remaining two resolutions in the foregoing series as reported by the executive committee—a discussion participated in by Messrs. Morrill, Welling, Gray, Coppée, and others—the resolutions were withdrawn.

Dr. Welling was appointed to inform Professor Langley of his election, and having done so, he was introduced to the Board, and in a few remarks expressed his acceptance of the office of Secretary with a solemn sense of the responsibility devolved upon him, and high appreciation of the honor which had been conferred.

Dr. Welling offered the following resolution, which was adopted:

Whereas the remains of the late Prof. Spencer F. Baird have not yet been committed to their last resting place; and

Whereas this solemn ceremonial has been postponed at the request of members of this Board and others, that the friends of the late Secretary in Congress might have the opportunity of testifying by their presence at his grave the respect in which they held him while living, and their reverence for his memory now that he is no more: Therefore be it

Resolved, That the Secretary of the Institution, after conference with Mrs. Baird, be requested to issue public notice of the time and place which shall be appointed for these funeral services, and to send a special notice to the members of the Smithsonian Establishment and of the Board of Regents.

On motion of General Meigs it was—

Resolved, That the Secretary be authorized to call the annual meeting of the Board for the present year at the time fixed for the funeral of Professor Baird.

On motion of Dr. Coppée it was—

Resolved, That the Secretary be authorized to purchase the oil portrait of Professor Baird, painted by Henry Ulke, now exhibited to the Regents, at a cost not to exceed $300.

The Board then adjourned to meet at the call of the Secretary.

REPORT OF S. P. LANGLEY,

SECRETARY OF THE SMITHSONIAN INSTITUTION, FOR THE YEAR ENDING JUNE 30, 1889.

To the Board of Regents of the Smithsonian Institution:

GENTLEMEN: I have the honor to present the report upon the operations of the Smithsonian Institution for the year ending June 30, 1889, together with the customary summary of the work performed by the Bureau of Exchanges, the National Museum, and the Bureau of Ethnology.

THE SMITHSONIAN INSTITUTION.

THE BOARD OF REGENTS.

As the Annual Reports of the Secretary are intended to present a history of the affairs of the Institution, it seems proper to state that by the appointment of the Hon. Melville W. Fuller as Chief Justice of the United States, the latter became *ex officio* a regent of the Institution, and that at the annual meeting of the Board of Regents, held on the 9th of January, 1889, he was unanimously elected its chancellor.

The Hon. Levi P. Morton has become a Regent by his election as Vice-President, the holder of that high office being *ex officio* a Regent of the Institution.

The terms of Senator S. M. Cullom, appointed March 23, 1885, and Senator R. L. Gibson, appointed December 10, 1887, having expired on March 3 of the present year, those gentlemen were re-appointed by the President of the Senate.

The Board has lost from its number by death the Hon. S. S. Cox, long connected with the Institution; but this event having occurred since the expiration of the year which forms the subject of this report, the remarks called out by this great loss will be more properly made in a later communication.

FINANCES.

I have in my last report referred to the fact that owing to the changing value of money, the purchasing power of the Smithsonian fund, in the language of a committee of the Regents—

"while nominally fixed, is growing actually less year by year, and of less and less importance in the work it accomplishes with reference to the immense extension of the country since the Government accepted the trust;"

so that it seems most desirable that the fund should be enlarged, if only to represent the original position of its finances relatively to those of the country and institutions of learning, and nothing has occurred in the course of the last year which does not rather increase than diminish the force of such an observation. It is on the Congressional Regents that the Institution must largely depend for making its wants known to Congress, and with reference to the suggestion that the Smithsonian fund should be enlarged by re-contribution from the Government as well as from contributions from private individuals, I desire to repeat the remark of Professor Henry, made in 1872, to the effect that the Government, in equity, should *then* have paid the Institution $300,000 for the use of the present building. This building, erected wholly out of Smithsonian funds, at the cost of over half a million dollars, has, with the exception of a small portion, continued to be used rent free by the Government ever since that time.

I recall briefly in this connection the well known facts that the will of James Smithson was made on October 23, 1826, and that by an act of Congress approved July 1, 1836, the bequest was accepted, while under the act of August 10, 1846, a definite plan of organization was adopted, and that finally, by the act of February 8, 1867, the Regents were authorized to add to the Smithsonian fund such other sum as they might see fit to deposit, not exceeding, with the original bequest, the sum of $1,000,000.

The original bequest and the sums since added are as follows:

Bequest of Smithson, 1846	$515,169.00
Residuary legacy of Smithson, 1867	26,210.63
Deposits from savings of income, etc., 1867	108,620.37
Bequest of James Hamilton, 1874	1,000.00
Bequest of Simeon Habel, 1880	500.00
Deposit from proceeds of sale of bonds, 1881	51,500.00
Total permanent Smithsonian fund in the Treasury of the United States, bearing interest at 6 per cent. per annum	703,000.00

There may, therefore, be added to the fund nearly $300,000, on which the Institution is entitled to receive 6 per cent. under the act of February, 1867, while it has received in bequests only the insignificant sum of $1,500. This is in striking contrast to the liberality which is understood to have endowed more than one American institution of learning within this time with something like ten times the amount of the entire Smithsonian fund. No institution in the country, it is believed, enjoys wider measure of public confidence or is more universally known, and it would seem that some action might well be taken to bring these facts before those who are seeking a trustee for the disposition of means intended for the advancement of knowledge.

In this connection, however, it seems proper to invite the attention of the Regents to the circumstances of the bequest of James Hamilton, who donated $1,000 to the Institution in 1874, the interest on which was to be appropriated biennially for a contribution, paper, or lecture on a scientific or useful subject. Your former Secretary, Prof. Joseph Henry, in his report for 1874, states that—

> "The first installment of interest on the Hamilton bequest has just been received, and will be appropriated in accordance with the will of the testator at the end of next year, and so on continually at the end of every two years."

And he adds—

> "A statement of the manner of spending this income will be given in the accounts of the operations of the institution with due credit to the donor. His name will therefore appear from time to time in the annual reports and thus be kept in perpetual remembrance."

Professor Henry continues, in this connection:

> "When the public shall become more familiar with the manner in which the income of the additional bequests to the Smithsonian fund is expended, with the permanence and security of the investment, and with the means thus afforded of advancing science and of perpetuating the names of the testators, we doubt not that additions to the fund in this way will be made until it reaches the limit prescribed by law of $1,000,000."

Owing, perhaps, to the small amount of this bequest, the intent of the Secretary does not appear to have been fulfilled. No contribution, paper, or lecture seems to have ever been furnished, biennially or otherwise, and with the exception of the exploration of certain bone caves, mentioned in the report of the Secretary for 1876, the income has remained unexpended.

I shall have elsewhere to speak of the great loss the Institution has sustained in the death of Dr. J. H. Kidder, curator of exchanges; but I refer to it here only in connection with a bequest made by him, constituting the Institution one of his residuary legatees. This bequest, the terms of which are still awaiting the consideration of the Regents, will be more properly described, in detail, after their action upon it, which can not well form a portion of the present report.

At the beginning of the fiscal year the balance on hand of the income from the fund was $4,809.23. The interest has been $42,180, while from miscellaneous sources $3,760.53 have been received. The total expenditures have been $38,992.29, leaving on July 1, 1889, $11,757.47, a somewhat larger balance than usual, which has been retained to meet certain delayed expenditures.

The Institution is charged by Congress with the disbursement of sundry appropriations through the Secretary, as follows:

For international exchanges	$15,000
For ethnological researches	40,000
For preservation of collections, National Museum	125,000
For furniture and fixtures, National Museum	40,000
For heating and lighting, National Museum	12,000

The vouchers for the expenditures from the appropriations are passed upon by the executive committee of the Board of Regents with the exception of those for ethnological researches. The disbursements from the latter appropriation are made under the direction of Major Powell.

The estimates prepared to be submitted for the fiscal year ending June 30, 1889, were as follows:

International exchanges	$27,050
Ethnological researches	50,000
Preservation of collections	150,000
Furniture and fixtures	40,000
Heating and lighting	13,000

For which Congress appropriated as follows:

International exchanges	15,000
Ethnological researches	40,000
Preservation of collections	125,000
Furniture and fixtures	40,000
Heating and lighting	13,000

Of the first of these items, that of international exchanges, urgent representations were made to Congress to the effect that though it had assumed the charge of this, the expenditures of the Bureau (whose work largely consists of the transportation of Government documents) continue to be met, in part, from the private fund of the Institution, but, as will be seen, no change in this respect has been made.

The estimates prepared to be submitted for the fiscal year ending June 30, 1890, were as follows:

International exchanges.—Twenty-seven thousand and five hundred dollars was asked for; the House committee reported $15,000; the Senate committee $20,000; and the amount finally appropriated was 15,000.

North American Ethnology.—The appropriation asked for this service was $50,000. The House reported $40,000; the Senate made no change and the amount of the appropriation remained as reported by the House.

Preservation of Collections, U. S. National Museum.—The appropriation asked for this service was $160,000. The House committee reported $135,000; the Senate committee $145,000. The amount finally appropriated was $140,000.

Furniture and Fixtures, U. S. National Museum.—An estimate of $35,000 was submitted. The House committee reported $30,000; the Senate committee also reported $30,000 and this amount was appropriated.

Heating and Lighting, U. S. National Museum.—The appropriation asked for this purpose was $12,000. This amount was agreed to by the House and Senate committees. There is a deficiency of $1,000 for the purchase of coal.

Living Animals, U. S. National Museum.—An estimate of $5,000 was submitted for this service. The House did not report the same.

Postage-Stamps and Foreign Postal-Cards, U. S. National Museum.—An appropriation of $1,000 was asked for this service. The same was reported from the Senate favorably, where it originated, and passed the House.

Publications, U. S. National Museum.—An estimate of $15,000 was submitted for this service. The House reported $10,000; the Senate committee reported $12,000; and in conference the amount as reported by the House was agreed upon.

In my last report I stated that it was desirable that the appropriations for the Museum should be made under the direction of the Institution, and no longer under the Department of the Interior, and I gave a correspondence with the honorable the Secretary of the Interior upon the subject. I am happy to state that the Secretary's assent being given the appropriations were transferred by Congress to the care of the Institution, and are now disbursed under direction of the Regents by a disbursing clerk in the Institution, whose bonds have been accepted by the Treasury Department.

A detailed statement of the expenditures for the fiscal year 1889, under appropriations for International Exchanges, North American Ethnology, and the National Museum is given in the report of the Executive Committee.

BUILDINGS.

It will be remembered that the Board of Regents in their meeting January 17, 1883, recommended to Congress the erection of a new building planned exclusively for museum purposes, and the steps taken in pursuance of their instructions were laid before the Regents in my last report, but I regret now to be unable to report any further progress.

The necessity for additional space for the storage of collections, independent of that demanded for exhibition purposes, is constantly becoming greater, while the assignment by the last Congress to the Fish Commission of the principal parts of the rooms occupied by the Museum in the Armory building has still further aggravated the crowded condition of the Museum exhibition halls and storage rooms, and I deem it my duty again to urge the necessity of the erection of a new building, if only for such requirements of storage as may be inferred from the following statements:

Since the erection of the present Museum building there have been nearly 14,000 accessions to the Museum, chiefly by gifts, such "accessions" representing frequently collections, and the collections including, in many cases, thousands of specimens. From the year 1859 to 1880 the accessions numbered 8,475. It is thus evident that during the last nine years the accessions have exceeded by more than 5,000 those of the previous twenty-one years.

Among the more recent collections are several of very great extent, such as the bequest of the late Isaac Lea, of Philadelphia, which contains 20,000 specimens of shells, besides minerals and other objects; the Jeffries collection of fossil and recent shells of Europe, including 40,000 specimens; the Stearns collection of mollusks, numbering 100,000 specimens; the Riley collection of insects, containing 150,000 specimens; the Catlin collection of Indian paintings, about 500 in number; the collection of the American Institute of Mining Engineers, for the transportation of which to Washington several freight-cars were required; the Shepard collection of meteorites; the Wilson collection of archæological objects, more than 12,000 specimens; the Lorrillard collection of Central American antiquities, and very many others nearly as extensive.

In addition to these are the extensive collections obtained at the close of the exhibition in Berlin, London, and New Orleans, the annually increasing collections transferred to the Museum by the U. S. Geological Survey, the U. S. Fish Commission, and the Bureau of Ethnology, besides numerous contributions resulting from Government expeditions as well as those made by officers of the Army and Navy, and other Government officials.

The storage sheds contain many hundreds of boxes of valuable material which we have not room to unpack, and the great vaults under the Smithsonian building, and many of the attic and tower rooms are similarly crowded.

The growth of several of the most important departments in the Museum is seriously retarded owing to the fact that no exhibition space is available for the collections, and that there is not even storage room where incoming material can be properly cared for.

The collection of birds, which so far as North America is concerned, is the finest in the world, and now numbers nearly 60,000 specimens, is very inadequately shown, and requires double the case room now available.

The collection of mollusks, which is one of the most complete in the world, and contains nearly 470,000 specimens, is at present almost entirely unprovided for.

The collection of insects, now numbering over 600,000 specimens, is so far as North America is concerned, equally perfect, but is practically without any exhibition space.

The same is equally true in regard to the collections of birds' eggs (more than 50,000 specimens), of reptiles (nearly 30,000 specimens), of marine invertebrates (more than 515,000 specimens), of invertebrate fossils (more than 160,000 specimens), and of fossil and recent plants (nearly 50,000 specimens).

Many valuable collections elsewhere than in Washington are at the service of the Museum, but lack of space has compelled us to decline to receive them.

It should be borne in mind that under the roofs of the Smithsonian and the new Museum buildings are grouped together collections which, in London, Paris, or any other of the European capitals, are provided for in different museums, for the accommodation of which a much larger number of equally commodious buildings is found needful.

The necessity for additional space then is constantly becoming greater, and there is the further reason that by the action of the last Congress the Armory building, assigned to the uses of the Museum in 1876, and for several years past occupied in part by the U. S. Fish Commission, as a fish-hatching station, was assigned to this Commission for headquarters. It has been refitted as an office building, and is now almost entirely relinquished by the Museum, four apartments on the third floor being retained for the use of a part of the Museum taxidermists.

From the inadequate exposition of our needs just made, it will be apparent that an extensive additional building is needed, if only for storage, and where purposes of immediate exhibition are not in question.

Irrespective of the construction of this proposed building, however, I beg to urge the necessity of improving the lighting of the second floor of the main hall of the Smithsonian building, and more particularly the indispensability of fire-proofing the west wing, which I have already urged upon the attention of the Regents, and concerning the latter of which, one of their number, Senator Morrill, introduced a bill in the Senate on June 12, 1888, which is referred to in my last report, and on which no further action has been taken by Congress.

In regard to erections of minor importance, it may be mentioned that it is intended to put up a small wooden building of one story, of a temporary character, immediately south of the main building, as a cover for the instruments, which at the same time will render it possible to make certain observations pending the building of the proposed physical observatory, and this is more particularly alluded to under the following head of Research.

RESEARCH.

In my last report I spoke of the preparations made by the late Secretary for securing an astro-physical observatory and laboratory of research, and I mentioned that through his action some friends of the Institution had already offered to give the means for the erection of the simple structure needed for the accommodation of such a special observatory. I added that the site would necessarily be suburban on account of the special need of seclusion and the absence of tremor in the soil.

I have elsewhere referred to the collections of the Institution in connection with the purchase by Congress of a zoological park, which it would appear to have been the first intent of Congress to place under the care of the Regents. It had been my hope in that case to place this observatory somewhere in the park, but in view of the long delay which has already arisen, and of the indefinite further delay which may occur, I have thought it better to put a wooden structure of the simplest and most temporary character in grounds immediately south of the Institution, although this site is quite unsuitable for a permanent building. Such a shelter will probably be erected before the coming winter, and will, while serving as a store-house for the apparatus, enable observations to be commenced.

The promotion of original research has always in the history of the Institution been regarded as one of its most important functions, and the proper object of the personal attention of the Secretary; and I shall be very glad to do something in this direction on the most modest scale, rather than incur the chance of indefinite further delay.

In this connection I desire to say that a valuable collection of recently constructed apparatus, most of it exactly suited to the wants of the proposed laboratory, and which was the property of the late William Thaw, of Pittsburgh, has been, by his wish and the consent of his executors, loaned to the Institution for use in this direction.

Comparatively few of the collections of the Institution or of the Museum have reference to the physical sciences. The apparatus collected by Professor Henry, together with some few archaic instruments illustrating the early history of methods of precision which I have added, are now being placed in the south hall of the main building, and it will gratify me to see this lead to accessions in illustration of the history of research in all branches of science.

EXPLORATIONS.

The Smithsonian Institution has during the year enjoyed the valuable assistance of several persons who have expressed their willingness to prosecute special researches in its behalf, or have generously offered to allow the Museum to share in their results.

In embracing these opportunities it has been the policy of the Institution to endeavor to obtain information and, when possible, to secure specimens, in regard to subjects in which the Museum collections were most deficient, and thus to fill some of the most important gaps in special collections rather than to obtain large collections of miscellaneous material.

Mr. Talcott Williams, of Philadelphia, visited the northern part of Africa early in the present year, and, before going, kindly offered to make special inquiries in regard to the civilization of the modern Arabs and the natural history of the region, and to collect, if possible, linguistic specimens. It was his intention to journey direct to Tangiers, thence to Fez and Mequinez, continuing, if time permitted, as far as Mogador and Morocco. Mr. Williams is familiar with the Arabic language, which will greatly facilitate his investigations in that country. The region has rarely been visited by naturalists, and the Smithsonian Institution will no doubt obtain very important information, and probably also some valuable collections. The special studies to which Mr. Williams intends to devote himself are botany, geology, and archæology. At the time of his arrival the North African flora was in flower, so that his opportunities in the first direction were excellent. The geology of Northern Africa is poorly represented in the National Museum, and characteristic rocks and photographs of feature of physical geology will be very acceptable. The subject of most importance to the Smithsonian Institution, however, is the archæology of this region, and it is to this that Mr. Williams has been requested to chiefly direct his attention. It is his intention to visit El Kutel, one of the most striking monolithic remains in Northern Africa, and other ruins of equal interest. Photographs and measurements will be obtained, for which purpose a photographic outfit has been furnished to Mr. Williams, who is thoroughly competent to conduct investigations of this kind. The Smithsonian Institution has also provided an outfit of instruments for taking observations of temperatures and altitudes, and he has been requested to obtain musical instruments of all kinds, as far as the limited sum of money placed at his disposal from the Museum fund will enable him to purchase them.

News has already been received of Mr. Williams's arrival in Africa. He has secured a complete series of musical instruments, from the rudest whistle to stringed instruments of skillful manufacture. In each instance the native names and names of the parts have been ascertained, the proper pitch of each string taken, and a native melody, as played on each kind of instrument, has been noted in our musical notation. He has also succeeded in obtaining a varied collection of objects illustrating the domestic life of the people.

Mr. W. W. Rockhill, of the German legation of Pekin, has for several years made himself familiar with the customs of the natives of Thibet, and having recently undertaken a journey through that country, will make a special study of the ethnology of the region. He has been supplied by the institution with a barometer and other instruments desired by him for his journey. His previous investigations have resulted in an exceedingly valuable collection of objects illustrating the religious practices, occupations, and amusements of various peoples in different parts of China, Thibet, Turkestan.

Dr. James Grant Bey, who some years ago established a sanitarium in Cairo, Egypt, and attended the International Medical Congress held in Washington in 1887, became much interested in the work of the National Museum, and has since his return to Egypt devoted his leisure time to special studies of the arts of the ancient Egyptians. Several valuable collections have already been received from him.

During the summer, the Bureau of Ethnology decided to send Mr. Jeremiah Curtin to Hoopa Reservation in California for the purpose of studying the languages and mythology of the tribes of Indians inhabiting the reservation. The Smithsonian Institution was fortunately enabled to secure the assistance of Mr. Curtin in investigating their arts and industries also, and a small sum of money was placed in his hands for the purchase of objects of Indian manufacture.

Dr. John M. Crawford, U. S. consul-general at St. Petersburg, has kindly offered to allow the National Museum to participate in the results of his ethnological researches in Russia and Finland. Dr. Crawford is well known in the United States as a philologist and a student of Scandinavian antiquities, and as the author of the English translation of the Finish epic "The Kalevala." His appointment as consul-general at St. Petersburg was made with a special view to enable him to carry on his studies of the traditions and antiquities of the Finish race and related peoples. He has offered to make collections for the National Museum, and in order to facilitate his work, the Smithsonian Institution has provided him with letters of introduction to several of its correspondents in Russia and Finland. These will no doubt be of great service to him in enabling him to carry out the object which he desires to further.

Rev. Frederick H. Post, an Episcopal clergyman of Salem, Oregon, has recently undertaken missionary work in Alaska, and has taken up his residence at Anvik, on the Yukon River. He has entered into correspondence with the Smithsonian Institution, and has offered to collect information relating to the tribes of the Upper Yukon. He has also proposed to make meteorological observations at Anvik. This offer has been referred to the Signal Office. It is probable that an outfit of alcohol, guns, and ammunition will be sent to Mr. Post next year to enable him to collect the mammals and birds of that region.

Lieut. J. F. Moser, commanding the U. S. Coast Survey steamer *Bache* has continued his explorations for the Museum, and has transmitted a collection of fishes, mollusks, insects, and marine invertebrates from the vicinity of Cape Sable, Florida.

Prof. O. P. Jenkins, of De Pauw University, Indiana, has made arrangements to visit the Hawaiian Islands for the purpose of collecting fishes, and has expressed his intention of presenting a duplicate series of specimens to the National Museum. The Smithsonian Institution has supplied him with seines and has furnished him with a letter of introduction to the curator of the national museum in Honolulu.

Ensign W. L. Howard, U. S. Navy, has kindly offered to collect zoological and ethnological material in Alaska, and has been supplied with collecting apparatus and supplies for use in trading with the Indians.

A large outfit of tanks, bottles, and alcohol was supplied to Mr. W. A. Stearns, of Cambridgeport, Mass., for use in collecting specimens of natural history in northern Labrador. No collections have yet been received from him.

PUBLICATIONS.

Under an arrangement made by the late Secretary, Prof. E. D. Cope was engaged at the time of my last report in completing and preparing for publication an investigation upon the Reptilia and Batrachia of North America, which has been in progress, under the direction of the Smithsonian Institution, for more than twenty years. The monograph on the Batrachia, mentioned in my last report as having been received, is now in type, though not yet published, but that on the Reptilia is still delayed. I have positive assurance from Professor Cope that it will be completed within the present year, but the expense entailed in the publication has continued to prove far greater than the late Secretary had anticipated, and I am sorry that the expectation of its completion during the past year has not been fulfilled.

I have referred in my last report to the demand for greater economy in publication, and to the probability that some change would be requisite in the form of the annual reports. It will be remembered that the Smithsonian Institution has three classes of publications:

The Contributions to Knowledge.
The Miscellaneous Collections.
The Annual Reports.

A brief review of the past and present condition of each of these publications may here be made, with special reference to the latter. For details concerning these different classes, and for the matter actually presented under each, reference may be made to the appendix.

Smithsonian Contributions to Knowledge.—The first work of original research published by the Institution was the well-known treatise by Messrs. Squier and Davis, in 1848, on Ancient Monuments of the Mississippi Valley. This was the commencement of the quarto series entitled "Smithsonian Contributions to Knowledge," which now numbers twenty-five volumes. This series is designed to record the results of original research, offering positive additions to human knowledge, either undertaken by agents of the Institution or encouraged by its assistance. In general character these contributions correspond somewhat with the more elaborate transactions of learned societies. From causes briefly adverted to in my last report, original memoirs deemed worthy of a place in this series have been much rarer in later years than in the earlier portion of the Institution's history.

Smithsonian Miscellaneous Collections.—In 1862, a second series of publications was commenced by the Institution, in octavo form, with the Meteorological and Physical Tables of Professor Guyot, under the title of "Smithsonian Miscellaneous Collections." This series embraces papers or treatises of a more practical character than those of the Contributions, including résumés of existing knowledge in special departments, systematic lists or classifications of species in the animal, botanical, or mineral kingdoms of nature, tabular collections of natural constants, scientific bibliographies, and other summaries, of value to the students of physical or biological science. These collections now number thirty-three volumes.

Among the subjects heretofore included in this series have been the proceedings or transactions of several scientific societies of Washington (the Philosophical, the Anthropological, and the Biological), which were organized under the auspices of officers of the Smithsonian Institution. To promote their usefulness the stereotyping of their several published journals was undertaken by the Institution and a large extension of their distribution was thus effected by including their re-issue in the Miscellaneous Collections, of which series they constitute three volumes. These societies having now severally attained a highly successful and self-supporting condition of active membership, it has been thought that this form of patronage might well be withdrawn without detriment to the welfare of the societies and with advantage to the Institution. These publications are accordingly no longer stereotyped by the Institution, or included in its issues.

The Bulletins and Proceedings of the U. S. National Museum, published by an appropriation of Congress, have also been heretofore reprinted by the Institution and this supplementary edition has occupied five volumes of the Miscellaneous Collections. It has been decided in like manner to hereafter omit these publications from the series.

Smithsonian Annual Reports.—A provision of the act of Congress organizing the Smithsonian Institution (Revised Statutes, Title 73, Sec. 5593) requires that "the Board shall submit to Congress at each session thereof a report of the operations, expenditures, and condition of the Institution." These annual reports have been accompanied with a "general appendix," giving summaries of lectures, interesting extracts from the correspondence, and accounts of the results of explorations undertaken by the Institution or aided and promoted by it, as well as of new discoveries in science. In the annual report for 1880 and the following years my lamented predecessor undertook to give a more systematic character to the history of discoveries, by engaging a number of able collaborators in various fields of knowledge, to furnish a general summary or record of scientific progress for the year. Appropriate as the scheme appears, it has not been found to work as satisfactorily as is desirable, and as had been hoped for. It has seldom been possible to collect as complete summaries as were originally contemplated; and the delay of publication deprives the record of much of the freshness and interest it would otherwise possess, while in all these the rapid increase of scientific literature demanded such a corresponding increase in the corps of reporters and such a correlatively increasing expenditure as the fixed Smithsonian fund was growing quite unable to afford. It will be remembered that of this appendix there are distributed through members of Congress as many as 9,000 copies, forming the larger part of the whole edition, and that it is thus incumbent on us to observe that it reaches a large class of readers unable to follow the work of specialists in original memoirs.

After serious consideration it has been finally determined to restrict, if not forego, the scheme of a general annual survey of scientific literature and progress, and to recur in large part to the system of Henry of selecting memoirs of a special interest and permanent value, which have already appeared elsewhere and which are sufficiently untechnical to be readily apprehended by readers fairly representative of the intelligent and educated class among the constituents of the members of Congress, by whom they are chiefly distributed.

If, as I have already suggested, Congress sees fit to make a small appropriation for the editing as well as the publication of this appendix, so as to enable it to include, for instance, information relative to the progress of scientific discovery and its useful application in the United States, such a record would be in keeping with the objects of this Institution, and would maintain for this report the popularity and the educational character just referred to, while promoting industrial interests in the country.

In this connection I beg to repeat the remark that it would be desirable to have the supplementary matter of the report placed under a special clause for the avoidance of all question as to the "necessity and entire relation to the public business" of such information, a question which has arisen by the construction given by the Public Printer to the act of Congress of August 4, 1886.

Publications of the National Museum.—These publications (already referred to as being issued by Government appropriations) comprise two series: First, the "Proceedings of the National Museum," consisting of short essays giving early accounts of recent accessions, or newly ascertained facts in natural history, and promptly issued to secure the earliest diffusion of the information, of which series ten annual volumes have now been issued; and secondly, the "Bulletins of the National Museum," consisting of more elaborate memoirs relative to the collections, such as biological monographs, taxonomic lists, etc., of which series thirty-six numbers have been issued. These bulletins vary greatly in size from pamphlets of fifty pages to works of many hundred pages.

Publications of the Bureau of Ethnology.—The principal publication of this Bureau is the "Annual Report." This series consists of large royal octavo volumes, detailing researches relative to the aborigines of North America, handsomely printed and illustrated with numerous cuts and lithographic plates. The fifth Annual Report has been issued during the year, and the series may be referred to, as at the same time creditable to the Government and as fitted to engage public attention by matter of an interest beyond what is ordinarily found in any Government document.

Distribution of Smithsonian Publications.—It is manifestly impossible for the Institution, with its fixed and limited income, to keep pace in its issues and their distribution with the increase of popular interest in scientific productions. The ordinary edition of 1,500 copies of each of the Smithsonian publications which has been produced from the beginning, cannot be enlarged without seriously impairing the efficiency of the fund for other services; although it would be a great satisfaction to be able to supply more liberally the growing demand for the works as published. The impracticability, however, of furnishing these to all interested in scientific pursuits, has required the adoption of more formal regulations to secure the most judicious application of the available stock of publications. These are presented, first, to those learned societies of the first class which give to the institution in return complete sets of their own publications; secondly, to colleges of the first class furnishing catalogues of their libraries and students, and publications relative to their organization and history; thirdly, to public libraries in this country having 25,000 volumes; fourthly, in some cases to still smaller libraries, especially if no other copies of the Smithsonian publications are given in the same place and a large district would be otherwise unsupplied; lastly, to institutions devoted exclusively to the promotion of particular branches of knowledge, such of its publications are given as relate to their special objects. These rules apply chiefly to distribution in the United States. The number sent to foreign countries, under somewhat different conditions, is about the same as that distributed in this country.

A small number of copies not otherwise disposed of has been usually reserved for sale; although such returns have of course contributed but little toward the cost of production. As an experiment (which had been tried in the early history of the institution), I have placed a small edition of one of our works in the hands of a large publishing house, the well-known firm of MacMillan & Co., of London and New York. The work selected for this purpose is the newly revised "tables of specific gravity for solids and liquids," by Prof. F. W. Clarke, Chemist of the U. S. Geological Survey. This being a valuable work of reference for all practical chemists, as well as for many others, was thought to be a very suitable subject for trial as to its commercial success. An edition of 1,000 copies having been reserved for the regular gratuitous distribution, 500 copies were prepared with the imprint of Messrs. MacMillan & Co. on the title page, to be disposed of as one of their own publications, and by their regular business methods.

Facilities afforded to others.—A few instances of assistance in the direction of printing, etc., granted in special cases, may here be mentioned. The widow of Dr. Asa Gray having about 80 imperfect copies on hand of her husband's "Flora of North America," desired, in order to complete her sets and render them available for sale, a corresponding number of copies of the first part of the second volume. The request was cheerfully complied with, and Messrs. Wilson & Son, of Cambridge, Mass., were authorized by the Regents to print the desired small edition at the expense of the Institution.

Prof. M. W. Harrington, of Ann Arbor, Mich., made application for the use of the stereotype plates of Professor Henry's meteorological essays (included in his published scientific writings), with a view to the publication of a cheap popular edition of this treatise. In the belief that such a republication would be in the interests of science and its wider diffusion, permission to use the plates was readily granted.

A similar request was made by Dr. George H. Horn, of Philadelphia, who, as joint author with the late Dr. John L. Le Conte of a work of 600 pages on the "Classification of the Coleoptera of North America" (published by the Institution in 1883, and now out of print), desired the use of the stereotype plates, from which to print an edition of the book. This request was also favorably entertained, and the privilege sought was conceded.

The Eighth International Congress of Orientalists, appointed to be held at Stockholm and at Christiania, in September, 1889, solicited through its officers the co-operation of the Smithsonian Institution. In furtherance of its laudable aims the Institution undertook to print and distribute in this country 1,000 copies of its circular of announcement and information.

In compliance with the request of Mr. Sylvester Baxter, secretary of the Hemenway Expedition of exploration, the privilege of the Smithsonian exchange system was granted for the distribution of the report of the expedition, giving an account of its researches in the Southwest.

These various allowances are believed to be in the spirit of the Smithsonian foundation, and of its ancient maxim—"Co-operation, rather than rivalry or monopoly."

Storage of the Smithsonian Stereotype Plates.—The stereotype plates of the Smithsonian publications now constitute a very large collection, and as the printing of the works had been done in various cities, as appeared most economical or convenient, a considerable portion of this material had been stored in Boston, and especially in Philadelphia. As the fire-proof renovation of the eastern portion of the Smithsonian building furnished a safe and suitable depository in the basement rooms, these plates have now all been collected within its store-rooms.

THE SMITHSONIAN EXCHANGE SYSTEM.

The international exchange system was established early in the history of the Institution, at first purely as a channel for the interchange of scientific publications and specimens, and therefore as a direct means for "the diffusion of knowledge," a means which has proved to be a great benefit to the scientific institutions of the world, and incidentally to Congress in building up the unequalled collection of works of reference deposited in its Library.

Of late years, however, the Government, having assumed the charge of this system, has made the Institution its agent not only for this scientific distribution but for the much larger distribution of the publications of the United States Government abroad, and also for the receipt and transmission to the Library of Congress of the publications of other countries sent in return. In this twofold service it is now performing an important public duty, for which such inadequate provision is made, that in spite of the efforts for an economical and efficient administration of this department the best interests of the Government as well as those of the Institution are seriously suffering.

In reviewing the past year it is necessary to mention first of all the serious loss in the death of Dr. Jerome H. Kidder, which however has been more fully referred to elsewhere. At the date of his death, which occurred on the 6th of April, 1889, owing to the efficient condition of the division due to the hearty co-operation of all in it with the labors of its lamented chief, the office was free from any parcels whatever, and was ready to close its book accounts completely for the first time.

I regret to record, also, the death on June 17, 1889, of Mr. George Hillier, superintendent of the New York Custom-House. Mr. Hillier had for more than thirty years attended to the transmission of Smithsonian exchange packages, rendering the Institution most valuable and efficient service without compensation. In response to a request made to the Secretary of the Treasury, Mr. Quackenbush, chief entry clerk of the New York Custom-House, has been designated to receive and transmit cases addressed to the Smithsonian Institution in future.

Dr. Kidder was succeeded as curator of exchanges by Mr. William C. Winlock, who was appointed May 15, 1889. The curator's report to the Secretary, containing the usual statistics for the fiscal year, will be found in the appendix.

In order to convey an idea of the present magnitude and character of the exchange transactions it may be stated that during the year, 17,218 packages were mailed to correspondents in the United States and 693 boxes, containing 58,035 packages, were shipped to our agents abroad for distribution to correspondents in nearly every civilized nation of the earth. The total number of packages received was 75,966, of which 34,996, or nearly one-half, were governmental exchanges.* The services of eleven† clerks and packers have been required in handling and accounting for this material and in conducting the extensive correspondence that such a business involves. The societies and individuals upon the exchange list now number 13,130.

The entire expense of "international exchanges" for the fiscal year was $17,152.10.‡ Of this sum $15,000 were appropriated directly by Congress, $1,363.54 were repaid by several of the Government Departments to which appropriations had been granted for payment of freight on publications sent abroad through the Institution, leaving a deficit of $788.56, which was paid from the Smithsonian fund.

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*It should be noted that almost from the very beginning of the exchange system the publications of several of the scientific bureaus of the Government were voluntarily transmitted by the Smithsonian Institution; but it was not officially designated for the service till 1878.

†It is not superfluous to repeat that these are engaged in addition to the proper personnel of the Institution, the services of whose officers are given without charge.

‡The items $2,329.99, under the head of expenditures for exchanges, and $2,189.52 repayments, in the report of the executive committee, include receipts and expenditures made on account of the preceding fiscal year.

With reference to this deficiency let me observe that in the history of the Government's connection with the exchanges three periods may be distinguished. The first was in 1867 and 1868, when, after twenty years of useful work in the interests of knowledge, a new duty was imposed upon the service by acts of Congress* which established for the benefit of the Congressional Library an international exchange of works published by the Government and made the Smithsonian Institution the agency for this exchange. The second was in 1878, when the Institution was distinctly recognized† by the Department of State as the agent of the United States in the exchange of all Government publications (including exchanges for the benefit of Bureau libraries) and *also* in the exchange between learned societies.

The Institution possessed unequalled experience and facilities for such work, and though the new class of books brought to the exchange department was partly foreign to its original object, the propriety of its assuming such a service, if the Government's interest could be promoted by this experience, is evident. It certainly, however, was not to have been anticipated that the Institution should conduct a purely administrative work of the General Government out of its private funds, as it appears to have done for thirteen years, from 1868 to 1881, when the first appropriation of $3,000 was made by Congress.

In the act‡ of March 3, 1881, making this appropriation it appears to have been the intent of Congress to apply the amount indifferently to all exchanges, whether to those which it undertakes for the Library of Congress, to those of Governmental bureaus, or to other literary and scientific objects, thus constituting a third change* in the relations of the Smithsonian to the Government in regard to the Exchange Bureau.

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*Statutes at Large, vol. 14, p. 573, Thirty-ninth Congress, second session, resolution 55. Statutes at Large, vol. 15, pp. 260, 261, Fortieth Congress, second session, resolution 72.

†Letter from Hon. Wm. M. Evarts, Secretary of State, to the Secretary of the Smithsonian Institution. Smithsonian Annual Report for 1881, p. 785.

‡"International exchanges, Smithsonian Institution, 1882: For the expense of exchanging literary and scientific productions with all nations by the Smithsonian Institution, $3,000 (act March 3, 1881)." This was changed in 1883 to the following: "International exchanges, Smithsonian Institution, 1883: For expenses of the international exchanges between the United States and foreign countries, in accordance with the Paris convention of 1877, including salaries and compensation of all necessary employés, $5,000 (sundry civil act August 7, 1882)," and in 1886 it again was changed to "International exchanges, Smithsonian Institution, 1886: For expenses of the system of international exchanges between the United States and foreign countries, under the direction of the Smithsonian Institution, including salaries or compensation of all necessary employés, $10,000 (sundry civil act March 3, 1885)."

An approximate estimate of the cost of the exchange for the Library of Congress from 1868 to 1878, together with the cost of the "Governmental" exchange (the Congressional and Departmental) for 1879 and 1880, shows that about $20,000 were paid from the Smithsonian funds for handling Government property alone. Regarding the whole expense of international exchanges since 1881 as a charge on the Government, the entire amount paid out of the funds of the institution on account of the General Government is somewhat over $50,000, exclusive of office rent and minor expenses.

In the report that I had the honor to submit to the Board of Regents at their last meeting the expenses and needs of the exchange department were dwelt upon at some length, and it was stated that a revised estimate of $27,050 had been submitted through the Secretary of the Treasury for the purpose of meeting the expenses of contemplated improvements in the service during the fiscal year 1888-'89. The amount finally appropriated was $15,000, an increase of only $3,000 over the sum appropriated for the year preceding. As I have already remarked, in spite of efforts for an economical and efficient administration of the department, slow transportation and free ocean freight, this was $2,152.10 less than the service actually cost, and the interests of both the Government and the Institution suffer from the entire inadequacy of the appropriation.

Although all of the Government bureaus that have occasion to transmit their publications through the Institution are not provided with funds available for defraying the cost of the service, it seems to have been the intention of Congress that its specific appropriation for the exchange business should be supplemented by special appropriations to some of the bureaus and departments of the Government, so that the charge of 5 cents per pound weight imposed by the regents in 1878 might be met by them. The average amount annually repaid to the Institution in this way during the past eleven years has been about $1,400, and does not represent all the cost to the Institution which has been made up from its private fund.

It has been repeatedly urged that this procedure, for which sufficient reasons existed at the time of its adoption, may now be discontinued as no longer advantageous or economical.

By the present system the cost of the service is actually larger than appears in the specific appropriations for exchanges, and as the specia appropriations to the different departments vary from year to year, and are often omitted altogether, a burden which can not be accurately foreseen continues to be imposed upon the Smithsonian fund.

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* A convention, at which the United States was represented, was concluded at Brussels March 15, 1886, for establishing a system of international exchanges of the official documents and of the scientific and literary publications of the states adjoining thereto.

In order to effect the change contemplated, that is, to collect in a single item the entire appropriation for international exchanges and at the same time to make allowance for a needed compensation to the ocean steam-ship companies for freight and for like necessary expenses, tending to secure to the United States a return of many times what they now receive from foreign governments, with a prompt delivery, an estimate of $27,500 was submitted for the fiscal year 1889-'90.

It should be premised that only about one-third of the Government's publications are actually received from the office of the Public Printer and elsewhere for transmission abroad, and that while special application on our part might call out the remainder, we can not undertake to do this while only partly paid the actual outlay for the portion we carry already, while a sufficient appropriation to justify the employment of a special exchange agent in Europe, as has been frequently and earnestly recommended by the Librarian of Congress, would bring back in return probably about eight times what we now receive. Accordingly, in the subjoined estimate of what could be done if Congress paid the actual cost of efficient service (the services of the officers of this Institution being given without charge), more packages appear under the new plan than under the old.

Statement of exchanges during the fiscal year ended June 30, 1889, *together with estimates for proposed new departure.*

I. *Amount of exchanges sent abroad.*

	Old plan, 1888-'89.	New plan (estimated).
Congressional	22,673	40,000
Departmental	2,998	30,000
Society and private	32,364	35,000
	58,035	105,000

The receipts from abroad would then probably be more than double.

II. *Time.—Average time in transit to western Europe.*

	Slow freight.		Fast freight.
	Extremes.	Average.	
	Days.	*Days.*	*Days.*
England	47 to 21	37	16
Germany	47 to 30	36	15
France	47 to 24	36	17

This sum of $27,500 asked for would have been divided somewhat as follows:

Salaries		$16,600
Transportation:		
From Washington to sea-board	$2,280	
Ocean freight	5,000	
From point of debarkation to destination	1,750	
		9,030
Boxes		950
Incidentals		920
		27,500

No increase, however, over the amount appropriated for 1887-'88 ($15,500) was granted, and it is probable that the deficiency for the coming year will be at least $2,000.

Recurring now to one of the effects of the insufficient appropriations the writer repeats that there are too many and too great delays in the transit of packages sent by international exchanges. These delays do not occur in the office at Washington, nor in those of the agents of the Institution at London and Leipzig. They are due, broadly speaking, to the fact just stated, that the Institution has not the means to pay for rapid transit on land or sea, and that for what it obtains on the latter it is dependent upon the courtesy of several ocean steam-ship companies, with the natural result that the free freight is often delayed to make room for that which is paid for. A subordinate cause, however, lies in the apathy or indifference, or possible insufficient clerical force, of most of the foreign exchange bureaus.

The employés of the bureau are paid much lower salaries than similar services command in other branches of the public service, and the Government pays no rent for the rooms in which they labor, in which even the office furniture forms a part of the charge on the private funds of this Institution.

The convention between the United States of America, Belgium, Brazil, Italy, Portugal, Serbia, Spain, and Switzerland for the international exchange of official documents and scientific and literary publications, as well as the convention between the same countries (excepting Switzerland) for "the immediate exchange of the official journals, parliamentary annals, and documents," was concluded at Brussels March 15, 1886, ratification advised by the Senate June 18, 1888, ratified by the President July 19, 1888, ratifications exchanged January 14, 1889, and proclaimed January 15, 1889, and since that date formal notification has been received of the adhesion to both conventions of the Government of Uruguay. The full texts of these conventions were given in the Curator's report for last year.

The adhesion of the United States to the first convention involves no new departure in the exchange service from the methods of previous years; but for the fulfillment of the obligations incurred by the second convention—the immediate exchange of official journals—an appropriation of about $2,000 to cover the necessary postage and additional clerical assistance is required, and provision should be made for the prompt delivery to the exchange office of the documents referred to.

This sum of $2,000 was estimated in reply to an inquiry made by the Secretary of State, dated February 12, 1889, as to the ability of the Smithsonian Institution to execute all of the provisions of the two conventions without further legislation by Congress, and the estimate was duly transmitted by the Secretary of State in a letter to the President of the Senate, but no appropriation was made.

As heretofore, the Institution is greatly indebted to the lines of ocean steamers between the United States and other countries, and especial acknowledgment is due to the agencies of the following companies for the continuation of many favors in the free transportation of international exchange packages:

Allan Steam-ship Company (A. Schumacher & Co., agents), Baltimore.
Anchor Steam-ship Line (Henderson & Brother, agents), New York.
Atlas Steam-ship Company (Pim, Forwood & Co., agents), New York.
Bailey, H. B., & Co., New York.
Bixby, Thomas E., & Co., Boston, Mass.
Borland, B. R., New York.
Boulton, Bliss & Dallett, New York.
Cameron, R. W., & Co., New York.
Compagnie Générale Transatlantique (L. de Bébian, agent), New York.
Cunard Royal Mail Steam-ship Line (Vernon H. Brown & Co., agents), New York.
Dennison, Thomas, New York.
Florio Rubattino Line, New York.
Hamburg American Packet Company (Kunhardt & Co., agents), New York.
Inman Steam-ship Company, New York.
Merchants' Line of Steamers, New York.
Muñoz y Espriella, New York.
Murray, Ferris & Co., New York.
Netherlands American Steam Navigation Company (H. Cazaux, agent), New York.
New York and Brazil Steam-ship Company, New York.
New York and Mexico Steam-ship Company, New York.
North German Lloyd (agents, Oelrichs & Co., New York; A. Schumacher & Co., Baltimore).
Pacific Mail Steam-ship Company, New York.
Panama Railroad Company, New York.
Red Star Line (Peter Wright & Sons, agents), Philadelphia and New York.
White Cross Line of Antwerp (Funch, Edye & Co., agents), New York.
Wilson & Asmus, New York.

LIBRARY.

I may best preface what I have to say about the library by a repetition of some introductory remarks in my previous report:

"Chiefly through its exchange system, the Smithsonian had in 1865 accumulated about forty thousand volumes, largely publications of learned societies, containing the record of the actual progress of the world in all that pertains to the mental and physical development of the human family, and affording the means of tracing the history of at least every branch of positive science since the days of revival of letters until the present time.*

"These books, in many cases presents from old European libraries and not to be obtained by purchase, formed even then one of the best collections of the kind in the world.

"The danger incurred from the fire that year, and the fact that the greater portion of these volumes, being unbound and crowded into insufficient space, could not be readily consulted, while the expense to be incurred for this binding, enlarged room, and other purposes connected with their use threatened to grow beyond the means of the Institution, appear to have been the moving causes which determined the Regents to accept an arrangement by which Congress was to place the Smithsonian Library with its own in the Capitol, subject to the right of the Regents to withdraw the books on paying the charges of binding, etc. Owing to the same causes (which have affected the Library of Congress itself) these principal conditions, except as regards their custody in a fire-proof building, have never been fulfilled.

"The books are still deposited chiefly in the Capitol, but though they have now accumulated from 40,000 to fully 250,000 volumes and parts of volumes, and form without doubt the most valuable collection of the kind in existence, they not only remain unbound, but in a far more crowded and inaccessible condition than they were before the transfer. It is hardly necessary to add that these facts are deplored by no one more than by the present efficient Librarian of Congress."

At the last meeting of the Board, the Regents passed the following resolution:

"*Resolved*, That, since the Smithsonian deposit now numbers over 250,000 titles, and is still increasing, at the cost of the Institution, it is, in the opinion of the Regents, desirable that in the new building for the Library of Congress, sufficient provision shall be made for its accommodation and increase in a distinct hall or halls, worthy of the collections, and such as, while recalling to the visitor the name of Smithson, shall provide such facilities for those consulting the volumes as will aid in his large purpose of the diffusion of knowledge among men."

I have brought this resolution of the Regents to the attention of the present Librarian of Congress and to that of the Chief of Engineers, the officer in charge of the new building. I learn from the latter official that, owing to the length of time occupied in the construction, it will probably be from six to eight years before any effect can be given to this resolution; and, in the mean time, with the overcrowded condition of the present quarters of the Library, the chests sent up from the Institution still often continue to lie unopened, so that their contents are inaccessible.

Owing to this overcrowding and, as it is understood, to insufficient clerical aid in the Capitol Library, this noble collection, the product of thirty years' accumulation from the fund of Smithson, is, if not altogether lost to science and learning, at any rate so impaired in its usefulness that it can not be assumed that any series of learned transactions is now complete or that any student can any longer find what he seeks in what was once provided for his aid. I beg to recommend this regrettable state of things to the notice of the Congressional Regents. The present sad condition must, from the nature of the case, grow yearly still worse under the present arrangement; and it seems certain that, by the time the new building is ready for the books, the entire collection will have its value so impaired as to be pecuniarily and otherwise of little value in comparison with the original cost. The only remedy still applicable would seem to lie in providing temporary quarters for the collection under the care of the Librarian of Congress, but outside of the overcrowded quarters in the Capitol.

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* See Smithsonian Report of 1867.

The labor of recording and caring for the accessions to the library has been carried on as during the last fiscal year, with this exception, that, the work being now thoroughly organized, it has been practicable to dispense with the services of one of the three clerks previously employed in this department.

The construction of additional cases in the reading-room has given increased facilities for the display of periodicals, and the number of serials now at the disposal of readers has arisen from 265 (as at the time of my last report) to 432. The reading-room is well used by those classes of readers for whom it was designed.

The most important operation in connection with the library during the year has been the commencement of the work of carrying out the plan for increasing the library by systematic exchanges, which was originated soon after I entered on my duties as Assistant Secretary, at the desire of Secretary Baird.

Realizing that there must be many scientific and technical periodicals of value, especially in branches of science not directly related to the work carried on at the Institution, which were not known in our library, and recognizing the fact that many new publications have come into existence since the last systematic attempt to procure full returns for the publications distributed by the Institution, I addressed circulars to three hundred gentlemen in this country who are noted for their eminence in the different branches of knowledge, desiring them to furnish me with lists of the scientific periodicals which were of value to them in their special fields of investigation.*

In reply to these circulars, 174 voluminous lists were received, and these I caused to be carefully collated. The result of this collation is a list of 3,600 titles, embracing, as it is believed, nearly if not quite all periodical literature of importance in the various branches of knowledge, exclusive of belles-lettres and the art of medicine.

In order, however, that this list should be of any practical service to the Institution, it is first necessary to learn which of these publications the Institution may already possess, either in complete or imperfect files. To ascertain this, each title in the list must be laboriously compared with the records of the library, running back frequently for many years. Again, should a learned society, publishing transactions, or the publishers of a journal mentioned in this list, be found to have received Smithsonian publications without making any adequate return, the records of the distribution of publications must be searched, in order to find the exact amount of publications furnished, that upon this the Institution may base its demand for a return.

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* Copies of these circulars are to be found in Appendix 4 to my report for 1887-'88

It will be seen that the publications in question fall naturally into four classes.

(1) Journals which receive the Smithsonian publications, and which are not to be found in the library of the Institution.

(2) Journals which receive the Smithsonian publications, but which make either no return or an inadequate return for these.

(3) Journals which regularly exchange with the Institution, but of which the files in the library are for any reason defective.

(4) Journals which regularly exchange with the Institution, and of which the library possesses a complete file.

When each of the 3,600 titles has been assigned to its proper place in one of these four classes, a letter must be written to each one of the journals belonging to the first three classes, as follows: To the first class, offering to exchange; to the second, calling attention to the fact that the Institution has received no adequate returns for its favors, and to the third, asking for the volumes or parts of volumes required to complete the files.

It will thus be evident that a work of no small magnitude remained to be performed after the list of journals was prepared. A careful estimate showed that it would require the entire time of a competent clerk for at least twelve months to perform the necessary routine work. As, however, the Institution was not in a position to employ an extra clerk for work which would be so largely for the benefit of the Library of Congress, the matter was allowed to rest here.

The desirability of the plan, however, commended itself so strongly to me that I could not willingly see it given up and the large amount of labor already expended remain unfruitful. Accordingly, towards the latter part of the past fiscal year, I presented the matter to Mr. A. R. Spofford, the Librarian of Congress, who, recognizing the advantages that would accrue to that Library from carrying out the plan, consented to defray the expense of the necessary clerical work from his own appropriations. The work was accordingly begun on June 1, 1889, and will be carried on continuously under the immediate supervision of the librarian, Mr. John Murdoch.

It is estimated that of the 3,600 titles under consideration, at least one-half, or 1,800, will prove to be new and desirable accessions to the library, while the work done in endeavoring to complete broken series must prove to be of great value.

The following is a statement of the books, maps, and charts received by the Smithsonian Institution from July 1, 1888, to June 30, 1889:

Volumes:		
Octavo or smaller	1,002	
Quarto or larger	498	
		1,500
Parts of volumes:		
Octavo or smaller	5,556	
Quarto or larger	6,646	
		12,202
Pamphlets:		
Octavo or smaller	2,705	
Quarto or larger	473	
		3,178
Maps		474
Total		17,354

Of these accessions 4,810 (namely, 441 volumes, 3,752 parts of volumes, and 617 pamphlets, were retained for use in the Museum library, and 521 medical dissertations were deposited in the library of the Surgeon-General's Office, U. S. Army; the remainder was promptly sent to the Library of Congress on the Monday following their receipt.

The following universities have sent complete sets of all their academic publications for the year, including the inaugural dissertations delivered by the students on graduation: Bern, Bonn, Dorpat, Erlangen, Freiburg-im-Breisgau, Giessen, Göttingen, Halle-an-der-Saale, Heidelberg, Helsingfors, Jena, Kiel, Königsberg, Leipzig, Louvain, Lund, Tübingen, Utrecht, and Würzburg.

A list of the important accessions will be found in the Appendix (Report of the Librarian).

THE DEPARTMENT OF LIVING ANIMALS.

The collection of the department of living animals has increased during the year (almost wholly by donations) to such an extent as to quite overcrowd its accommodations, and render it necessary to resolutely check its growth, while the degree of interest manifested in this small display has been surprising. This has been shown not only by the residents of Washington, and visitors to the city, who form the daily crowd of visitors, but many residents of remote States and Territories have testified their interest by sending valuable gifts to the collection.

Besides these, many valuable gifts of quadrupeds and birds have been received from United States Army officers in Texas. A most valuable donation received during the year came from the Hon. W. F. Cody (Buffalo Bill), of North Platte, Nebr., and consisted of three fine American elks, two males and a female.

Dr. V. T. McGillycuddy, of Rapid City, Dak., offered to deposit in the collection four American bisons which have been in his possession for several years. The conditions of the offer were considered sufficiently liberal to justify its acceptance, and accordingly Mr. George H. Hedley, of Medina, N. Y., was requested to proceed to Rapid City, where he received the animals and arrived in Washington with them in good condition. Being fine specimens they have naturally attracted much attention.

The overcrowded condition of the temporary cages and yards containing the larger animals has caused extreme trouble, not only to provide properly for the shelter and comfort of the specimens, but to keep them from either killing or injuring each other. Only with larger space and better facilities will it be possible to so care for these animals, and many others like them, that they will not only be a stock from which to replenish their races, so rapidly vanishing from the continent, but a source of constant instruction and recreation for the people.

The department of living animals has served an important purpose in aiding to bring about the establishment by Congress of a National Zoological Park, for the public interest manifested in the collection, forcibly emphasized the general desire and need for such an institution founded on a liberal scale. During the period when the Zoological Park proposition was before the Fiftieth Congress, the Secretary considered that the curator of this department, Mr. Hornaday, could not render more important service than by explaining to Members the details of the plan proposed, and he was accordingly directed to devote a portion of his time to that duty.

The actual accommodations provided for the living animals are necessarily of the most temporary character, and do not in the slightest degree indicate the proper construction of permanent improvements of this kind in a first-class zoological garden. At present a large number of living quadrupeds, birds, and reptiles are crowded together in one small and ill-ventilated building heated by steam, which, during exhibition hours, is usually filled with visitors to an uncomfortable extent. It will be a great boon to the public and to the animals composing the collection as well, when the latter can be transferred to the Zoological Park and provided with suitable accommodations. Under the circumstances it is very desirable that this should be accomplished at the earliest date possible.

The total number of living specimens received during the year was 271, of which 126 were gifts, 37 were deposited, and 8 purchased. The final catalogue entry on June 30, 1889, was 341, which represents the total number of specimens received since the collection was begun. In spite of the disadvantages the curator and his two assistants have labored under in the care of this collection, it is gratifying to be able to report that during the year the losses by death have been almost wholly confined to the small and least valuable animals; and, with the exception of an antelope which was presented by Senator Stanford and died before it had time to recover from the effects of its long journey, all the large and most valuable specimens are alive and in good health.

It is well to direct attention to the fact that Congress has as yet made no special appropriation for the care of these animals, which, with their food, represents a considerable sum, ill spared from the limited appropriation at the disposal of the Secretary for the increase and preservation of the collections, on which so many other pressing demands are made.

ZOOLOGICAL PARK.

In my previous report I stated that a bill had been introduced by Senator Beck to create, under the care of the Regents of the Smithsonian Institution, a zoological garden on Rock Creek, where these animals might not only form the subject of study, but be expected to increase as they do not in ordinary captivity; and I gave the amendment to the sundry civil appropriation bill, reported by Senator Morrill, which was substantially the same as the bill of Senator Beck.

For reasons which may be found in my letter to the chairman of the Committee on Public Buildings and Grounds, quoted later, I gave much time and labor in the interests of this measure, at first without success, the House Committee on Appropriations having reported its non-concurrence in the Zoological Park amendment, and, after a long debate, which occupied the attention of the House through a considerable portion of the 12th of September, 1888, the motion to concur was defeated. In the subsequent conference on the sundry civil bill, the Senate conferees agreed that the amendment should be stricken out, so that the bill was lost.

In pursuance of what seemed to me a public duty, I did not accept this defeat of the bill as final, but brought the matter again before the attention of Congress.

On the 18th of January, 1889, at the request of the Hon. S. Dibble, I addressed a letter to him as chairman of the Committee on Public Buildings and Grounds, to which had been referred a bill of the House, introduced by the Hon. W. C. P. Breckinridge, of similar purport to that introduced in the Senate. This letter the committee made the basis of its recommendation for the passage of the bill in the following words:

REPORT to accompany bill H. R. 11810.

The Committee on Public Buildings and Grounds, to which was referred the bill (H. R. 11810) "for the establishment of a Zoological Park in the District of Columbia," having had the same under consideration, respectfully submits the following report:

Appended hereto is a letter of Prof. S. P. Langley, Secretary of the Smithsonian Institution, portraying the necessity of such a park and the advantages to be derived from its establishment; and, for reasons therein set forth, your committee respectfully recommends the passage of the bill.

SMITHSONIAN INSTITUTION,
Washington, D. C., January 18, 1889.

MY DEAR SIR: I write what follows in accordance with the suggestion of your yesterday's letter, intending it for your consideration and that of the committee.

From all parts of the country, for many years, presents of live animals have been made to the Government through the Smithsonian Institution or the Museum; but the absence of any appropriation for their care has led to their being sent away (though most reluctantly) to increase the collections of the zoological parks in Philadelphia, New York, London, and other cities. It should be better known than it is that everywhere through the country there is a disposition on the part of private individuals to give to the Government in this way, and without any expectation of return, remarkable specimens, which the donor (very commonly a poor man) sometimes refuses advantageous pecuniary offers for, and it seems hard to decline gifts made in such a spirit, or, accepting them, to give them away again.

But little over a year ago I gave instructions that these live specimens should be retained temporarily, as an experiment, and although a very few have been purchased, the collection, which is a subject of so much local popular interest, has been thus formed, substantially by gift, within perhaps fifteen months, and this though many proffers have been declined for want of means to care for them. I am persuaded that, if it were generally known that the Government would receive and care for such gifts, within a very few years the finest collection of American animals in the world might be made here in this way, with comparatively no expenditure for purchase.

Among the many interested in the incipient collection was Senator Beck, whose bill for the formation of a zoological park was brought before the Senate on April 23, 1888. The writer directed the Senator's attention to the fact that a piece of ground singularly suitable, by the variety of its features, to the provision for the wants of all the different kinds of animals, existed in the picturesque valley of Rock Creek in the part nearest to the city. Here not only the wild goat, the mountain sheep and their congeners would find the rocky cliffs which are their natural home, but the beavers brooks in which to build their dams; the buffalo places of seclusion in which to breed and replenish their dying race; aquatic birds and beasts their natural home, and in general all animals would be provided for on a site almost incomparably better than any now used for this purpose in any other capital in the world.

With this is the pre-eminently important consideration that the immediate neighborhood to the city would make it accessible not only to the rich, but to the poor, and therefore a place of recreation to the great mass of the residents, as well as to the hundreds of thousands of citizens from all parts of the country who now annually visit the capital.

It may be added that, so far as is known to the writer, all those interested in the desirable but larger plan for a public park along the whole Rock Creek region—that is to say, all those acquainted with the beauties and advantages of the site—regard the establishment of the proposed zoological park there with favor. It is very difficult for any one who has not visited the region to understand its singularly attractive character, due to the good fortune which has preserved its picturesque features intact until now, although the growing city is sweeping around and enveloping it.

The Smithsonian Institution has not customarily received with favor the propositions continually made it to place different local or national interests under its charge, but the very special reasons which seem in this case to enable it to at once secure a home and city of refuge for the vanishing races of the continent, and a place for the health and recreation of the inhabitants of the city, and citizens of the United States, together with an opportunity for the carrying out an enterprise of national scientific value, and the formation of what, as regards its site, at least, is the finest zoological garden in existence—all these considerations have moved it to see in this an opportunity to carry out its legitimate work, "the increase and diffusion of knowledge among men."

When, therefore, Senator Beck made the understanding that the Smithsonian Institution would accept the charge of such a park, the primary condition on which he would undertake to recommend it to Congress, the Secretary felt authorized to say that he believed it propable that the proposition would be favorably viewed by the regents, and, the matter once brought before Congress, he has not disguised his own interest in the success of the measure.

The bill, brought in by Mr. Breckinridge in the House (and by Senator Morrill in the Senate), appropriates $200,000 for the purchase of not less than 100 acres of land. The land actually most desired for the zoological park covers about 120 acres, being precisely that portion of the Rock Creek Valley which will be soonest destroyed, as regards its picturesque and attractive features, by the laying out of streets and lots. Nevertheless, and largely owing to the very fact that the picturesqueness of the locality implies the existence of rocks, precipices, and valleys, which it would cost much to level and fill in, this land can still be obtained at rates which, considering its neighborhood to the city, are remarkably cheap. The most thorough examination that I have been able to make, the testimony of various real-estate experts and others, have satisfied me that the purchase may and will be completed for somewhat less than the sum named in the appropriation, even leaving a small margin for the erection of a preliminary shelter for the animals.

I beg most respectfully to urge upon the attention of the committee the fact that it is at once the strength and weakness of this measure that, so far as is known, it is an entirely disinterested one, the real-estate holders in the vicinity being generally indifferent or opposed to it, for reasons which can be explained, if desired, and that it is being thus pressed upon Congress by those who have the measure at heart, because anything that is done must be done soon. It is probable that within a year or two more, the good fortune which has kept this singularly interesting spot intact, while the growing city is encircling it, will protect it no longer. It is not the mere space on the map which is to be secured, but natural advantages which have no relation to the number of acres, and which can not be restored if once destroyed, since it is not in the power of Congress itself by any expenditure of money to recreate a rock or a tree.

I am, very respectfully, yours,

S. P. LANGLEY,
Secretary.

Hon. SAMUEL DIBBLE,
House of Representatives.

It appears, however, that this recommendation could not be brought to the consideration of Congress in season for action, and at nearly the same time Senator Edmunds introduced an amendment to the District bill. There were at this time two measures being pressed upon the attention of Congress, one for the creation of a national park, including a thousand or more acres upon Rock Creek, extending far beyond the limits of the proposed zoological park, and requiring a large expenditure not for buildings but for lands, a measure with which the

Smithsonian Institution was not concerned; the other a much more limited scheme for the zoological park, which latter it was understood in Congress was to be placed under the Smithsonian Institution.

Under these circumstances the honorable Mr. Edmunds introduced an amendment to the District of Columbia bill, as follows:

AMENDMENT intended to be proposed by Mr. Edmunds to the bill (H. R. 11651) making appropriations to provide for the expenses of the government of the District of Columbia for the fiscal year ending June thirtieth, eighteen hundred and ninety, and for other purposes, viz: Insert the following:

"For the establishment of a zoological park in the District of Columbia, two hundred thousand dollars, to be expended under and in accordance with the provisions following, that is to say:

"That, in order to establish a zoological park in the District of Columbia, for the advancement of science and the instruction and recreation of the people, a commission shall be constituted, composed of three persons, namely: The Secretary of the Interior, the president of the board of Commissioners of the District of Columbia, and the Secretary of the Smithsonian Institution, which shall be known and designated as the commission for the establishment of a zoological park.

"That the said commission is hereby authorized and directed to make an inspection of the country along Rock Creek, between Massachusetts avenue extended and where said creek is crossed by the road leading west from Brightwood crosses said creek, and to select from that district of country such a tract of land, of not less than one hundred acres, which shall include a section of the creek, as said commission shall deem to be suitable and appropriate for a zoological park.

"That the said commission shall cause to be made a careful map of said zoological park, showing the location, quantity, and character of each parcel of private property to be taken for such purpose, with the names of the respective owners inscribed thereon, and the said map shall be filed and recorded in the public records of the District of Columbia; and from and after that date the several tracts and parcels of land embraced in such zoological park shall be held as condemned for public uses, subject to the payment of just compensation, to be determined by the said commission and approved by the President of the United States, provided that such compensation be accepted by the owner or owners of the several parcels of land.

"That if the said commission shall be unable to purchase any portion of the land so selected and condemned within thirty days after such condemnation, by agreement with the respective owners, at the price approved by the President of the United States, it shall, at the expiration of such period of thirty days, make application to the supreme court of the District of Columbia, by petition, at a general or special term, for an assessment of the value of such land, and said petition shall contain a particular description of the property selected and condemned, with the name of the owner or owners thereof, and his, her, or their residences, as far as the same can be ascertained, together with a copy of the recorded map of the park; and the said court is hereby authorized and required, upon such application, without delay, to notify the owners and occupants of the land and to ascertain and assess the value of the land so selected and condemned by appointing three commissioners to appraise the value or values thereof, and to return the appraisement to the court; and when the values of such land are thus ascertained, and the President shall deem the same reasonable, said values shall be paid to the owner or owners, and the United States shall be deemed to have a valid title to said lands.

"That the said commission is hereby authorized to call upon the Superintendent of the Coast and Geodetic Survey or the Director of the Geological Survey to make such surveys as may be necessary to carry into effect the provisions of this section; and the said officers are hereby authorized and required to make such surveys under the direction of said commission."

The amendment of Senator Edmunds was understood to be offered in a spirit entirely friendly to the interests of this Institution, but it differs from that reported from the Committee on Public Buildings and Grounds, in omitting the name of the Regents, in placing the appropriation under those for the District, in removing from the Commission the power to lay out the land, and in extending the limits within which they had choice, to the military road, in this, as in other respects, resembling the limits of the larger scheme of the national park, as generally proposed. On the 28th of February the Edmunds amendment passed substantially as above given, and by the President's approval of the District bill, became a law on March 2.*

In view of the fact that the zoological park will probably in any case be the ultimate place of deposit for the living collections now under the charge of the Regents, and that their secretary is named as one of the commissioners for effecting the purchase, it seems proper to add a brief statement of the work done by the commission, which, after personally and carefully inspecting the whole course of the stream from Massachusetts avenue to Military road, about 4 miles above the city, found no district so desirable for the single purpose of a zoological park as that lying between Woodley Lane and Klingle Bridge, and designated in the original bill of Senator Morrill; and the commissioners have proceeded to condemn a tract of 166 acres of the remarkably varied and picturesque country whose character is described in the secretary's letter to the chairman of the Committee on Public Buildings and Grounds already cited.

The condemnation is not complete without the President's approval, which had not been given at the date of the completion of the fiscal year, but I may be allowed to so far anticipate a statement properly belonging to a later report as to say that this approval has since been given, and that the land will almost undoubtedly become the property of the Government. The commission has no power to lay out the land, and has no instruction from Congress as to its ultimate destination, owing, it may well be supposed, to the general supposition in the House that the bill as voted contained a clause placing it under the care of the Smithsonian Institution.

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*Extracts from the Congressional Record. Mr. Breckinridge, of Kentucky, states, "I append the report of the Committee on Public Buildings and Grounds that the record may show the exact object in view. There is absolute protection from jobbery in the fact that this is to be under the supervision of the Smithsonian Institution." Mr. Dibble says in the same debate, "We are proud of the Smithsonian, and the Smithsonian has already, by gift, not purchase, the nucleus of a collection, and I am informed by the Secretary of the Smithsonian that this place furnishes the right kind of location for the propagation and perpetuation of these rapidly disappearing species of American animals, while at the same time it will serve the purposes of a public park." Mr. Dibble continued, "I am informed that the inquiries, estimates, and offers indicate that the 120 acres which is included in the design now in front of the reporter's desk [referring to a large map showing that part of the creek between Woodley Lane and Klingle road, which the Morrill bill placed under the care of the Regents] can be purchased for something less than $200,000, etc."

MISCELLANEOUS.

The Statue to Professor Baird.—In recognition of the distinguished services of the late Professor Baird, a bill was introduced in the Senate of the United States, and passed by that body February 10, 1888, making an appropriation for the erection of a bronze statue to commemorate his merits. This bill was referred, in the House of Representatives, to the Committee on the Library, but was not reported. It is hoped that this important subject will, during the coming session, receive the attention which it merits. An appropriation of $25,000 was made by Congress for the benefit of the widow of the late Secretary, whose life had been so unselfishly devoted to the service of the nation.

Art Collections.—I alluded in a previous report to the fact that a very valuable collection of art objects had been promised to the Smithsonian Institution. The intending donor is understood to contemplate the transfer of the collection at no very remote period, the principal condition being that the Institution shall provide a suitable fire-proof building for it.

Upon the representations of the agent of the Institution in Europe, as to the value of the collection and as to the desire of its owner to see your Secretary in order to arrange for the formal transfer, the writer made a brief visit to France last July, for the purpose of such conference and arrangement, but illness on the owner's part has delayed action, so that the Secretary is not able, as he had hoped to be, to lay the matter more fully before the Regents at their present meeting.

Assignment of rooms for scientific work.—During the past year the use of rooms in the Smithsonian building has been continued to the Coast and Geodetic Survey for pendulum experiments, and a room has been assigned to the use of the Zoological Park Commission.

Toner lecture fund.—The Secretary of the Institution is *ex officio* chairman of the board of trustees. The fund, consisting partly of Washington real estate and partly of Government bonds, has an estimated value of about $3,000. A lecture was delivered on May 29, in the hall of the Museum, by Dr. Harrison Allen, of Philadelphia, on "A Clinical Study of the Skull," the first delivered under this fund for several years.

Grants and subscriptions.—In accordance with the precedents established by your first Secretary for encouraging meritorious scientific enterprises, undertaken without view to pecuniary gain, a subscription of twenty copies of the Astronomical Journal, edited by Dr. B. A. Gould, has been continued.

Privilege of the floor of the House of Representatives.—Owing to the lamented death of the Hon. S. S. Cox, no further action appears to have been taken by the House in reference to a bill introduced by him to confer the privilege of the floor on the Secretary of the Smithsonian Institution.

Smithsonian grounds.—At the request of the Director of the Geological Survey, permission was granted to place stones for a base line 300 feet on B street, south, to be used as a standard of comparison for tape lines.

American Historical Association.—Reference was made in the last report to a bill introduced in the Senate to incorporate the Historical Association and to connect it with the Smithsonian Institution. Congress has since passed the act organizing the association.

Stereotyping.—All the stereotype plates belonging to the Institution have been brought from Philadelphia to Washington and stored in the basement of the building.

I have elsewhere alluded to the fact that the practice of stereotyping the bulletins and proceedings of the Washington scientific societies has been discontinued.

Temporary shed.—I have also elsewhere alluded to the purpose of putting up in the Smithsonian grounds a temporary shelter for instruments and apparatus, which may at the same time permit of some astrophysical observations being made. This, however, is only a temporary expedient, and if the Regents ever sanction the erection of an observatory for this purpose it will be necessary to place it in some very quiet locality far removed from all tremor. Such a locality exists in the new zoological park, but while the action of Congress in regard to the purchase of the latter was still uncertain I addressed a letter to the honorable the Secretary of War, asking permission in case it were found desirable to occupy a vacant tract of land in the southern portion of the cemetery at Arlington for this purpose. His assent was given in the following letter:

WAR DEPARTMENT,
Washington City, January 9, 1889.

SIR: I have the honor to acknowledge the receipt of your letter of the 18th ultimo, requesting that the Smithsonian Institution be authorized to occupy a site in the Arlington national cemetery, as indicated in a memorandum and plat inclosed by you, for the purposes of an astro-physical laboratory.

In reply I beg to advise you that there is no objection to the occupation, in the manner stated, of a piece of ground not exceeding 2 acres, indicated on a plat which may be examined in the office of the Quartermaster-General, provided that the ground in question be vacated whenever it is required by this Department.

Very respectfully,

WM. C. ENDICOTT,
Secretary of War.

Prof. S. P. LANGLEY,
Secretary Smithsonian Institution.

The plat in question shows the location of the lot near the center and highest part of the unoccupied wooded ridge, near the colored soldiers' portion of the cemetery. The site, however, is so distant that I should not propose to occupy it while any better could be procured.

Reception.—I have alluded in my previous report to the habit of the first Secretary of giving receptions from time to time in the rooms of the Institution and to the fact that though these rooms are now devoted to official purposes, the writer, desiring to maintain the traditions of this hospitality, had used them once for a similar purpose. He has again employed them in this year on the 18th of April for a reception where it was sought to unite the old and new friends of the Institution.

Correspondence.—The Institution receives annually inquiries from all parts of the country for information on topics often most incongruous, but usually connected with science, which are submitted to the Secretary. None of these inquiries is left unanswered, and the burden of this correspondence is very considerable. It has always been regarded, however, as incumbent on the Institution to reply to them as a part of its function in the distribution of knowledge, and a good deal of labor which does not appear, continues to be devoted to this end.

U. S. NATIONAL MUSEUM.

The main features of the work of the National Museum are briefly referred to in this place. They are fully described elsewhere, in the separate volume forming the report of Dr. Goode, Assistant Secretary in charge of the Museum, and the Curators of its several departments.

Classified service of the Museum.—In response to a resolution of the Senate asking for a "schedule of the classified service of the officers and employés of the National Museum," a letter was addressed by me on March 2 to Hon. John J. Ingalls, President *pro tempore* of the Senate, transmitting a schedule which upon very careful deliberation represents the actual necessities of the service.

This schedule and the letter of transmittal were printed as miscellaneous document No. 92, Fiftieth Congress, second session, and are here re-printed;

LETTER of the Secretary of the Smithsonian Institution in reference to Senate resolution of October 8, 1888, asking for "a schedule of the classified service of the officers and employés of the National Museum."

SMITHSONIAN INSTITUTION,
March 2, 1889.

SIR: In response to the Senate resolution asking for "a schedule of the classified service of the officers and employés of the National Museum," I have the honor to transmit the accompanying schedule, which represents the present actual necessities of the service.

The service for the fiscal year of 1887-'88 was reported upon in a letter to the Speaker of the House of Representatives, dated December 1, 1888 (H. R. Mis. Doc. No. 55, Fiftieth Congress, second session).

In this the aggregate expenditures for service were shown to have been $122,750.47, of which sum $97,493.32 was paid from the appropriation for preservation of collections, $19.203.79 from that for furniture and fixtures, and $6,053.36 from that for heating, lighting, and electrical and telephonic service.

A schedule of the number of persons employed in the various departments of the Museum was also given in this letter (pages 4, 9, 11). This schedule should, however, be regarded only as an approximate one, since many of the employés were actually engaged only a part of the year, and others were temporarily transferred to the pay-rolls of the Cincinnati Exhibition and were engaged in special work in connection with that exhibition.

It is estimated that the aggregate expenditures for services for the present fiscal year (1888-'89) will be $129,710, of which amount $103,000 will be paid from the appropriation for preservation of collections, $20,000 from that for furniture and fixtures, and $5,710 from that for heating, lighting, and electrical and telephone service.

In the schedule herewith transmitted it is shown that for the proper working of the Museum the amount required for services would be as follows:

For salaries of scientific assistants	$56,300.00
For clerical forces	36,920.00
For services in preparing, mounting, and installing the collections	22,060.00
For services in policing, caring for, and cleaning the buildings	36,740.00
For services in repairing buildings, cases, and objects in the collections	14,163.50
For salaries and wages in designing, making, and inspecting cases and other appliances for the exhibition and safe-keeping of the collections	18,337.50
For services in connection with the heating, lighting, and electrical and telephonic service	6,620.00
For services of miscellaneous employés, including draughtsmen, messengers, etc	7,980.00
Total	199,121.00

The increase in the total expenditure, as indicated, is due partly to the addition of a number of officers to the scientific staff, and also to the necessity for a few additional clerks, and a considerable number of watchmen, laborers, cleaners, and messengers, whose services are essential to the safety of the collections, as well as to provide for the cleanliness and proper care of the buildings and for the comfort of visitors.

The rates of pay indicated are in most cases considerably lower than are customarily allowed for a similar service in the Executive Departments.

In the schedule now presented, expenditure for services only is taken into consideration.

No attempt has been made to present the needs of the Museum in regard to the purchase or collecting of specimens, the purchase of general supplies, preservatives, materials for mounting and installing collections, books, exhibition cases, furniture, fuel, and gas, the maintenance of the heating and lighting appliances, freight and cartage, travelling expenses of collectors and agents, etc.

For these various purposes the expenditure in the last fiscal year amounted to $45,249.53, and that for the present fiscal year will, it is estimated, amount to about $48,000, a sum very inadequate to the needs of the service.

It does not include the expenditures for printing the labels and blanks and proceedings and bulletins of the Museum, for which the appropriation for many years past has been $10,000, and for which I have asked $15,000 for the coming fiscal year.

I must not omit to call your attention to the fact that, owing to the peculiar constitution of the Museum as a scientific establishment, it has hitherto been possible to secure a special economy, owing to the fact that its officers and employés are not scheduled as in the Executive Departments.

In thus presenting, in obedience to the request of the Senate, a schedule of a durable organization of the service, I wish to remark, emphatically, that there are pressing needs in other directions—needs that merit the serious consideration of Congress, in order that the National Museum may be enabled to maintain a satisfactory position in comparison with those of European nations.

I have the honor to be, your most obedient servant,

S. P. LANGLEY,
Secretary.

Hon. JOHN J. INGALLS,
President, pro tempore, of the Senate.

Schedule of the classified service of the officers and employés of the United States National Museum, arranged according to duty and salary, as required for the proper working of the Museum.

Designation.	Compensation.
Scientific staff.	
Secretary Smithsonian Institution, director *ex officio*	....
Assistant secretary Smithsonian Institution, in charge of National Museum	$4,000.00
Curator and executive officer	3,000.00
Five curators, at $2,400	12,000.00
Five curators, at $2,100	10,500.00
Four assistant curators, at $1,600	6,400.00
Four assistant curators, at $1,400	5,600.00
Four aids, at $1,200	4,800.00
Six aids, at $1,000	6,000.00
Special service by contract	4,000.00
	56,300.00
Clerical staff.	
Chief clerk	2,200.00
Four chiefs of divisions: Correspondence; transportation, storage, and record; publications and labels; installation, at $2,000	8,000.00
One disbursing clerk*	1,200.00

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* This officer receives pay also from the Smithsonian Institution for similar services.

Schedule of the classified service of the officers and employés of the United States National Museum, etc.—Continued.

Designation.	Compensation.
Clerical staff—Continued.	
One clerk of class 4	$1,800.00
Two clerks of class 3	3,200.00
Three clerks of class 2	4,200.00
Four clerks of class 1	4,800.00
Four copyists, at $900	3,600.00
Four copyists, at $720	2,880.00
Six copyists, at $600	3,600.00
Three copyists, at $480	1,440.00
	36,920.00
Preparators.	
Photographer	2,000.00
Assistant photographer	1,000.00
Artist	1,320,00
Chief taxidermist	2,000.00
One taxidermist	1,500.00
Two taxidermists, at $1,000	2,000.00
Two taxidermists, at $720	1,440.00
One modeller	2,000.00
One modeller	1,200.00
One general preparator	1,200.00
One general preparator	900.00
Special service by contract	5,500.00
	22,060.00
Buildings and labor.	
One superintendent of buildings	1,620.00
Two assistant superintendents, at $1,000	2,000.00
Four watchmen, at $780	3,120.00
Twenty-four watchmen and door-keepers, at $600	14,400.00
Twelve laborers, at $480	5,760.00
Three attendants, at $480	1,440.00
Ten attendants and cleaners, at $360	3,600.00
Special service of laborers and cleaners, to be paid by the hour	4,800.00
	36,740.00
Mechanics (*repairing buildings, cases, and objects in the collections*).	
Cabinet-maker, at $3.50 per day	1,095.50
Two painters, at $2.50 per day	1,565.00
One tinner, at $2 per day	626.00
One stone-cutter and mason, at $2 per day	626.00
Six skilled laborers, at $2.50 per day	4,695.00
Six skilled laborers, at $2 per day	3,756.00
Special service by contract	1,800.00
	14,163.50
Furniture and fixtures.	
Engineer of property	2,000.00
One copyist	900.00
One copyist	720.00

Schedule of the classified service of the officers and employés of the United States National Museum, etc.—Continued.

Designation.	Compensation.
Furniture and fixtures—Continued.	
One copyist	$600.00
One copyist	480.00
Six carpenters and cabinet-makers, at $3 per day	5,634.00
Three painters, at $2 per day	1,978.00
Two skilled laborers, at $2.50 per day	1.565.00
Two skilled laborers, at $2 per day	1,252.00
Three laborers, at $1.50 per day	1,408.50
Special service by contract	1,800.00
	18,337.50
Heating, lighting, and electrical service.	
Engineer	1,400.00
One assistant engineer	900.00
Six firemen, at $600	3,600.00
Telephone clerk	720.00
	6,620.00
Miscellaneous.	
Agent	1,200.00
One draughtsman	1,200.00
Two draughtsmen, at $600	1,200.00
Two messengers, at $600	1,200.00
One messenger	540.00
Two messengers, at $480	960.00
Two messengers, at $360	720.00
Four messengers, at $240	960.00
	7,980.00

In presenting these schedules to Congress I have shown what would be the cost of the administration of the Museum, in respect to salaries alone, if it were organized after the manner of the Executive Departments of the Government.

The salary list alone amounts to $199,121, and the amount expended in the previous fiscal year for other purposes was $45,000, a sum which might most advantageously be doubled.

I am not prepared at present to recommend the adoption of such a schedule of classified service, since I am of the opinion that the Museum at the present time has greater need of money to be used in the acquisition of new material by purchase and exploration. The opportunities for making collections are yearly growing less, and many things which can now be done at trifling expense will in a few years be impracticable.

The system of appropriation for specific objects, without designating the number of employés or the amounts of their salaries, has in the past been found to be economical and efficient, and although the necessity of the change to a classified service may arise at some later time, I trust that it may be deferred for the present.

The amount asked for in the estimates for the fiscal year of 1891–'92, for the "preservation of collections," is intended to provide for a certain amount of increase of the collections, and also to provide for the payment of certain salaries.

Increase of the collections.—At the close of the fiscal year (June 30, 1889) a very careful estimate showed that the collections were sixteen times as great in number of specimens as in the year 1882. I desire to call your attention especially to the statements bearing upon this point.

The Museum, as I have already said, is growing as it is fitting that the National Museum of a great country should grow, and it is not only necessary to care for what is already here, but to provide for the reception and display of the great collections which will unquestionably be received in the immediate future.

The extent and character of the accessions during the year is shown in the appended table, from which it appears that the total number of specimens in the Museum is now not far from 3,000,000:

Statistics of accessions to National Museum Collections, 1882 *to* 1885.

Name of department.	1882.	1883.	1884.	[1]1885.
Arts and industries:				
Materia medica	....	4,000	4,442	....
Foods	....	[2]1,244	1,580	....
Textiles	....	....	2,000	....
Fisheries	....	....	5,000	....
Animal products	....	....	1,000	....
Naval architecture	....	....	600	....
Historical relics	....	....	....	....
Coins, medals, paper money, etc	....	....	....	....
Musical instruments	....	....	....	....
Modern pottery, porcelain, and bronzes	....	....	....	....
Paints and dyes	....	....	....	....
"The Catlin Gallery"	....	....	....	....
Physical apparatus	....	....	....	....
Oils and gums	....	....	....	....
Chemical products	....	....	....	....
Ethnology	....	....	200,000	....
American aboriginal pottery	....	....	12,000	....
Oriental antiquities	....	....	....	....
Prehistoric anthropology	35,512	40,491	45,252	....
Mammals (skins and alcoholics)	4,660	4,920	5,694	....
Birds	44,354	47,246	50,350	....
Birds' eggs	....	....	40,072	....
Reptiles and batrachians	....	....	23,495	....
Fishes	50,000	65,000	68,000	....
Mollusks	[3]33,375	....	400,000	....
Insects	1,000	....	[4]151,000	....
Marine invertebrates	[3]11,781	[3]14,825	[5]200,000	....
Comparative anatomy:				
Osteology	3,535	3,640	4,214	....
Anatomy	70	103	3,000	....
Palæozoic fossils	....	20,000	73,000	....
Mesozoic fossils	....	....	100,000	....
Cenozoic fossils	(Included with mollusks.)			....
Fossil plants	....	4,624	[6]7,291	....
Recent plants	....	....	....	....
Minerals	....	14,550	16,610	....
Lithology and physical geology	[7]9,075	12,500	18,000	....
Metallurgy and economic geology	....	30,000	40,000	....
Living animals	....	....	....	....
Total	193,362	263,143	1,472,600	....

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[1] No census of collection taken.

[2] Including paints, pigments, and oils.

[3] Catalogue entries.

[4] Professor Riley's collection numbers 15,000 specimens.

[5] Estimated.

[6] Fossil and recent.

[7] In reserve series.

Statistics of accessions to National Museum collections, 1886 *to* 1889.

Name of department.	1885–'86.	1886–'87.	1887–'88.	1888–'89.
Arts and industries:				
Materia medica	4,850	5,516	5,762	5,942
Foods	[1]822	[2]877	[3]877	911
Textiles	3,063	3,144	[3]3,144	3,222
Fisheries	[1]9,870	10,078	[3]10,078	[3]10,078
Animal products	2,792	2,822	[3]2,822	2,948
Naval architecture	....	....	....	[3]600
Historical relics	1,002	13,634	14,640	[3]14,990
Coins, medals, paper money, etc.	1,005			
Musical instruments	400	417	427	[3]427
Modern pottery, porcelain, and bronzes	2,278	2,238	3,011	[3]3,011
Paints and dyes	[1]77	100	[3]100	109
"The Catlin Gallery"	500	500	[3]500	[3]500
Physical apparatus	250	251	[3]251	[3]251
Oils and gums	[1]197	198	[3]198	213
Chemical products	[1]659	661	[3]661	688
Ethnology	[4]500,000	503,764	505,464	506,324
American aboriginal pottery	25,000	[4]26,022	[4]27,122	28,222
Oriental antiquities	....	....	....	850
Prehistoric anthropology	65,314	101,659	108,631	116,472
Mammals (skins and alcoholics)	7,451	7,811	8,058	8 275
Birds	55,945	54,987	56,484	57,974
Birds' eggs	44,163	[5]48,173	50,055	50,173
Reptiles and batrachians	25,344	27,542	27,664	28,405
Fishes	75,000	100,000	101,350	107,350
Mollusks	[6]460,000	425,000	455,000	468,000
Insects	[4]500,000	[4]585,000	595,000	603,000
Marine invertebrates	[4]350,000	[4]450,000	515,000	515,300
Comparative anatomy:				
Osteology	10,210	[4]11,022	11,558	11,753
Anatomy				
Palæozoic fossils	80,482	81,491	84,649	91,126
Mesozoic fossils	69,742	70,775	70,925	71,236
Cenozoic fossils	....	....	....	....
Fossil plants	[7]7,429	8,462	10,000	10,178
Recent plants	30,000	[4]32,000	[4]38,000	38,459
Minerals	18,401	18,601	21,896	27,690
Lithology and physical geology	20,647	[4]21,500	22,500	27,000
Metallurgy and economic geology	48,000	[4]49,000	51,412	52,076
Living animals	....	....	220	491
Total	2,420,944	2,666,335	2,803,459	2,864,244

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[1] Duplicates not included.

[2] Feeds only.

[3] No entries of material received during the year have been made on the catalogue.

[4] Estimated.

[5] 2,235 are nests.

[6] Including Cenozoic fossils.

[7] Exclusive of Professor Ward's collection.

Catalogue entries.—The number of entries made in the catalogues of the various departments in the Museum during the year has been 23,171.

The registrar states that 16,625 boxes and packages* have been received during the year and entered upon the transportation records of the Smithsonian Institution. Of this number 2,182 contained specimens for the Museum.

PRINCIPAL ACCESSIONS TO THE COLLECTIONS.

Among the collections received during the year, those from the U. S. Geological Survey and the Bureau of Ethnology are especially noteworthy. The material transferred by the U. S. Fish Commission to the National Museum included two very valuable collections made by the steamer *Albatross* during the voyage from Washington to San Francisco and while cruising off the coast of Alaska.

The accessions received during the year from general sources are fully up to the standard of previous years. Among the most important are the following:

Ethnological.—Collections from Dr. James Grant Bey, of Cairo, Egypt, and from Mr. W. W. Rockhill, formerly connected with the German legation in Pekin, the former collection from Egypt, the latter illustrative of the religious practices, occupations, and amusements of various peoples in different parts of China, Thibet, and Turkestan; a collection of oriental seals from Mrs. Anna Randall Diehl, of New York City; casts of Assyrian and Egyptian objects obtained by Prof. Paul Haupt, of Johns Hopkins University.

The valuable co-operation of the Bureau of Ethnology is evidenced in the transmission of a large and interesting collection of pottery, stone implements, woven fabrics, shells, beans, etc., collected by Major J. W. Powell, Arthur P. Davis, Gerard Fowke, Dr. E. Boban, Dr. H. C. Yarrow, James Stevenson, Dr. J. S. Taylor, C. C. Jones, James D. Middleton, General G. P. Thruston, James P. Tilton, H. P. Hamilton, Victor Mindeleff, H. W. Henshaw, G. H. Hurlbut, W. W. Adams, De L. W. Gill, William A. Hakes, W. H. Holmes, and Charles L. R. Wheeler. This collection was the result of personal research in the following localities: Mexico, Peru, New Mexico, Wisconsin, California, Arizona, Alabama, Georgia, Pennsylvania, Tennessee, Massachusetts, New York, and Virginia.

Archæological.—Collection of aboriginal pottery from Lake Apopka, Florida, contributed by Dr. Featherstonehaugh, and a collection of similar material from Perdido Bay, Alabama, presented by Mr. F. H. Parsons, of the U. S. Coast and Geodetic Survey; a large collection of prehistoric weapons and ornaments from graves in Corea, presented by Mr.

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*An increase of 4,225 over the number received last year.

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P. L. Jouy; a valuable collection of prehistoric antiquities, for the most part from the Ohio River Valley, deposited by Mr. Warren K. Moorehead, of Xenia, Ohio.

Mammals.—A full-grown moose collected and presented by Col. Cecil Clay, of the Department of Justice; a fresh specimen of Sowerby's whale, contributed by Capt. J. L. Gaskell, keeper of the United States Life-Saving Station at Atlantic City; a skin of *Ovis musimon*, a skeleton of *Monachus albiventer*, and several European bats, received from the Royal Zoological Museum at Florence, Italy; three specimens of American elk presented by Hon. W. F. Cody; a Rocky Mountain sheep, contributed by Mr. George Bird Grinnell, of New York.

Birds and Birds' Eggs.—A rare collection of birds from the National Museum at Costa Rica; a valuable collection of skins from the Old World, presented by Dr. C. Hart Merriam, of the Department of Agriculture; a collection of Japanese birds, purchased from Mr. P. L. Jouy, of the National Museum; bones of Pallas cormorant, collected at the Commander Islands, Kamtchatka, by Dr. Leonhard Stejneger, of the National Museum, the only bones of this bird extant; a collection of typical Australian birds in alchohol, from the Australian Museum, Sydney, New South Wales; an interesting collection of birds' eggs and nests, presented by Mr. Dennis Gale, of Gold Hill, Colo.; eggs of *Cardellina rubrifrons*, new to the collection and to science, contributed by Mr. William W. Price, of Tombstone, Ariz.

Fishes.—Collections of fishes from the Gulf of California, transmitted by Messrs. O. P. Jenkins, of De Pauw University, and B. W. Evermann, of the State Normal School at Terre Haute, Ind.; a collection of fishes from New Zealand, sent in exchange from the Otago University Museum, at Dunedin, New Zealand.

Mollusks.—A valuable collection of marine and terrestrial shells presented by Messrs. F. B. and J. D. McGuire, of Washington.

Insects.—A large series of insects purchased from Dr. Taylor Townsend by the Department of Agriculture and transferred to the museum; an extensive series of dried Coleoptera presented by Mr. G. W. J. Angell, of New York.

Marine Invertebrates.—A collection of crustaceans from Japan, obtained by Mr. Romyn Hitchcock, of the National Museum; specimens of marine invertebrates collected by Lieut. J. F. Moser, of the U. S. Coast and Geodetic Survey, at Cape Sable, Florida.

Fossils.—A collection of cretaceous fossils presented by President David S. Jordan, of Indiana State University; a large series of Lower Cambrian fossils from Conception Bay, Newfoundland, including the types of thirteen species, collected and transferred to the Museum by Mr. C. D. Walcott, of the U. S. Geological Survey.

Botany.—Herbarium specimens from Dr. Ferdinand von Müller, of Melbourne, Australia; a series of specimens of algæ from the New England coast, presented by Mr. F. S. Collins, of Malden, Mass.; agatized wood from the Drake Manufacturing Company, Sioux Falls, Dak.; fossil leaves from Constantine von Ettingshausen, of the University of Gratz, Austria-Hungary.

Geology.—Specimens of ancient and modern marbles from Europe and Africa received in exchange from the Museum of Natural History in Paris; a series of metamorphic and eruptive rocks, presented by Prof. O. A. Derby, of the National Museum of Brazil; a collection of minerals consisting of nearly 1,400 specimens, and obtained by Prof. S. L. Penfield, of the U. S. Geological Survey, in St. Lawrence County, N. Y.; a similar collection gathered by Mr. W. F. Hillebrand, of the U. S. Geological Survey, in Colorado, Utah, New Mexico, and Arizona; a series of petroleums and related material collected by Prof. S. F. Peckham, of Providence, R. I., in connection with his work for the Tenth Census.

Miscellaneous.—The following specially important collections have also been added to the collections during the year: A collection of drugs, from Dr. J. W. Jewett, examiner of drugs, custom-house, New York City, and a collection of similar material transmitted by the royal gardens at Kew; a valuable collection of photo-mechanical process work presented by Prof. Charles F. Chandler, of Columbia College, New York; General Washington's toilet-table deposited by Mrs. Thomas C. Cox, of Washington; account-book belonging to General Washington, together with a number of engravings and other personal property of General Washington, deposited by Mr. Lawrence Washington, of Virginia; an interesting collection of coins, including specimens of the "hook money" and other coins of the native princes of India, from Hon. W. T. Rice, United States consul at Horgen, Switzerland; a model of the locomotive "Old Ironsides," built by Matthias Baldwin in 1832, and presented by the Baldwin Locomotive Works; a model of Trevithick's locomotive, built in 1804 by Mr. D. Ballauf, from drawings lent to the Museum; a stereoscope with examples of the daguerreotype process, and the old albumen process on glass received from Mrs. E. J. Stone, of Washington; a valuable series of prints in carbon and other processes presented by Mr. J. W. Osborne, of Washington. Some of the most valuable collections received during the year were obtained through the co-operation of Government officials, and are referred to at length in the report on the Museum for this year.

Co-operation of Departments and Bureaus of the Government.—The Museum has received, as in past years, many valuable contributions from United State consuls, officers of the Army and the Navy, and through the co-operation of the Departments and Bureaus of the Government.

Through the courtesy of the Department of State the work of collectors in foreign countries has been greatly facilitated. The Secretary of the Treasury has issued several permits for the free entry of Museum material.

The Secretary of Agriculture has expressed his willingness to co-operate with the National Museum in the matter of making a forestry exhibit, and Dr. B. E. Fernow has been appointed honorary curator of the collection.

By direction of the Postmaster-General the Superintendent of the Dead Letter Office has been instructed to inform the Museum of the receipt in his office of specimens which might be of value for addition to the collections.

The Superintendent of the Coast and Geodetic Survey has, as in previous years, aided our work in many ways.

Photographic exhibit.—A collection intended to show the uses of photography was prepared by Mr. T. W. Smillie, of the National Museum, for exhibition at the Cincinnati Exposition. This collection included valuable contributions of photographs from Prof. E. C. Pickering, of Harvard University, Mr. J. W. Osborne, of Washington, and from several officers connected with the Government service, notably the Light-House Board, the Army Medical Museum, and the proving ground at Annapolis. At the close of the Exposition this collection was returned to the Museum, and is now being prepared, in connection with additional material which has since been received, for permanent exhibition. It is intended that the scope of this exhibit shall be enlarged so as to take the form of a historical collection in which shall be shown examples of every photographic process that has been invented, together with the appliances used, beginning with the photograph of the solar spectrum as made by Sheele in 1777. Considerable material has been already gathered which will be incorporated in this collection. The first camera made in the United States has been acquired by purchase. A stereoscope containing daguerreotypes and transparencies by the old albumen process on glass has been presented by Mrs. E. J. Stone. The Scoville Manufacturing Company of New York has presented a series of cameras showing the latest improvements, and from the Eastman Dry Plate Company, of Rochester, N. Y., has been received a Kodak camera, together with a series of enlarged photographs illustrating its use.

Distribution of duplicate specimens.—Duplicate specimens, to the number of 11,382, were distributed during the year among museums, colleges, and individuals. The following table shows the character and extent of these distributions:

Nature of specimens distributed.	Number of specimens.
Ethnology	268
American prehistoric pottery	32
Prehistoric anthropology	833
Mammals	42
Birds	636
Birds' eggs	3
Reptiles	47
Fishes	39
Mollusks	369
Insects	197
Marine invertebrates	2,072
Invertebrate fossils	588
Fossil and recent plants	2,945
Minerals	2,370
Rocks	804
Metallurgy	58
Photographs and drawings	79
Total	11,382

The decrease in the number of specimens thus given away, as compared with last year, is accounted for by the fact that only 2,072 specimens of marine invertebrates have been distributed this year, while last year 24,750 specimens of this class were presented to applicants. Eliminating this class of specimens, the number distributed this year is double that of last year. The number of requests for duplicate specimens increase yearly. It is hoped that in the future it may be possible for the Museum to extend its usefulness in this important part of the work. The material now available for distribution is quite inadequate to supply the demand. The curators of mineralogy and of geology obtained a large quantity of material during the past summer for this special purpose. As soon as it has been classified and arranged into sets, an endeavor will be made to fill the many applications for mineralogical and lithological material now awaiting action. The matter of making up sets of duplicate bird-skins is now receiving careful attention, there being much of such material available for distribution.

Labels.—During the year, 3,991 labels were printed, chiefly for use in the departments of metallurgy, materia medica, and birds, *Accessions to the library.*—The number of publications added to the library during the year is 6,052, of which 618 are volumes of more than 100 pages, 903 are pamphlets, 4,343 are parts of regular serials, and 158 charts. The most important accession was the gift by the heirs of the late Dr. Isaac Lea, consisting of 137 volumes, 276 "parts," and 693 pamphlets, and including a nearly complete series of the "Proceedings of the Zoological Society of London." There are now nineteen sectional libraries attached to the several curatorships in the Museum.

Publications of the Museum.—The issue of Museum publications during the year has been unusually small, owing to the pressure of Congressional work at the Printing Office during the long Congressional session of 1888, which caused the Museum work to be set aside. A number of special publications are partially completed, and will be issued soon after the beginning of the next fiscal year.

During the year volume 10 of Proceedings of the U. S. National Museum (1887) was issued. This contains viii + 771 pages and 39 plates. It includes 78 papers by 26 authors, 10 of whom are officers of the Museum. Nearly three-fourths of the papers relate to birds and fishes. In the appendix is printed a catalogue of the exhibit prepared by Mr. S. R. Koehler, in charge of the section of graphic arts, for the Ohio Valley Centenial Exposition. Special papers were prepared by the curators of several departments, in connection with the exhibits for this exposition, which will be reprinted in Section III of the Museum report for the present year.

Bulletin 33 of the United States National Museum, "A catalogue of minerals and their synomyms alphabetically arranged for the use of museums" by Prof. T. Egleston, Ph. D., of Columbia College, was issued in May. This volume contains a complete catalogue of the names of minerals and their synonyms, and will be of much value to students of mineralogy and others interested in this science.

The assistant secretary in charge of the Museum has submitted a statement reviewing the history of the publications of the Museum, and making certain suggestions with a view to increasing the extent of the editions and to the establishment of a systematic method of distribution. From this statement I quote the following remarks and recommendations relating to the Proceedings and Bulletin:

"The Proceedings was established for the purpose of securing prompt publication of the discoveries in the Museum. In order to secure this object the printing has been done, signature by signature, as fast as matter was prepared. A certain number of signatures has always been distributed, as soon as published, to scientific institutions and specialists. The number of sets of signatures thus distributed has been in the neighborhood of 200.

"This method of publication has seemed to be to some extent wasteful, and it is thought that equally good results may be secured by distributing a certain number of the advance copies in the form of authors' extras. In making arrangements for the printing of Volume XII it was decided that out of the edition of 1,200 copies, 100 should be delivered in signatures as fast as printed, and 300 in extras or reprints, in paper covers, of which 50 are to be given to the authors and the remainder distributed to specialists in the various departments to which the papers relate, who are not otherwise provided with the publication; while the 800 remaining volumes are to be bound previous to distribution.

"In special instances, where a given paper in the Proceedings is believed to possess great general interest, it has been customary to print a considerable number of extra copies.

"The publication of the Proceedings and the Bulletin was at first paid for from the printing fund of the Interior Department, with which the Museum was at that time in close relations in respect to financial matters. Subsequently it was paid for from the fund for the printing of Museum labels, estimates for which were annually submitted by the Secretary of the Institution. The amount asked for was usually $10,000. In the Book of Estimates the Museum appeared as asking a certain sum for printing, though the money was actually included in the gross sum allotted to the Interior Department as a printing fund.

"In 1882, a separate appropriation was made for the first time, in these words: 'For the National Museum, for printing labels and blanks and for the Bulletins and annual volumes of the Proceedings of the Museum, ten thousand dollars.'

"In 1888 the appropriation for the fiscal year 1888–9 was made in the same words, but was not included, as heretofore, in the appropriations for the Department of the Interior.

"The edition of the earlier volumes of the Proceedings and Bulletins was usually only 1,000, of which a portion was distributed by the Department of the Interior and a portion by the Museum. The number received by the Museum being sometimes 500 and sometimes as few as 250. The edition placed at the disposal of the Museum being so small, and withal so uncertain as to extent, the distribution was always of necessity informal, and no effort was made to supply a regular list of institutions and specialists. A considerable number was expended in the work of the Museum, and the remainder were sent to correspondents of the Museum in exchange for publications, for specimens, and incidentally to such institutions as might apply for copies, as well as to individuals, especially students who made it evident that they were in a position to make good use of the books.

"Formal publication was undertaken by the Smithsonian Institution, it being the intention that the first cost of composition and electrotyping having been provided for by the special Congressional appropriation, the Smithsonian Institution should avail itself of the electrotype plates and use them in making up certain volumes of the Miscellaneous Collections. The papers published in the Proceedings and Bulletins of the Museum were of precisely the same character which, since 1862, had made up the great majority of the most important papers in the Miscellaneous Collections. The Institution undertook to print an edition of 1,200 copies in the form of volumes of the Miscellaneous Collections and to distribute them to the principal libraries of the world. This was, at the time, regarded as advantageous, since the cost of composition and electrotyping made up at least two-thirds of the cost of the edition of 1,200, while the miscellaneous distribution, for which the Institution, in the case of similar publications printed at its own expense, had been accustomed to provide, was now already arranged for out of the preliminary issue of several hundred copies paid for from the Museum fund.

"The first four volumes of the Proceedings and the first sixteen numbers of the Bulletin were published in this manner.

"Since 1883 no *publication* of the Bulletins has been made, and none has been made in the case of the Proceedings since 1882.

"There remain *unpublished* eleven volumes of the Proceedings and twenty-one numbers of the Bulletin—in all, enough to make ten thick volumes of the Miscellaneous Collections. Possibly, by condensation and omissions, the number might be reduced to nine volumes. If the Institution were to undertake to print the edition of 1,000 now customary in the case of the Miscellaneous Collections, the cost would be not less than $9,000. The same amount expended by the Institution in printing fresh matter would probably not produce more than one and one half volumes, or at most two volumes, of Miscellaneous Collections.

"The Institution is possibly under obligations to provide for the publication of these papers, since in the advertisement to each volume of the Bulletin as late as 1887 (Bulletin 33) appears the statement that 'from time to time the publications of the Museum which have been issued separately are combined together and issued as volumes of the Miscellaneous Collections.'

"As a matter of fact, however, the publication of an edition of 1,000 copies by the Smithsonian Institution would not really meet the necessities of the case, since it would leave unsupplied a very large number of libraries quite as deserving as those already on the distribution list."

It seems, in view of all these facts, that it is not desirable that the Institution should undertake hereafter the publication of the Museum Bulletin and Proceedings, since it is evident that these will increase in bulk from year to year, and that the demand upon the Institution would very soon become too burdensome. Dr. Goode suggests that Congress be requested to increase the appropriation for the Museum printing to $18,000 in order that an edition of 2,000 copies may be printed in addition to the customary number. If this arrangement should be carried out, the Smithsonian Institution would be relieved of the responsibility of providing for the publication of these documents. The issue of the enlarged edition would commence with volume 13 of the Proceedings and with Bulletin 40 or 41. In considering the question of publishing back volumes of the Proceedings and Bulletin, Dr. Goode remarks:

"When we come to the question of the *publication* of the back volumes, volumes 1 to 4 of the Proceedings and Bulletins 1 to 16 may be regarded as *published*, although not to the extent to which it would seem desirable in the way of supplying local institutions. Of the following, we have in hand enough to make a very fair distribution, viz: Proceedings, volumes 10 and 11 and Bulletins No. 33 to 37. Of volumes 5 to 9 of Proceedings and of Bulletins 17 to 32, however, no systematic publication can be made without the printing of an additional number of copies."

Students.—In accordance with the policy of past years, free access to the collections has been granted to students in the various branches of natural history, and in many instances specimens have been lent to specialists for comparison and study. Instruction in taxidermy has been given to several applicants. Two of these intend to apply the knowledge thus acquired in making collections for the Museum, namely, Lieut. E. H. Taunt, United States consular agent to the Congo, and Mr. Harry Perry, who expects to spend several years in Honduras. Mr. T. W. Smillie has given instruction in photography to the following persons: Lieut. E. H. Taunt, Mr. W. H. Perry, Mr. Barton Bean, Mr. Howard, Prof. Daish, and Miss Frances B. Johnston.

Special researches.—The special researches of the curators are referred to at length in the report of the National Museum. I may say, in this connection, that the time which those officers are able to devote to work of this kind is very limited, owing to the large amount of mechanical and routine work to which, in the absence of necessary assistance, it is necessary for them to give their personal attention.

Meetings and lectures.—The use of the lecture hall has been granted for lectures and meetings of scientific societies, as follows: The National Dental Association met on July 24, 25, and 26. On the evening of September 20 was held one of the meetings of the Medical Congress. The American Ornithologists' Union held its sixth congress on November 13, 14, and 15. A meeting of the Department of Superintendence of the National Educational Association was held on March 6, 7, and 8. The National Academy of Sciences held its meetings on April 16, 17, and 18. The Council of the American Geological Society and the American Committee of the International Geological Congress held business meetings on April 19. The American Historical Association held its fifth meeting in Washington during Christmas week; the evening sessions being held at the Columbia University, the morning sessions at the Museum.

In the Toner course Dr. Harrison Allen delivered a lecture on May 29 entitled "Clinical Study of the Skull undertaken in connection with the Morbid Condition of the Jaws and Nasal Chambers."

The usual course of Saturday lectures, ten in number, beginning March 9 and ending May 11, was delivered under the direction of the oint committee of the scientific societies of Washington.

The usual courtesies have been extended to museums and other public institutions by the gift and loan of drawings and photographs of specimens and copies of Museum labels.

Visitors.—The number of visitors to the Museum building is constantly increasing. The register shows that a total number of 374,843 persons visited the Museum during the year. This exceeds the number for last year by 125,818, and shows an increase of more than 50 per cent. The visitors to the Smithsonian building numbered 149,618, an increase of 46,177 over last year. On March 5, owing to the crowds of visitors to the city attending the Inauguration cermonies, no less than 86,107 persons visited the Smithsonian and Museum buildings.

Personnel.—During the year a department of forestry has been established, and with the consent of the Secretary of Agriculture, Dr. B. E. Fernow, chief of the forestry division of the Department of Agriculture, has been appointed its curator.

Dr. George Vasey, of the Department of Agriculture, has been appointed curator of botany, and in that capacity controls the botanical collections in the National Museum and in the Department of Agriculture. Prof. Paul Haupt, curator of Oriental antiquities in the National Museum, was designated as the representative of the Smithsonian Institution at the Eighth International Congress of Orientalists, to meet in Stockholm and Christiania in September. Prof. Otis T. Mason was instructed to proceed to Europe to visit the principal ethnological museums of France, Germany, Denmark, and England, for the purpose of making arrangements for the increase of the collections at the U. S. National Museum, and incidentally, through the study of methods of installation, of providing for the more effectual preservation and utilization of these collections. Mr. Thomas Wilson was directed to proceed to Europe to visit the principal museums of France, England, and Dublin for the purpose of studying the methods of installation employed by the European museums.

On August 13, Mr. Silas Stearns, of Pensacola, Fla., who for many years has been a correspondent of the Smithsonian Institution, and has made important collections of fishes in the Gulf of Mexico, died at Asheville, N. C.

Explorations.—During the summer of 1888, Mr. George P. Merrill, curator of geology, made a collecting trip to North Carolina, Pennsylvania, New York, Vermont, New Hampshire, Massachusetts, and Maine. Large collections of rocks were obtained for the Museum. Mr. Thomas Wilson, curator of prehistoric anthropology, visited mounds in Ohio, and made interesting collections. Ensign W. L. Howard, U. S. Navy, who, acting under orders from the Navy Department, sailed for Kotzebue Sound in May last, is making collections in Alaska for the National Museum. Prof. O. P. Jenkins, of De Pauw University, Indiana, is visiting the Hawaiian Islands for the purpose of collecting fishes. A series of his specimens has been promised for the National Museum. In August Dr. W. F. Hillebrand, of the U. S. Geoglogical Survey, visited some of the Western States and Territories partly with a view to making collections of minerals. These will eventually be incorporated with the Museum collections.

Centennial Exposition of the Ohio Valley and Central States.—The act of Congress directing the Executive Departments of the Government, the Department of Agriculture, and the Smithsonian Institution (including the National Museum and the U. S. Commission of Fish and Fisheries) to participate in the Centennial Exposition of the Ohio Valley and Central States, to be held in Cincinnati from July 4 to October 27, 1888, passed both houses of Congress and received the approval of the President on May 28. In addition to this, a joint resolution was adopted in which the true intent of the act was declared, with a view to correcting certain misapprehensions which had arisen in regard to the objects for which the money appropriated by Congress in connection with this exhibition could be legally expended. This joint resolution was approved by the President on July 16. A copy of the act and of the joint resolution will be found in the report of the assistant secretary for 1889, wherein is also published a full account of the exhibit prepared under the direction of the Smithsonian Institution in accordance with the terms of the act referred to. Of the $50,000 appropriated for the Smithsonian Institution $10,000 was set apart for the U. S. Fish Commission. About 42,000 square feet of exhibition space were reserved for the Government exhibits, 12,000 square feet being devoted to that of the Smithsonian Institution. The assistant secretary was on May 29 appointed representative of the Smithsonian Institution, and active operations for the preparation of a creditable display were immediately commenced. It was unfortunate that only a little more than a month intervened between the passage of the act and the opening of the exhibition. The Smithsonian Institution has, however, had a varied experience in preparing exhibits at a short notice. The first car-load of exhibits left for Cincinnati on June 22, and the last of the twelve car-loads which were sent was shipped on July 12. The following departments of the National Museum were represented at the exhibition, the number of square feet assigned to each being also given:

Department.	Square feet.
Prehistoric anthropology	600
Ethnology	1,120
Biblical archæology	280
Transportation and engineering	600
Naval architecture	312½
Graphic arts	1,500
Photography	925
Mammals (systematic exhibit)	953
" (extermination series)	884
Birds	325
Insects	238
Mollusks	250
Marine invertebrates	125
Botany	90
Mineralogy	60
Total	8,262½

In addition to this a special exhibit was prepared by the Bureau of Ethnology, Maj. J. W. Powell, Director, to which 1,425 square feet were assigned. The total number of visitors to the exhibition was 1,055,276.

Dr. Goode was unable, on account of other duties, to personally attend the exhibition, and Mr. R. E. Earll was placed in charge of the exhibit.

Considerable difficulty was experienced in connection with the expenditure of the funds appropriated by Congress for the work of preparing exhibits, owing to the decisions of the special auditor appointed to audit the exposition accounts. His objections were in every instance finally withdrawn, and all vouchers have now, after protracted delays, been approved by that official. An extraordinary number of points of a trivial nature were raised, which necessitated the writing of as numerous letters to answer questions which had not previously been understood to come within the province of an auditor. In view of this experience it is urged that should Congress at any time direct the Smithsonian Institution to participate in future expositions, the law be so framed as to require the appointment of an auditing officer who is familiar with the demands of exhibition work. If, however, this be impracticable, it seems proper that the responsibility of selecting and deciding as to what should be the character of the exhibits should be left entirely to the judgment of the various Departments, the auditor's work being limited to the examination of the accounts, which should of course be sufficiently detailed to prevent errors. Another cause of delay in settling the exhibition accounts was due to the fact that the disbursing officer was stationed at Newport, Ky., instead of Washington, where by far the greater part of the bills were contracted. The paymaster drew checks upon the Cincinnati depository only, and this method appeared to be unjust, since it obliged employés to wait several days before receiving payment, and in addition to lose some part of their money, owing to the refusal of the Treasury Department in Washington to honor the checks. The only alternative for them was to present the checks to local banks, paying the usual discount rates.

Marietta Centennial Exposition.—By an Executive order, dated July 11, 1888, permission was granted to the heads of the departments represented at the Cincinnati Exhibition to send to the Centennial Exposition at Marietta, Ohio, such objects as could be conveniently spared either from the exhibits at Cincinnati or direct from Washington. In accordance with this order an exhibit was prepared under the direction of the assistant secretary. Mr. W. V. Cox, chief clerk of the Museum, was appointed by him as his representative. Since only one day intervened in this instance between the issuing of the Executive order and the opening of the exhibition there was no time to be lost. An exhibit, with a total weight of 7,327 pounds, was prepared and installed at Marietta before the opening of the exhibition. The exhibit included specimens selected from the Haida collection of ethnological objects, lithographs of the game fishes of the United States, a series of medals, photographs of public buildings in Washington, a collection of autotypes, and a series of specimens illustrating the composition of the human body. In addition to these a collection of models, engravings, and paintings illustrative of the methods of transportation adopted by the early settlers in America was selected by Mr. J. E. Watkins from the exhibit of the department of transportation at the Cincinnati Exhibition and forwarded to Marietta.

The organization of the Government Board which was charged with preparations for the Philadelphia Exhibition was so far superior to that of those more recently formed, that it would seem desirable that the plan in favor at that time should be followed as far as possible should similar work be decided upon in connection with future exhibitions.

I regret the growing tendency to withdraw for special expositions a considerable portion of some of the most valuable parts of the collection. The Museum is now approaching a final arrangement in classification, and the objections to this are therefore much stronger now than some years ago when the condition of the collections was more unsettled. The preparation for an exposition seriously impairs the work of the Museum, while considerable damage invariably results to the collections, and often in such a degree that it requires much time and expense to restore them. The managers of local expositions are no longer satisfied to accept the specimens which can be most conveniently spared, but are always anxious to have the most valuable and costly objects. Temporary exposition buildings are never made fire-proof, and the time is sure to come, if the present practice prevails, when some exhibition building containing Government collections to the value of hundreds of thousands of dollars will be destroyed. The experience of the Mexican Government in its participation at the New Orleans Exposition, and of the Government of New South Wales in 1883, may be cited as warnings. If, however, Congress should order in future our participation in expositions, I would especially urge that provision for the work be made at least six months before the date of opening. In each instance in the past the notice has always been extremely short, usually only a few weeks, and in one or two cases less than a week.

I am also disposed to lay stress upon the necessity of liberal appropriations, which should be made with the understanding that new material may be obtained, which shall not only replace that which has been lost in past exhibitions, but shall enrich the Museum collections for home use and for use in future exhibition work. If this necessity is not recognized, the result will be that in a few years the Museum will be greatly impoverished, not only by the destruction of material, but also by the dissipation of the energy of its staff, which, being applied to temporary purposes in this way, is taken away from its legitimate work. It would indeed seem only fair that a distraction of this kind, which affects in large degree every officer and employé, should be compensated for by the opportunity to purchase new material which will remain permanently the property of the Government and increase the usefulness of the governmental Museum work.

BUREAU OF ETHNOLOGY.

Ethnologic researches among the North American Indians were continued, under the Secretary of the Smithsonian Institution, in compliance with acts of Congress, during the year 1888–'89. Maj. J. W. Powell, as director of the work, has furnished the following account of operations:

A report upon the work of the year is most conveniently given under two general heads, viz., field work and office work.

FIELD WORK.

The field work of the year is divided into (1) mound explorations and (2) general field studies, the latter being directed chiefly to archæology, linguistics, and pictography.

Mound explorations.—The work of exploring the mounds of the eastern United States was, as in former years, under the superintendence of Prof. Cyrus Thomas. The efforts of the division were chiefly confined to the examination of material already collected, and to the arrangement and preparation for publication of the data in hand. Field work received much less attention, therefore, than in previous years, and was mainly directed to such investigations as were necessary to elucidate doubtful points, and to the examination and surveys of important works which had not before received adequate attention.

The only assistants whose engagements embraced the entire year were Mr. James D. Middleton and Mr. Henry L. Reynolds. Mr. Gerard Fowke, one of the regular assistants, closed his connection with the division at the end of the second month. Mr. John W. Emmert was engaged as a temporary assistant for a few months.

During the short time he remained with the division, Mr. Fowke was engaged in exploring certain mounds in the Scioto Valley, Ohio, a field to which Messrs. Squier and Davis had devoted much attention. The re-examination of this field was for the purpose of investigating certain typical mounds which had not been thoroughly examined by those explorers.

Mr. Middleton was employed from July to the latter part of October in the exploration of mounds and other ancient works in Calhoun County, Ill., a territory to which special interest attaches because it seems to be on the border line of different archæologic districts. From October until some time in December he was engaged at Washington in preparing plates of Ohio earth-works. During the next month he made re-surveys of some of the more important inclosures in Ohio, after which he continued work in the office at Washington until the latter part of March, when he was sent to Tennessee to examine certain mound groups, and to determine, so far as possible, the exact locations of the old Cheroki "Over-hill towns." The result of this last-mentioned investigation was one of the most valuable of the year, as it indicated that each of these "Over-hill towns" was, with possibly one unimportant exception, in the locality of a mound group.

Near the close of October Mr. Reynolds, having already examined the inclosures of the northern, eastern, and western sections of the mound region, was sent to Ohio and West Virginia to study the different types found there, with reference to the chapters he is preparing on the various forms of inclosures of the United States. While thus engaged he explored a large mound connected with one of the typical works in Paint Creek Valley, obtaining unexpected and important results. The construction of this tumulus was found to be quite different from most of those of the same section examined by Messrs. Squier and Davis.

Mr. Emmert devoted the few months he was employed to the successful exploration of mounds in eastern Tennessee. Some important discoveries were made, and additional interesting facts were ascertained in regard to the customs of the mound builders of that section.

General field studies.—Early in the month of July Col. Garrick Mallery proceeded to Maine, Nova Scotia, and New Brunswick, to continue investigation into the pictographs of the Abnaki and Micmac Indians, which had been commenced in 1888. He first visited rocks on the main-land, near Machiasport, and on Hog Island, in Holmes Bay, a part of Machias Bay. In both localities pecked petroglyphs were found, accurate copies of which were taken. Some of them had not before been reported. They were probably of Abnaki origin, either of the Penobscot or the Passamaquoddy divisions, the rocks lying on the line of water communication between those divisions. From there he proceeded to Kejemkoojik Lake, on the border of Queen's and Annapolis counties, Nova Scotia, and resumed the work of drawing and tracing the large number of petroglyphs found during the previous summer. Perfect copies were obtained of so many of them as are amply sufficient for study and comparison. These petroglyphs were etched and were made by Micmacs. The country of the Malecites, on the St. John's River, New Brunswick, was next visited. No petroglyphs were discovered, but a considerable amount of information upon the old system of pictographs on birch bark and its use was obtained. Illustrative specimens were secured, together with myths and legends assisting in the elucidation of some of the pictographs which had been obtained elsewhere.

Dr. W. J. Hoffman proceeded in July to visit the Red Lake and White Earth Indian reservations in Minnesota. At Red Lake he obtained copies of birch-bark records pertaining to the Midewiwin or Grand Medicine Society of the Ojibwa, an order of shamans or priests professing the power of prophesy, the cure of disease, and the ability to confer success in the chase. The introductory portion of the ritual of this society pertains particularly to the Ojibwa ideas of creation. At the same place several mnemonic charts were secured, consisting of birch-bark records of hunting expeditions, battles with neighboring tribes of Indians, maps, and songs. He also investigates the former and present practice of tattooing, and the Ojibwa works of art in colors, beads, and quills.

At White Earth Reservation two distinct charts of the Grand Medicine Society were obtained, together with full explanations by two of the chief midé or shamans, one of whom was the only fourth-degree priest in either of the reservations. Although a considerable amount of difference between these three charts is apparent, the principles are common to them all as well as the general course of the initiation of candidates. An interesting fact appears in the survival of archaic forms in the charts and ritual, seemingly indicating a considerable antiquity. A large number of mnemonic songs was also obtained at this reservation. In addition to much of the ritual, secured directly from the priests, in the original language, translations of the songs were also recorded in musical notation. After the completion of his labors at the above reservations, Dr. Hoffman proceeded to Pipestone, Minn., to secure copies of pictographs reported to occur upon the cliffs of that well-known locality. The reports of the great number of petroglyphs were found to have been greatly exaggerated, though a number of what appeared to be personal names were found on the rocks. He then returned to St. Paul, Minn., to search the records of the library of the Minnesota Historical Society for copies of pictographs reported to have been made near La Pointe, Wis. Little information was gathered, although it is well known that such records existed upon conspicuous cliffs and rocks near Lake Superior at and in the vicinity of Bayfield and Ashland.

Dr. Hoffman afterwards made a personal examination of the "pictured cave" 8 miles northeast of La Crosse, Wis., to obtain copies of the various characters occurring there. These are rapidly being destroyed by the disintegration of the rock. The colors employed in delineating the various figures consisted of dark red and black. The figures represented deer, human beings, and various animals and forms not now distinguishable.

Mr. H. W. Henshaw spent the months of August, September, and October on the Pacific coast, engaged in the collection of vocabularies of certain Indian languages, with a view to their study and classification. The Umatilla Reservation in Oregon was first visited with the object of obtaining a comprehensive vocabulary of the Cayuse. Though there are about four hundred of these Indians on the reservation probably not more than six speak the Cayuse tongue. The Cayuse have extensively intermarried with the Umatilla, and now speak the language of the latter, or that of the Nez Percé. An excellent Cayuse vocabulary was obtained, and at the same time the opportunity was embraced to secure vocabularies of the Umatilla and the Nez Percé languages. His next objective point was the neighborhood of the San Rafael Mission, Marin County, Cal., the hope being entertained that here would be found some of the Indians formerly gathered at the mission. He learned that there were no Indians at or near San Rafael, but subsequently found some half dozen on the shores of Tomales Bay, to the north. From one of these a good vocabulary was collected, and, as was expected, was subsequently found to be related to the Moquelumnan family of the interior, to the southeast of San Francisco Bay. Later the missions of Santa Cruz and Monterey were visited. At these points there still remain a few old Indians who retain a certain command of their own language, though Spanish forms their ordinary means of intercourse. The vocabularies obtained are sufficient to prove, beyond any reasonable doubt, that there were two linguistic families instead of one, as had been formerly supposed, in the country above referred to. A still more important discovery was made by Mr. Henshaw at Monterey, where an old woman was found who succeeded in calling to mind more than one hundred words and short phrases of the Esselen language, formerly spoken near Monterey, but less than forty words of which had been previously known. Near the town of Cayucas, to the south, an aged, blind Indian was visited who was able to add somewhat to the stock of Esselen words obtained at Monterey, and to give besides valuable information concerning the original home of this tribe. As a result of the study of this material, Mr. Henshaw determines the Esselen to be a distinct linguistic family, a conclusion first drawn by Mr. Curtin, from a study of the vocabularies collected by Galiano and Lamanon in the 18th century. The territory occupied by the tribe and linguistic family lies coastwise, south of Monterey Bay, as far as the Santa Lucia Mountain.

On July 5 Mr. James Mooney started on a second trip to the Cheroki Nation in North Carolina, returning November 14, after an absence of about four months. During this time he made considerable additions to the linguistic material already obtained by him, and was able to demonstrate the former existence of a fourth, and perhaps even of a fifth, well-marked Cheroki dialect in addition to the upper, lower, and middle dialects already known. The invention of a Cheroki syllabary, which was adapted to the sounds of the upper dialect, has tended to make that the universal dialect. A number of myths were collected, together with a large amount of miscellaneous material relating to the Cheroki tribe, and the great tribal game of ball play, with its attendant ceremonies of dancing, conjuring, scratching the bodies of the players, and going to water, was witnessed. A camera was utilized to secure characteristic pictures of the players. Special attention was given to the subject of Indian medicine, theoretic, ceremonial, and therapeutic. The most noted doctors of the tribe were employed as informants, and nearly five hundred specimens of medicinal and food plants were collected and their Indian names and uses ascertained. The general result of this investigation shows that the medical and botanical knowledge of the Indians has been greatly overrated. A study was made of Cheroki personal names, about five hundred of which were translated, being all the names of Indian origin now existing. The most important results of Mr. Mooney's investigation were the discovery of a large number of manuscripts containing the sacred formulæ of the tribe, written in Cheroki characters by the shamans for their own secret use, and jealously guarded from the knowledge of all but the initiated. The existence of such manuscripts had been discovered during a previous visit in 1887, and a number had been procured. This discovery of genuine aboriginal material, written in an Indian language by shamans for their own use, is believed to be unique in the history of aboriginal investigation, and was only made possible through the invention of the Cheroki syllabary by Sequoia in 1821. Every effort was made by Mr. Mooney to obtain all the manuscripts possible, with the result of securing nearly all such material in the possession of the tribe. The whole number of formulæ obtained is about six hundred. They consist of prayers and sacred songs, explanations of ceremonies, directions for medical treatment, and underlying theories. They relate to medicine, love, war, hunting, fishing, self-protection, witchcraft, agriculture, the ball play, etc., thus forming a complete exposition of an aboriginal religion as set forth by its priests in the language of the tribe.

Early in October Mr. Jeremiah Curtin left Washington for the Pacific Coast. During the remainder of the year he was occupied in Shasta and Humboldt Counties, Cal., in collecting vocabularies and data connected with the Indian system of medicine. This work was continued in different parts of Humboldt and Siskiyou Counties until June 30, 1889. Large collections of linguistic and other data were gathered and myths were secured, which show that the whole system of medicine of these Indians and the ministration of remedies originated in and is limited to sorcery practices.

The field of work of Mr. Albert S. Gatschet during the year was of limited duration. It had been ascertained that Mrs. Alice M. Oliver, now in Lynn, Mass., formerly lived on Trespalacios Bay, Texas, near the homes of the Karankawa, and Mr. Gatschet visited Lynn with a view of securing as complete a vocabulary as possible of their extinct language. Mrs. Oliver was able to recall about one hundred and sixty terms of the language, together with some phrases and sentences. She also furnished many valuable details regarding the ethnography of the tribe. Ten days were spent in this work.

Mr. J. N. B. Hewitt was occupied in field work from August 1 to November 8, as follows: From the 1st of August to September 20 he was on the Tuscarora Reserve, in Niagara County, in which locality fifty-five legends and myths were collectkd. A Penobscot vocabulary was also obtained here, together with other linguistic material. From September 20 to November 8 Mr. Hewitt visited the Grand River Reserve, where a large amount of text was obtained, together with notes and other linguistic material.

Dr. Franz Boas was employed from February to April in preparing for convenient use a series of vocabularies of the several Salish divisions, previously collected by him in British Columbia.

Mr. Victor Mindeleff left Washington on October 23 for St. John's, Ariz., where he examined the Hubbell collection of ancient pottery and secured a series of photographs and colored drawings of the more important specimens. Thence he went to Zuñi and obtained drawings of interior details of dwellings and other data necessary for the completion of his studies of the architecture of this pueblo. He returned to Washington December 7.

Mr. A. M. Stephen continued work among the Tusayan pueblos under the direction of Mr. Victor Mindeleff. He added much to our knowledge of the traditionary history of Tusayan, and has made an extensive study of the house-lore and records of house-building ceremonials. He furnished also a full nomenclature of Tusayan architectural terms as applied to the various details of terraced house construction, with etymologies. He secured from the Navajo much useful information of the ceremonial connected with the construction of their conical lodges, or "hogans," supplementing the more purely architectural records of their construction previously collected by Mr. Mindeleff. As opportunity occurred he gathered small, typical collections of baskets and other textile fabrics illustrative of the successive stages of their manufacture, including specimens of raw materials and detailed descriptions of the dyes used. These collections are intended to include also the principal patterns in use at the present time, with the native explanations of their significance.

OFFICE WORK.

Director Powell has devoted much time during the year to the final preparation of the paper to accompany the map of the linguistic families of North America north of Mexico, the scope of which has been alluded to in previous years. The report and map are now practically completed, and will appear in the Seventh Annual Report of the Bureau, soon to go to press.

Mr. Henshaw was chiefly occupied with the administrative duties of the office, which have been placed in his charge by the Director, and with the completion of the linguistic map, which is now ready for the engraver.

Col. Garrick Mallery, after his return from the field work elsewhere mentioned, was engaged in the elaboration of the new information obtained and in further continued study of, and correspondence relating to, sign language and pictography.

Dr. W. J. Hoffman continued the arrangement and classification of material embracing the subjects of pictography and gesture language of the North American Indians, but more particularly of the date and sketches secured by him during previous field seasons.

While Mr. J. Owen Dorsey did no field work during the year, he devoted much of the time to original investigations. Samuel Fremont, an Omaha Indian, came to Washington in October, 1888, and until February, 1889, assisted Mr. Dorsey in the revision of the entries for the ¢egiha-English Dictionary. A similar work was undertaken by Little Standing Buffalo, a Ponca Indian from the Indian Territory, in April and May, 1889. The summary of Mr. Dorsey's office work is as follows: He completed the entries for the ¢egiha-English Dictionary, and a list of Ponca, Omaha, and Winnebago personal names was made. He translated from the Teton dialect of the Dakota all the material of the Bushotter collection in the Bureau of Ethnology, and prepared therefrom a paper on Teton folk-lore. He also prepared a brief paper on the camping circles of Siouan tribes, and in addition furnished an article on the modes of predication in the Athapascan dialects of Oregon and in several dialects of the Siouan family. He also edited the manuscript of the Dakota grammar, texts, and ethnography, written by the late Rev. Dr. S. R. Riggs. This will soon be published as Part 1, Volume VII, Contributions to North American Ethnology. In May, 1889, he began an extensive paper on Indian personal names, based on material obtained by himself in the field, to contain names of the following tribes: Omaha and Ponka, Kansa, Osage, Kwapa, Iowa, Oto and Missouri, and Winnebago.

Mr. Albert S. Gatschet's office work was almost entirely restricted to the composition and completion of his Grammar of the Klamath Language of Oregon, with the necessary appendices. The grammar and dictionary are now printed and will soon be published. The ethnography will follow.

During the year Mr. Jeremiah Curtin arranged and copied myths of various Indian families, and also transcribed Wasco, Sahaptin, and Yana vocabularies previously collected.

On his return from the Cheroki reservation in 1888, Mr. James Mooney began at once to translate a number of the prayers and sacred songs obtained from the shamans during his visit. The result of this work will appear in a paper in the seventh annual report of the bureau entitled "Sacred formulas of the Cheroki." Considerable time was devoted also to the elaboration of the botanic and linguistic notes obtained in the field. In the spring of 1889 he began the collection of material for a monograph on the aborigines of the Middle Atlantic slope, with special reference to the Powhatan tribes of Virginia. As a preliminary, about one thousand circulars, requesting information in regard to local names, antiquities, and surviving Indians, were distributed throughout Maryland, Delaware, Virginia, and northeastern Carolina. The information thus obtained affords an excellent basis for future work in this direction.

From July 1 to August 1, Mr. J. N. B. Hewitt was engaged in arranging alphabetically the recorded words of the Tuscarora-English dictionary mentioned in former reports, and in the study of adjective word-forms to determine the variety and kind of the Tuscarora moods and tenses. After his return from the field, Mr. Hewitt recorded and tabulated all the forms of the personal pronouns employed in the Tuscarora language. Studies were also prosecuted to develop the predicative function in the Tuscarora speech. All the terms of consanguinity and affinity as now used among the Tuscarora were recorded and tabulated. Literal translations of many myths collected in the fields were made, and free translations added to four of them. In all of these studies linguistic notes were made relating to etymology, phonesis, and verbal change.

Mr. James C. Pilling has, as usual, given all the time he could spare from his executive duties to the preparation of bibliographies of North American languages. The Bibliography of the Iroquoian Languages was completed early in the fiscal year and the edition was issued in February last. In the mean time a Bibliography of the Muskhogean Languages has been compiled, the manuscript of which was sent to the printer January 8, 1889, the first proof received Febuary 9, and proof-reading completed early in June. The edition, however, was not delivered during the fiscal year. Early in March, 1889, Mr. Pilling made a trip to Philadelphia to inspect the linguistic material, particularly the manuscripts, belonging to the American Philosophical Society. The library authorities gave him every facility, and much new material was secured. In June Mr. Pilling made a somewhat extended trip through New England States and into Canada, visiting the Astor, Lenox, and the Historical Society libraries in New York; the libraries of the Atheneum, Public, Massachusetts Historical Society, and the American Board of Commissioners for Foreign Missions, in Boston; that of Harvard University, in Cambridge; the American Antiquarian Society, in Worcester, and the private library of Dr. J. Hammond Trumbull, in Hartford. In Canada he visited the library of Laval University, and the private library of Mr. P. Gagnon, in Quebec, of St. Mary's College and Jacques Cartier School, in Montreal, and various missions along the St. Lawrence River, with a view of inspecting the manuscripts left by the early missionaries. In addition to these he visited many smaller institutions, private libraries, and publishing houses, and the result of the whole trip was the accumulation of much new material for insertion in the Algonquian bibliography. It is thought that the manuscript for this publication will be in shape to send to the printer before the close of the year 1889.

Mr. W. H. Holmes has continued to edit the illustrations for the Bureau publications, and has besides engaged actively in his studies of aboriginal archæology. He has completed papers upon the pottery of the Potomac Valley and upon the objects of shell collected by the Bureau during the last eight years, and he has others in preparation. As curator of Bureau collections he makes the following statement of accessions for the year: From Dr. Cyrus Thomas and his immediate assistants working in the mound region of the Mississippi Valley and contiguous portions of the Atlantic slope, the Bureau has received one hundred and forty-six specimens, including articles of clay, stone, shell, and bone. Mr. Victor Mindeleff obtained sixteen specimens of pottery from the Pueblo country. Other collections by members of the Bureau and of the Geological Survey are as follows: Shell beads and pendents (modern) from San Buenaventura, Cal., by H. W. Henshaw. Fragments of pottery and other articles from the vicinity of the Cheroki agency, N. C., by James Mooney. A large grooved hammer from the bluff at Three Forks, Mont., by Dr. A. C. Peale. A large series of rude stone implements from the District of Columbia, by DeLancey W. Gill. Donations have been received as follows: An important series of earthen vases from a mound on Perdido Bay, Ala., by F. H. Parsons. Ancient pueblo vases from southwestern Colorado, by William M. Davidson. A series of spurious earthen vessels, manufactured by unknown persons in eastern Iowa, by C. C. Jones, of Augusta, Ga. Fragments of pottery, etc., from Romney, W. Va., by G. H. Johnson. Fragments of a steatite pot from Ledyard, Conn., by G. L. Fancher. A series of stone tools, earthen vessels, etc., from a mound on Lake Apopka, Fla., by Thomas Featherstonhaugh. Fragments of gilded earthenware and photographs of antiquities from Mexico, by F. Plancarte. Fragments of gold ornaments from Costa Rica, by Anastasio Alfaro. Loans of important specimens have been received as follows: Articles of clay from a mound on Perdido Bay, Ala., by Mrs. A. T. Mosman. Articles of clay from the last mentioned locality, by A. B. Simons. Pottery from the Potomac Valley, by W. Hallett Phillips, by S. V. Proudfit, and by H. L. Reynolds. Articles of gold and gold-copper alloy from Costa Rica, by Anastasio Alfaro, secretary of the National Museum at San José.

Prof. Cyrus Thomas was chiefly occupied during the year in the preparation of the second and third volumes of his reports upon the mounds. It is probable that these will be finished during the present fiscal year. He also prepared a bulletin on the Circular, Square, and Octagonal Earth-works of Ohio, with a view of giving a summary of a recent survey by the mound division of the principal works of the above character in southern Ohio. A second bulletin was completed, entitled "The Problem of the Ohio Mounds," in which he presented evidence to show that the ancient works of the State are due to Indians of several different tribes, and that some, at least, of the typical works were built by the ancestors of the modern Cheroki.

Since his return from the field, Mr. H. L. Reynolds has been engaged in the preparation of a general map of the United States, showing the area of the mounds and the relative frequency of their occurrence. He has since assisted Professor Thomas in the preparation of the monograph upon the inclosures.

Mr. Victor Mindeleff, assisted by Mr. Cosmos Mindeleff, has been engaged in preparing for publication a "Study of Pueblo Architecture" as illustrated in the provinces of Tusayan and Cibola, material for which he has been engaged in collecting for a number of years. This report is now completed, and will appear in the Seventh Annual Report of the Bureau.

At the beginning of the fiscal year Mr. Cosmos Mindeleff and the force of the modelling room completed the bureau exhibit for the Cincinnati Exposition, and during the early part of the year Mr. Mindeleff was at Cincinnati in charge of the same. Owing to restricted space the exhibit was limited to the Pueblo culture group, but this was illustrated as fully as the time would permit. The exhibit covered about 1,200 feet of floor space as well as a large amount of wall space, and consisted of models of pueblo and cliff ruins; models of inhabited pueblos, ancient and modern pottery, examples of weaving, basketry, etc., a representative series of implements of war, the chase, agriculture, and the household, manikins illustrating costumes, and a series of large photographs illustrative of aboriginal architecture of the pueblo region, and of many phases of pueblo life. Upon Mr. Mindeleff's return from Cincinnati he resumed assistance to Mr. Victor Mindeleff upon a report on pueblo architecture, and the close of the fiscal year saw the two chapters which had been assigned him completed. They consist of a review of the literature on the pueblo region and a summary of the traditions of the Tusayan group from material collected by Mr. A. M. Stephen. Work was also continued on the duplicate series of models, and twelve were advanced to various stages of completion. Some time was devoted to repairing original models which had been exhibited at Cincinnati and other expositions, and also to experiments in casting in paper, in order to find a suitable paper for use in large models. The experiments were successful.

Mr. J. K. Hillers has continued the collection of photographs of prominent Indians, in both full-face and profile, by which method all the facial characteristics are exhibited to the best advantage. In nearly every instance a record has been preserved of the sitter's status in the tribe, the age, biographic notes of interest, and in case of mixed bloods the degree of intermixture of blood. The total number of photographs obtained during the year is 27, distributed among the following tribes, viz: Sac and Fox, 5; Dakota, 6; Omaha, 6, and mixed-bloods (Creeks), 10.

LIST OF PUBLICATIONS OF THE BUREAU OF ETHNOLOGY.

ANNUAL REPORTS.

First Annual Report of the Bureau of Ethnology, 1879-'80. 1881. xxxv, + 603 pp. 8vo.

Second Annual Report of the Bureau of Ethnology, 1880-'81. 1883. xxxvii, + 477 pp. 8vo.

Third Annual Report of the Bureau of Ethnology, 1881-'82. 1884. lxxiv, + 606 pp. 8vo.

Fourth Annual Report of the Bureau of Ethnology, 1882-'83. 1886. lxxiii, + 532 pp. 8vo.

Fifth Annual Report of the Bureau of Ethnology, 1883-'84. 1887. liii, + 564 pp. 8vo.

Sixth Annual Report of the Bureau of Ethnology, 1884-'85. 1888. lvii, + 675 pp. 8vo.

CONTRIBUTIONS.

Contributions to North American Ethnology, Vol. I. 1877. xiv, + 361 pp. 4to.
Contributions to North American Ethnology, Vol. III. 1877. 3. 635 pp. 4to.
Contributions to North American Ethnology, Vol. IV. 1881. xiv, + 281 pp. 4to.
Contributions to North American Ethnology, Vol. V. 1882. 112. 32. xxxvii, + 237 pp. 4to.

INTRODUCTIONS.

Powell, J. W. Introduction to the Study of Indian Languages. 1877. 104 pp. 4to.
Powell, J. W. Introduction to the Study of Indian Languages. 2nd ed. 1880. xi, + 228 pp. 4to.
Mallery, Garrick. Introduction to the Study of Sign Language. 1880. iv, + 72 pp. 4to.
Yarrow, H. C. Introduction to the Study of Mortuary Customs. 1880. ix, + 114 pp. 4to.
Mallery, Garrick. Collection of Gesture Signs and Signals. 1880. 329 pp. 4to.
Pilling, J. C. Proof-sheets of Bibliography of North American Indian Languages. 1885. xl, + 1135 pp. 4to.

BULLETIN.

Pilling, J. C. Bibliography of the Eskimo Language. 1887. v, + 116 pp. 8vo.
Henshaw, H. W. Perforated Stones from California. 1887. 34 pp. 8vo.
Holmes, W. H. The use of Gold and other Metals among the Ancient Inhabitants of Chiriqui, Isthmus of Darien. 1887. 27 pp. 8vo.
Thomas, C. Work in Mound Exploration of the Bureau of Ethnology. 1887. 15 pp. 8vo.
Pilling, J. C. Bibliography of the Siouan Languages. 1887. v, + 87 pp. 8vo.
Pilling, J. C. Bibliography of the Iroquoian Languages. 1888. vi, + 208 pp. 8vo.
Pilling, J. C. Bibliography of the Muskhogean Languages. 1889. v, + 114 pp. 8vo.
Thomas, C. The Circular, Square, and Octagonal Earth-works of Ohio. 1889. 35 pp. 8vo.
Thomas, C. The Problem of the Ohio Mounds. 1889. 54 pp. 8vo.
Holmes, W. H. Textile Fabrics of Ancient Peru. 1889. pp. 17. 8vo.

NECROLOGY.

JEROME H. KIDDER.

Dr. Jerome H. Kidder was born in Baltimore County, Md., on the 26th of October, 1842, and graduated in 1862 at Harvard, where he is still remembered as foremost in the gymnasium as well as on his class-rolls. He immediately then tendered his services for the war, and was placed in charge of the sea island plantations near Beaufort, S. C., where he contracted yellow fever, and was invalided home early in 1863; but upon recovery enlisted in the Tenth Maryland Infantry, in which he served as private and non-commissioned officer until the following year, when he was selected to be medical cadet, and in that capacity was employed in the military hospitals near the capital. During this time he was prosecuting the study of medicine, and in 1866 received from the University of Maryland the degree of M. D. In the same year he was commissioned an assistant surgeon in the U. S. Navy, becoming full surgeon in 1876.

Dr. Kidder's first duty was at Japan, where he quickly acquired the language of the country, and in other ways established the reputation which attached to him throughout his career for his "capacity for taking pains." While on this foreign service he was decorated by the King of Portugal in recognition of services to a distressed vessel of His Majesty's navy.

Dr. Kidder took part in observing the transit of Venus at Kerguelen Island, in 1874, as surgeon and naturalist of the expedition, and the excellent results of his scientific labors and researches therewith will be found described in the Bulletins of the U. S. National Museum. After the return of this expedition, Dr. Kidder arranged his specimens and collections in the Smithsonian Institution, and commenced those kindly and intimate relations with it which continued through his after life, with the regard of all his associates there.

In 1878 Surgeon Kidder married, at Constantinople, Annie Mary, daughter of the Hon. Horace Maynard, minister of the United States to Turkey, and in 1884, having inherited an adequate fortune, he resigned his commission and established his home in Washington, and here organized the bacteriological laboratory in connection with the Navy Museum of Hygiene, and also made a sanitary survey of the site proposed for the new Naval Observatory, while later he was appointed chemist of the U. S. Fish Commission, and in that capacity became one of the most trusted advisers of Professor Baird. His laboratory was in the Smithsonian building, and under the direction of the Secretary of the Institution he made, at the request of Congress, an exhaustive study of the ventilation of the Capitol and of the air in the Senate chambers and the hall of the House, and submitted an extended report or the use of the committees engaged upon the sanitary reform of the building. In 1887, after the death of Commissioner Baird, he served for a time as Assistant Commissioner of Fisheries, under Commissioner Goode. While connected with the Fish Commission he carried on a successful series of experiments to solve the problems relative to the temperature of living fishes, which have been made public through the reports of the Fish Commission. Besides the reports just referred to, Dr. Kidder contributed valuable papers to various professional and educational publications, and held for years a place on the literary staff of the New York World, and maintained membership in many learned societies. He was one of the founders of the Cosmos Club, and among the organizers of the Harvard Club in Washington, and a prominent member in the Masonic fraternity.

In 1888 Dr. Kidder accepted from the present Secretary the appointment of curator of laboratory and exchanges. His pleasant past relations to the Institution, and the esteem in which he was held by those connected with it, made the closer connection thus established agreeable to all; and the writer can not speak in too warm terms of the character of Dr. Kidder as shown in their business relations. His liberal education and views, served by the "capacity for taking pains" already referred to, were all under the control of the most conscientious regard for duty, and made him a valued administrator of the department under his charge. He knew how to maintain, together with exact order, the kindliest relations with all employed in it, who, it is safe to say, remember him with an affection and regard due to his excellent personal qualities, an affection and regard which the writer profoundly shares. Just in his best work, in his fullest physical vigor, Dr. Kidder was stricken with pneumonia, and died after a brief illness on the 8th of April, 1889.

His attachment to this Institution, which had always been of the peculiarly intimate character, was also shown in a bequest of which I shall elsewhere have to speak.

In conclusion, I can not but add to the statement of this great deprivation to the Institution an expression of my sense of personal loss in the parting with a friend who, in every relation of life, was a man as honorable and worthy of trust as any I have ever known.

JAMES STEVENSON.

In recording the death of Mr. James Stevenson, which occurred on the 25th of last July, I have to announce the loss of one of the most valuable as well as one of the oldest and most active collaborators of this Institution.

Mr. Stevenson was born in Maysville, Ky., in 1840, and while still little more than a boy, in the spring of 1857, ascended the Missouri River with the Warren Expedition; and from that time, with the exception of the interval caused by his acceptable services in the civil war, he annually and regularly visited the Rocky Mountain region, first under the auspices of the United States Exploring Expeditions of Warren and Reynolds, and latterly under that of the U. S. Geological Survey, of which he became the executive officer when that organization first took form, a position in which he remained up to the time of his death. His capacity and integrity were valued not only by the officials of the Survey, which he did so much in connection with, but by those of this Institution, for which during thirty years he gathered in remote regions specimens of natural history, geology, and ethnology, which are permanent testimonials of his enterprise and his industry.

During the season of 1885 he was engaged in making an extended search among the pueblos in the Moquis and Navajo districts of New Mexico, and in this elevated country he was stricken by the dreaded disease which lurks there. I met him in this region in 1887, when he was already aroused, though too late, to a sense of his danger, and am glad to recollect the circumstances of an acquaintance that associated him with the regions of the West, in which so much of his life had been passed, where so much valuable work was done, and where I had an opportunity to learn something of his fertility of resource in emergency and in the intimacy of camp life, of the amiable traits of his private character.

Mr. Stevenson's work was a double one, for he was equally at home in cities, and especially in Washington, where he was extensively known among members of Congress, and where the general confidence reposed in him by them was a deserved tribute not merely to his skill but to his personal integrity.

Respectfully submitted.

S. P. LANGLEY,
Secretary of the Smithsonian Institution.

APPENDIX TO SECRETARY'S REPORT.

APPENDIX I.

PUBLICATIONS OF THE YEAR.

SMITHSONIAN CONTRIBUTIONS TO KNOWLEDGE.

A memoir presented by Prof. Alpheus Hyatt, of Massachusetts Institute of Technology on the "Genesis of the Arietidæ," and recommended by Messrs. Alexander Agassiz, Charles A. White, and William H. Dall, was accepted for publication in the series of Contributions to Knowledge, in February last (1889). In order that the printing of the memoir might be under the convenient revision of the author, the work was placed in the hands of John Wilson & Son, of Cambridge, Mass. The printing of the treatise is well advanced, and it will probably be completed and distributed during the present year. It will form a volume of about 230 quarto pages, illustrated by 35 figures and 14 plates.

Two other publications of the year in the quarto size should be mentioned here, although not intended to be included in the collected volumes of the Contributions. No. 671 of the Smithsonian list is "Natural History Illustrations prepared under the direction of Louis Agassiz, 1849. The Anatomy of Astrangia Danae. Six lithographs from drawings by A. Sonrel. Explanation of the plates by J. Walter Fewkes." This issue represents merely a fragment of a memoir undertaken forty years ago by the eminent naturalist, Louis Agassiz, on material collected by him during his first dredging excursion in one of the steamers of the U. S. Coast Survey. This memoir, postponed by other occupations, was never completed, and even the original notes are no longer to be found. But the excellence of the drawings made under his direction from living specimens seems to warrant their publication, even at this late day. The text descriptive of the six plates, by Mr. Fewkes, occupies 20 quarto pages.

672. "Natural History Illustrations prepared under the direction of Louis Agassiz and Spencer F. Baird, 1849. Six lithographs from drawings by A. Sonrel. Explanation of the plates by David Starr Jordan." This, like the preceding, represents merely a fragment of a memoir projected by the joint labors of the two distinguished ichthyologists, and in like manner laid aside under the pressure of more immediate duties. The text explanatory of the six plates is comprised in 12 quarto pages. Were these two brochures more recent and more extended they would well deserve a place in the Smithsonian Contributions to Knowledge.

SMITHSONIAN MISCELLANEOUS COLLECTIONS.

Taking the various publications for the past year belonging to this series in the order in which they stand in the Smithsonian list, the first is:

No. 663. "Index to the Literature of Columbium, from 1801 to 1887." By Frank W. Traphagen. This is one of the special bibliographies of chemical literature published by the Institution on the recommendation of the committee appointed by the

American Association for the Advancement of Science, for the purpose of promoting such indexes. The present number forms an octavo pamphlet of 30 pages.

664. "Bibliography of Astronomy for the year 1887." By William C. Winlock. This is in continuation of the series of such bibliographies heretofore appended to the Regents' annual reports. It forms an octavo pamphlet of 63 pages.

665. "Bibliography of Chemistry for the year 1887." By H. Carrington Bolton. This is a similar continuation: an octavo pamphlet of 13 pages.

666. "Additions and Corrections to the List of Foreign Correspondents, to July 1888." By George H. Boehmer. Octavo pamphlet of 36 pages.

667. "Systematic Arrangement of the List of Foreign Correspondents to July, 1888." By George H. Boehmer. Octavo pamphlet of 55 pages.

675. "Report on Astronomical Observatories for 1886." By George H. Boehmer. (From the Smithsonian Report for 1886.) Octavo pamphlet of 119 pages.

683. "Report on Smithsonian Exchanges for the year ending June 30, 1886." By George H. Boehmer. (From the Smithsonian Report for 1886.) Octavo pamphlet of 30 pages.

684. "Miscellaneous Papers relating to Anthropology." (From the Smithsonian Report for 1886.) This collection comprises the following articles: "The Ray Collection from the Hupa Reservation." By Otis T. Mason. Thirty-five pages, with 26 plates. "A Navajo Artist and his Notions of Mechanical Drawing." By R. W. Shufeldt. Five pages with 3 plates. "Notes on the customs of the Dakotahs." By Paul Beckwith. Thirteen pages. "The Atnatanas, Natives of Copper River, Alaska. By Henry T. Allen. Nine pages. "Indians of the Quinaielt Agency, Washington Territory." By C. Willoughby. Sixteen pages with 7 figures. "The Stone Age of Oregon." By Myron Eells. Thirteen pages. "Charm Stones: Notes on the so-called 'plummets,' or sinkers." By Lorenzo G. Yates. Ten pages with 4 plates. "Studies on the Archaeology of Michoacan, Mexico." By Nicholas Leon. Twelve pages with 1 plate. "On some Spurious Mexican Antiquities, and their relation to Ancient Art." By William H. Holmes. Sixteen pages with 18 figures. "Earth-works at Fort Ancient, Ohio." By William M. Thompson. Three pages with 1 figure. Forming in all an octavo pamphlet of 132 pages, illustrated by 26 figures and 34 plates.

685. "On certain Parasites, Commensals, and Domiciliars, in the Pearl Oysters, *Meleagrinæ*." By Robert E. C. Stearns. (From the Smithsonian Report for 1886.) Octavo pamphlet of 6 pages with 3 plates.

686. "Time reckoning for the Twentieth Century." By Sandford Fleming. (From the Smithsonian Report for 1886.) Octavo pamphlet of 22 pages with 5 figures.

687. "Catalogue of Publications of the Smithsonian Institution; with a classified list of separate publications, and an alphabetical index of authors and subjects." By William J. Rhees. This work embraces all the articles published by the Smithsonian Institution from its organization, in 1846, to the 1st of July, 1886 (a period of forty years), and forms an octavo volume of 383 pages.

688. "Report upon International Exchanges, under the direction of the Smithsonian Institution, for the year ending June 30, 1888." By J. H. Kidder, curator. (From the Smithsonian Report for 1888.) Octavo pamphlet of 16 pages.

SMITHSONIAN ANNUAL REPORTS.

668. Report of Samuel P. Langley, Secretary of the Smithsonian Institution, for the year ending June 30, 1888. An octavo pamphlet of 126 pages.

676. Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution for the year ending June 30, 1886. Part I. This part, the report of the Institution proper, contains the Journal of Proceedings of the Board of Regents at the annual meeting held January 13, 1886, the Report of the Executive Committee of the Board of Regents, the report of Professor Baird, the Secretary of the Institution, with subsidiary report on

the exchanges for the year, and a list of additions to the number of foreign correspondents; followed by the usual "General Appendix," in which are given various anthropological papers, by Otis T. Mason, R. W. Shufeldt, Paul Beckwith, Henry T. Allen, C. Willoughby, Myron Eells, L. G. Yates, Nicholas Leon, William H. Holmes, and W. M. Thompson; also papers by Robert E. C. Stearns, Sanford Fleming, List of Astronomical Observatories, by George H. Boehmer, and Catalogue of Smithsonian Publications, by William J. Rhees—forming an octavo volume of xviii + 878 pages, illustrated by 31 figures in the text and 37 plates.

677. Annual Report of the Board of Regents of the Smithsonian Institution for the year ending June 30, 1886, Part II. This part relates to the U. S. National Museum (under the direction of the Smithsonian Institution), showing its progress and condition and containing: (1) Report of the Assistant Secretary of the Smithsonian Institution, G. Brown Goode, upon the condition and progress of the Museum for the year; (2) reports of the curators of the various departments of the Museum; (3) reports upon special collections in the Museum, and papers illustrative of the collections: the meteorite collection, by F. W. Clarke; the gem collection, by George F. Kuntz; the collection of building and ornamental stones, by George P. Merrill; the collection of textiles, fibers, and fabrics, by Romyn Hitchcock; preparation of microscopical mounts of vegetable textile fibers, by the same; and how to collect mammal skins for purposes of study and mounting, by William T. Hornaday; (4) Bibliography of the National Museum; and (5) list of accessions to the collections; followed by a general index. The whole forms an octavo volume of xi + 842 pages, illustrated by 23 figures and 20 plates.

PUBLICATIONS OF THE NATIONAL MUSEUM.

669. Proceedings of the U. S. National Museum, Vol. x, for 1887. This volume contains descriptive papers by Tarleton H. Bean, Charles W. Beckham, C. E. Bendire, Charles H. Bollmann, Ellsworth R. Call, E. D. Cope, Carl H. Eigenmann, Charles H. Gilbert, Theodore Gill, O. P. Hay, Elizabeth G. Hughes, David S. Jordan, F. H. Knowlton, S. R. Koelher, George N. Lawrence, Leo Lesquereux, W. Lilljeborg, Edwin Linton, Frederick A. Lucas, Jerome McNeill, Richard Rathbun, Robert Ridgway, R. W. Shufeldt, John B. Smith, Leonhard Stejneger, Charles H. Townsend, Frederick W. True, George Vasey, and José C. Zeledon. With a general index, this forms an octavo volume of viii + 771 pages, illustrated by 39 plates.

674. Bulletin of the U. S. National Museum, No. 33. Catalogue of Minerals and their Synonyms, alphabetically arranged for the use of museums. By T. Egleston. Octavo, 198 pages.

PUBLICATIONS OF THE BUREAU OF ETHNOLOGY.

670. Fifth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution. By J. W. Powell, Director. This contains the introductory report of the Director, 37 pages, with accompanying papers, as follows: Burial mounds of the northern sections of the United States, by Cyrus Thomas; the Cherokee Nation of Indians, by Charles C. Royce; the mountain chant, a Navajo ceremony, by Washington Matthews; the Seminole Indians of Florida, by Clay MacCauley; the religious life of the Zuñi child, by Mrs. Tilly E. Stevenson. The work forms a royal octavo volume of liii + 564 pages, including a general index, and is illustrated by 77 figures in the text and 23 plates, 8 of which are chromo-lithographs.

APPENDIX II.

REPORT OF THE CURATOR OF INTERNATIONAL EXCHANGES FOR THE YEAR ENDING JUNE 30, 1889.

WASHINGTON, D. C., *November* 20, 1889.

SIR: I have the honor to submit the following report of the operations of the exchange bureau for the fiscal year ending June 30, 1889. During the greater part of this time the bureau was under the charge of the late Dr. Jerome H. Kidder, whose able administration has contributed largely to its present efficiency. At the date of his death, April 6, 1889, Mr. Boehmer, upon whom the care of the office immediately devolved, reported that the exchange department, for the first time in its history, had disposed of all packages received, and was prepared to close its book accounts.

In continuation of the statistics usually presented, the following table exhibits in detail the exchange transactions for each month of the fiscal year:

Transactions of the exchange office of the Smithsonian Institution during the fiscal year 1888–'89.

	1888, July.	Aug.	Sept.	Oct.	Nov.	Dec.	1889. Jan.
Number of packages received	3,305	2,754	12,215	5,448	2,733	12,616	2,180
Weight of packages received (lbs.)	16,262	6,835	24,337	19,162	9,248	19,190	6,196
Entries made:							
Foreign	2,294	2,232	2,994	4,158	2,482	3,088	3,570
Domestic	2,098	1,028	1,342	1,532	1,896	1,590	4,002
Ledger cards:							
Foreign societies	4,194	......	......	......	......	4.339	......
Domestic societies	1,070	......	......	......	......	1,198	......
Foreign individuals	4,153	......	......	......	......	4,437	......
Domestic individuals	1,556	......	......	......	......	2,152	......
Domestic packages sent	2,001	787	1,063	1,410	1,647	1,664	1,328
Invoices written	459	199	1,889	2,034	341	788	795
Cases shipped abroad	16	33	81	73	24	71	27
Acknowledgments recorded:							
Foreign	908	934	572	791	700	708	594
Domestic	558	471	373	512	686	637	560
Letters:							
Recorded	86	87	109	115	93	72	127
Written	146	147	220	166	131	201	177

Transactions of the exchange office of the Smithsonian Institution during the fiscal year 1888–'89—Continued.

	1889. Feb.	Mar.	Apr.	May.	June.	Total.	Increase over 1887–'88.
Number of packages received	3,926	10,432	3,032	5,107	12,218	75,966	859
Weight of packages rec'd (lbs.)	12,233	18,972	9,931	12,002	25,560	179,928	30,298
Entries made:							
Foreign	4,560	4 304	3,006	5,186	8,268	46,142	5,994
Domestic	1,542	1,302	1,282	2,078	1,164	18,256	*5,254
Ledger cards:							
Foreign societies	......	......	......	......	......	4,466	331
Domestic societies	......	......	......	......	......	1,355	299
Foreign individuals	......	......	......	......	......	4,699	650
Domestic individuals	......	......	......	......	......	2,610	86
Domestic packages sent	1,293	1,426	971	2,575	1,053	17,218	4,917
Invoices written	886	1,371	886	985	3,462	14,095	570
Cases shipped abroad	40	96	51	57	124	693	30
Acknowledgments recorded:							
Foreign	491	389	542	424	387	7,440	*530
Domestic	472	757	345	928	583	6,882	2,074
Letters:							
Recorded	138	111	86	82	112	1 214	152
Written	143	225	161	102	231	2,050	246

* Decrease.

Or for comparison with the number of packages handled during recent years:

Packages.	1886–'87.	1887–'88.	1888–'89.
Received	52,218	75,107	75,966
Shipped:			
Domestic	10,294	12,301	17,218
Foreign	41,424	62,306	58,035

The small increase in the number of packages (859) received during 1888–'89 as compared with the preceding fiscal years, though offset by the large increase in weight (30,298 pounds), is accounted for by the fact that a number of regular shipments from Government bureaus were delayed beyond the close of the fiscal year.

EXPENSE.

From an examination of the books of the disbursing officer it appears that the actual cost of the exchange service for the year has been $17,152.10, divided as follows:

Salaries and compensation of employés	$11,479.25
Salaries of foreign agents (London and Leipzig)	1,500.00
Freight	2,555.23
Packing-boxes	586.20
Printing, postage, stationery, and miscellaneous	1,031.42
Total	17,152.10

Fifteen thousand dollars of this sum were appropriated directly by Congress or "the expenses of the system of international exchanges * * * under the direction of the Smithsonian Institution," $1,363.54[*] were repaid to the Institution by Government Departments to which specific appropriations had been granted for this purpose, leaving a deficit of $788.56, which was paid from the Smithsonian fund.

Although all of the Government bureaus that have occasion to transmit their publications through the Institution are not provided with funds available for defraying the cost of the service, it seems to have been the intention of Congress that its specific appropriation for the exchange business should be supplemented by special appropriations to some of the bureaus and departments of the Government, so that the charge of 5 cents per pound weight imposed by the regents in 1878 might be met by them. The average amount annually repaid to the Institution in this way during the past eleven years has been about $1,400.

Dr. Kidder strongly recommended, and I beg to renew his recommendation, that this procedure, for which sufficient reasons existed at the time of its adoption, may now be discontinued as no longer advantageous or economical. By the present system the cost of the service is actually larger than appears in the specific appropriations for exchanges, and, as the special appropriations to the different Departments vary from year to year and are often omitted altogether, a burden which can not be accurately foreseen, is imposed upon the Smithsonian fund.

In order to effect the change contemplated—that is, to collect in a single item the entire appropriation for international exchanges, and at the same time to make allowance for a proper compensation to the ocean steam-ship companies for freight, and to bring the schedule of salaries more nearly up to the standard established for the classified service of the Government—an estimate of $27,500 was submitted for the fiscal year 1889–'90.

This sum would then have been divided somewhat as follows:

Salaries		$16 600
Transportation:		
From Washington to seaboard	$2,280	
Ocean freight	5,000	
From point of debarkation to destination	1,750	
		9,030
Boxes		950
Incidentals		920
Total		$27,500

The amount finally appropriated was $15,000, no increase having been granted.

CORRESPONDENTS.

The number of correspondents has been increased during the year by 2,157, making the total number now upon our books 13,130, classified as follows:

	Foreign.	Domestic.
Societies and institutions	4,466	1,355
Individuals	4,699	2,610

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[*] The items in the report of the executive committee—$2,329.99 under the head of expenditures for exchanges, and $2,189.52 repayments—include receipts and expenditures made on account of the preceding fiscal year.

The geographical distribution is—

Country.	Establishments.	Individuals.
Africa	60	61
America:		
British America	106	250
Central America	14	24
Mexico	60	76
South America	152	148
United States	1,355	2,610
West Indies	24	65
	1,711	3,173
Asia	145	162
Australasia	130	95
Europe	3,766	3,802
Polynesia	9	16
Total	5,821	7,309

INTERNATIONAL EXCHANGE OF OFFICIAL DOCUMENTS, ETC.

The convention between the United States of America, Belgium, Brazil, Italy, Portugal, Servia, Spain, and Switzerland for the international exchange of official documents and scientific and literary publications, as well as the convention between the same countries (excepting Switzerland), for the "immediate exchange of the official journals, parliamentary annals and documents," was ratified by the President of the United States on July 19, 1888, but final ratifications were not exchanged by the representatives of the contracting powers until January 14, 1889. The convention was proclaimed on January 25, the day following, and since that date formal notification has been received of the adhesion to both conventions of the Government of Uruguay. The full text of these conventions was given in the Curator's report for last year.

The adhesion of the United States to the first of these conventions involves no new departure in the exchange service from the methods of previous years; but for the fulfillment of the obligations incurred by the second convention—the immediate exchange of official journals—an appropriation of about $2,000 to cover the necessary postage and additional clerical assistance is required, and provision should be made for the prompt delivery to the exchange office of the documents referred to.

This sum of $2,000 was estimated in reply to an inquiry made by the Secretary of State, dated February 12, 1889, as to the ability of the Smithsonian Institution to execute all of the provisions of the two conventions without further legislation by Congress, and the estimate was duly submitted by the Secretary of State in a letter to the President of the Senate, but no appropriation was made.

While the United States is thus bound by formal agreement to an exchange of its official publications with but eight countries, a full set of all publications received from the Government Printer is transmitted to forty-one countries upon the basis of mutual agreement.

A complete list of the official depositories for publications sent abroad during the fiscal year, in accordance with the act of Congress of July 25, 1868, with a statement of the number of packages sent and received from each of the countries represented, is contained in the annexed table:

Condition of parliamentary exchanges, 1888–'89.

Country.	Depository.	No. of publications—	
		Sent to.	Received from.
Argentine Republic	Minister of Foreign Affairs, Buenos Ayres.	553	....
Austria	I. and R. Statistical Central Commission, Vienna.	553	4,426
Baden	Minister of Foreign Affairs, Karlsruhe.	553	11
Bavaria	Royal Public Library, Munich	553	....
Belgium	Royal Public Library, Brussels	553	16
Buenos Ayres	Minister of Foreign Affairs of the Province of Buenos Ayres.	553	....
Brazil	Central Commission of Exchanges, Rio Janeiro.	553	....
Canada	Parliamentary Library, Ottawa	553	....
Canada	Legislative Library, Toronto	553	....
Chili	National Library, Santiago	553	....
Colombia	National Library, Bogota	553	....
Denmark	Royal Library, Copenhagen	553	....
France	Exchange Bureau, Paris	553	47
Germany	Library of the German Parliament, Berlin.	553	114
Great Britain	British Museum, London	553	10
Greece	United National and University Library, Athens.	553	....
Hayti	Minister of Foreign Affairs, Port-au-Prince.	553	....
Hamburg	City Government, Hamburg	....	21
Hawaii	Minister of Foreign Affairs, Honolulu.	....	68
Holland	Library of the Parliament, The Hague.	553	57
Hungary	President of the Hungarian Ministry, Budapest.	553	200
India	Secretary to the Government of India, Calcutta.	553	....
Italy	National Victor Emanuel Library, Rome.	553	122
Japan	Minister of Foreign Affairs, Tokio	553	....
Mexico	Minister of Justice and Public Instruction, Mexico City.	553	....
New South Wales	Parliamentary Library, Sydney	553	....
New Zealand	Parliamentary Library, Wellington	553	....
Norway	The Royal Government, Christiania	553	9
Peru	National Library, Lima	553	....
Portugal	Minister of Foreign Affairs, Lisbon	553	....
Prussia	Royal Public Library, Berlin	553	....
Queensland	Colonial Library, Brisbane	553	213
Russia	Imperial Public Library, St. Petersburg.	553	....
Saxony	Royal Public Library, Dresden	553	80
South Australia	Government, Adelaide	553	....
Spain	Government, Madrid	553	....
Sweden	Royal Library, Stockholm	553	32
Switzerland	Central Library, Bern	553	....
Tasmania	Parliamentary Library, Hobart Town.	553	3
Turkey	General Ottoman Library, Constantinople.	553	....
Venezuela	University Library, Caracas	553	....
Victoria	Public Library, Melbourne	553	355
Würtemberg	Royal Public Library, Stuttgart	553	652
	Total	22,673	6,442

The utter inadequacy of the return received by the United States, 6,442 volumes and pamphlets for 22,673 sent out, is but a repetition of the experience of previous years, and has been dwelt upon at length in former reports. The Austrian Government forms a notable exception to the general apathy of foreign nations in the matter, having transmitted 4,426 volumes, including complete and very valuable sets of Parlimentary Proceedings; and it is hoped that negotiations now in progress will result, in the near future, in a more equitable and satisfactory exchange with other nations, more especially with England and Germany.

If a complete account of all "governmental" exchange business carried by the Smithsonian Institution is made, that is, if all publications sent or received by the Government and its bureaus are included, it appears that 9,325 packages were received and forwarded to United States Government Departments, including the Library of Congress, while 25,671 were sent abroad through the exchange service from the same Departments. The apportionment among the different countries is shown below:

The United States Government, including Departmental Bureaus, in exchange with—	Number of publications—	
	Sent by the United States.	Received by the United States.
Africa	47	....
Argentine	1,192	89
Austria	753	5,059
Baden	553	11
Bavaria	553	....
Belgium	697	121
Brazil	796	152
British America	1,171	....
Chili	601	3
China	5	158
Colombia	572	....
Central America	76	179
Denmark	580	290
Ecuador	1	....
France	802	498
Germany	1,147	217
Great Britain	807	10
Greece	586	81
Hayti	553	....
Hamburg	....	21
Hungary	553	200
India	603	128
Italy	593	191
Japan	634	....
Mexico	667	....
Netherlands	579	115
New South Wales	579	52
New Zealand	591	....
Norway	684	12
Paraguay	3	....
Peru	562	....
Polynesia	12	68
Portugal	591	....
Prussia	553	....
Queensland	570	411
Roumania	6	77
Russia	627	1
Saxony	553	80
South Australia	568	....
Spain	553	....
Sweden	667	82

The United States Government, including Departmental Bureaus, in exchange with—	Number of publications— Sent by the United States.	Number of publications— Received by the United States.
Switzerland	561	3
Tasmania	553	3
Turkey	553	....
Uruguay	15	....
Venezuela	5[illegible]2	....
Victoria	622	355
West Indies	12	....
Würtemberg	53	658

EFFICIENCY OF THE SERVICE.

While a marked improvement appears to have taken place in the exchange service during the past few years, still further improvements are no doubt desirable and possible. The plan adopted by Dr. Kidder of following up promptly and diligently all complaints, or failures of packages to reach their destinations, has produced excellent results. The delays due to the fact that the Smithsonian Institution is dependent upon the generosity and public spirit of most of the ocean steam-ship lines for the free transportation of its exchange boxes will be provided against, if the appropriation asked for is granted by Congress. The delays which occur in some of the foreign bureaus, due to indifference or to insufficient clerical force, are at present beyond the control of the Institution. Where regularly paid agencies have been established, as in London and Leipzig, this cause of embarrassment to the service no longer exists, and all packages are transmitted with promptness.

Still another difficulty arises from an inadequate or erroneous address upon the packages, rendering it necessary for the agent to hold them until the error or omission can be corrected by correspondence. Increased attention to this point on the part of those who have occasion to send publications through the exchange service will assist materially in decreasing the number of delayed transmissions.

An important need of the exchange bureau is a more complete index to the early records, but with the present clerical force this additional work can not be effectually undertaken.

I take pleasure in bearing witness to the faithfulness and efficiency of the employés of the bureau, and to the prompt attention to the interests of the Institution of its foreign agents, Messrs. William Wesley & Son, at London, and Dr. Felix Flügel, at Leipzig.

The employés of the bureau receive much lower salaries than those established for similar grades of work by the classified lists of the Government Departments, and it is manifestly to the interest of the service to be able to retain, by reasonable expectation of promotion, men who have acquired peculiar and valuable experience in the exchange transactions.

Grateful acknowledgments are due the following transportation companies and firms for their continued liberality in granting free freight on exchange parcels and boxes:

Allan Steam-ship Company (A. Schumacher & Co., agents), Baltimore.
Anchor Steam-ship Line (Henderson & Brother, agents), New York.
Atlas Steam-ship Company (Pim, Forwood & Co., agents), New York.
Bailey, H. B., & Co., New York.
Bixby, Thomas E., & Co., Boston, Mass.
Borland, B. R., New York.

Boulton, Bliss & Dallett, New York.
Cameron, R. W., & Co., New York.
Compagnie Générale Transatlantique (A. Forget, agent), New York.
Cunard Royal Mail Steam-ship Line (Vernon H. Brown & Co., agents), New York.
Dennison, Thomas, New York.
Florio Rubattino Line, New York.
Hamburg American Packet Company (Kunhardt & Co., agents), New York.
Inman Steam-ship Company, New York.
Merchants' Line of Steamers, New York.
Muñoz y Espriella, New York.
Murray, Ferris & Co., New York.
Netherlands American Steam Navigation Company (W. H. Vanden Toorn, agent), New York.
New York and Brazil Steam-ship Company, New York.
New York and Mexico Steam-ship Company, New York.
North German Lloyd (agents, Oelrichs & Co., New York; A. Schumacher & Co., Baltimore).
Pacific Mail Steam-ship Company, New York.
Panama Railroad Company, New York.
Red Star Line (Peter Wright & Sons, agents), Philadelphia and New York.
White Cross Line of Antwerp (Funch, Edye & Co., agents), New York.
Wilson & Asmus, New York.

In conclusion, I beg leave to add a list of correspondents that courteously act as agents of the Institution for the transmission of exchanges, and also a copy of the rules of the exchange service, calling especial attention to the necessity of observing rules 3 and 8, which provide that all packages sent shall be carefully addressed, and that all packages received from the Smithsonian shall be promptly acknowledged upon the receipt form which will always be found inclosed therein.

LIST OF THE FOREIGN CORRESPONDENTS OF THE SMITHSONIAN INSTITUTION ACTING AS ITS AGENTS FOR THE INTERNATIONAL EXCHANGES.

Algeria: Bureau Français des Echanges Internationaux, Paris, France.
Austria-Hungary: Dr. Felix Flügel, 57 Sidonien Strasse, Leipzig, Germany.
Brazil: Commissão Central Brazileira de Permutaçœs Internaçionães, Rio Janeiro.
Belgium: Commission des Echanges Internationaux, Rue du Musee, No. 5, Bruxelles.
British America: McGill College, Montreal; or Geological Survey Office, Ottawa.
British Colonies: Crown Agents for the Colonies, London, England.
British Guiana: The Observatory, Georgetown.
Cape Colony: Agent-general for Cape Colony, London, England.
China: Dr. D. W. Doberck, government astronomer, Hong-Kong; for Shanghai, United States consul-general, Shanghai.
Chili: Museo Nacional, Santiago.
Colombia (United States of): National Library, Bogótà.
Costa Rica: Biblioteca Nacional, San José.
Cuba: Prof. Felipe Poëy, Calle del Principe Alfonso, No. 416 Havana.
Denmark: Kong. Danske Videnskabernes Selskab, Copenhagen.
Dutch Guiana: Surinaamsche Koloniaale Bibliotheek, Paramaribo.
East India: Secretary to the Governmént of India, Calcutta.
Ecuador: Observatorio del Colegio Nacional, Quito.
Egypt: Institut Egyptien, Cairo.
France: Bureau Français des Echanges Internationaux, Paris.
Germany: Dr. Felix Flügel, 57 Sidonien Strasse, Leipzig.

Great Britain and Ireland: William Wesley & Son, 28 Essex street, Strand, London.
Greece: United National and University Library, Athens.
Guatemala: Instituto Nacional de Guatemala, Guatemala.
Guadeloupe: (Same as France.)
Haiti: Sécrétaire d'état des rélations extérieures, Port au Prince.
Island: Islands Stiptisbokasáfn, Reykjavík.
Italy: Biblioteca Nazionale Vittorio Emanuele, Rome.
Japan: Minister of Foreign Affairs, Tokio.
Java: (Same as Holland.)
Liberia: Liberia College, Monrovia.
Madeira: Director-General, Army Medical Department, London, England.
Malta: (Same as Madeira.)
Mauritius: Royal Society of Arts and Sciences, Port Louis.
Mozambique: Sociedad de Geographia, Mozambique.
Mexico: Sr. Ministro de Justicia e Instruccion Pública, City of Mexico.
New Caledonia: Gordon & Gotch, London, England.
Newfoundland: Postmaster-General, St. Johns.
New South Wales: Royal Society of New South Wales, Sydney.
Netherlands: Bureau Scientifique Central Néerlandais, Leiden.
New Zealand: Colonial Museum, Wellington.
Norway: Kongelige Norske Frederiks Universitet, Christiania.
Paraguay: Government, Asunçion.
Peru: Biblioteca Nacionale, Lima.
Philippine Islands: Royal Economical Society, Manilla.
Polynesia: Department of Foreign Affairs, care of Capt. H. W. Mist, Honolulu.
Portugal: Bibliotheca Nacional, Lisbon.
Queensland: Government Meteorological Observatory, Brisbane.
Roumania: (Same as Germany.)
Russia: Commission Russe des Echanges Internationaux, Bibliothèque Impériale-Publique, St. Petersburg.
St. Helena: Director General, Army Medical Department, London, England.
San Salvador: Museo Nacional, San Salvador.
Servia: (Same as Germany.)
South Australia: Astronomical Observatory, Adelaide.
Spain: R. Academia de Ciencias, Madrid.
Sweden: Kongliga Sevenska Vetenskaps Akademien, Stockholm.
Switzerland: Central Library, Bern.
Tasmania: Royal Society of Tasmania, Hobarton.
Turkey: Bibliothèque Générale Ottomane, Constantinople.
Uruguay: Bureau de Statistique, Montevideo.
Venezuela: University Library, Caracas.
Victoria: Public Library, Museum, and National Gallery, Melbourne.

RULES FOR THE TRANSMISSION OF SCIENTIFIC AND LITERARY EXCHANGES.

1. Transmissions through the Smithsonian Institution must be confined exclusively to books, pamphlets, charts, and other printed matter sent as donations or exchanges, and can not include those procured by purchase.

The Institution and its agents will not knowingly receive for any address purchased books, nor apparatus and instruments, philosophical, medical, etc. (including microscopes), whether purchased or presented; nor specimens of natural history, except where special permission from the Institution has been obtained.

2. Before transmission, a list of packages, with the address on each package, is to be mailed by the sender to the Smithsonian Institution, when sent from the United States, or to the foreign agent of the Institution when sent from abroad. The Institution must be informed by mail of each sending on the day of transmission.

3. Packages must be legibly addressed and indorsed with the name of the sender.

4. Packages must be enveloped in stout paper, securely closed, and tied with strong twine.

5. No package to a single address is allowed to exceed one-half of one cubic foot in bulk.

6. Packages must not contain letters, or written matter.

7. Packages must be delivered to the Smithsonian Institution or its foreign agents free of expense.

8. Packages must contain a blank acknowledgment, to be signed and returned by the party addressed.

9. If returns are desired, the fact should be explicitly stated on the package.

10. Packages received through the agency of the Smithsonian Institution must be acknowledged without delay by mail.

11. The Institution assumes no responsibility beyond that of the delivery of the packages.

S. P. Langley
Secretary Smithsonian Institution.

Smithsonian Institution,
Washington.

Very respectfully,

W. C. Winlock,
Curator of Exchanges.

APPENDIX III.

REPORT ON THE LIBRARY.

SIR: I have the honor respectfully to submit my report on the work of the library during the year from July 1, 1888, to June 30, 1889.

The work of recording and caring for accessions has been carried on as during the preceding year, the entry numbers on the accession-book running from 182,060 to 193,430.

The following condensed statement shows the number and character of these accessions:

PUBLICATIONS RECEIVED BETWEEN JULY 1, 1888, AND JUNE 30, 1889.

Volumes:		
Octavo or smaller	1,002	
Quarto or larger	498	
		1,500
Parts of volumes:		
Octavo or smaller	5,556	
Quarto or larger	6,646	
		12,202
Pamphlets:		
Octavo or smaller	2,705	
Quarto or larger	473	
		3,178
Maps		473
Total		17,353

Of these accessions, 4,810 (namely, 441 volumes, 3,752 parts of volumes, and 617 pamphlets) were retained for use in the Museum library, and 521 medical dissertations were deposited in the library of the Surgeon-General's Office, U. S. Army.

The remainder were promptly sent to the Library of Congress on the Monday following their receipt.

Among the most important additions to the list of serials during the year may be specified the following publications:

American Angler.
American Field.
American Grocer.
Bollettino di paletnologia Italiana.
Cosmos (formerly "Les Mondes"), Paris.
Export Journal.
Forest Leaves.
Gazzetta Chimica Italiana.
Himmel und Erde.
Journal of American Folk-Lore.
Journal of the Gypsy Lore Society.
Journal of the Marine Biological Association of the United Kingdom.
Journal of the Society of Chemical Industry.
Life-Lore.
Manufacturer and Inventor.
Menorah.
Monatshefte für Chemie (published by the Vienna Academy of Sciences).
"Old New York."
Orientalische Bibliographie.
Pittonia.
Praktische Physik.
Recueil des Travaux Chimiques des Pays-Bas.
Reports from the laboratory of the Royal College of Physicians, Edinburgh.
Research.
Revue des Traditions Populaires.
Revista di Mineralogia e cristallografia Italiana.
Shooting and Fishing.
The Steamship.
Studies from the Museum of Zoology, University College, Dundee.
Victorian Naturalist.

The following universities have sent complete sets of all their academic publications for the year, including the inaugural dissertations delivered by the students on graduation: Bern, Bonn, Dorpat, Erlangen, Freiburg-im-Breisgau, Giessen, Göttingen, Halle-an-der-Saale, Heidelberg, Helsingfors, Jena, Kiel, Königsberg, Leipzig, Louvain, Lund, Tübingen, Utrecht, and Würzburg.

Among other important accessions during the year may be mentioned the following:

From the office of the secretary of state for India, London, a large series of Indian Government publications, including the final volumes (Vols. 12, 13, and 14) of the great Gazetteer of India, and Part 1 of the Catalogue of Sanskrit Manuscripts in the library of the India Office; full sets of official publications from the Italian Government, the Canadian Government, and the colonial government of New Zealand; from the Museum d'Histoire Naturelle at Lyons, the two magnificent works, Archéologie de la Meuse, by F. Liénard, in six large volumes, and Recherches Anthropologiques dans le Caucase, by E. Chantre, in five large volumes; Moeurs et Monuments Préhistoriques, from the author, the Marquis de Nadaillac; a further set of scientific papers from Prince Albert of Monaco; Catalogue des Monnaies Musulmanes de la Bibliothèque Nationale, from the National Library in Paris; Vol. 3 of the Reports of the German Commission for the Observation of the Transit of Venus; Vols. 26, 27, 28, 29, 30, and 31 of the *Challenger* Report (Zoology), from the British Government; from the Egypt Exploration Fund, the Memoirs on Tanis, Part II, The Store-City of Pithom, Naukratis, Part I, and The Shrine of Saft-el-Henneh, as well as a complete set, in duplicate, of all the memoirs published by this association, presented to the Institution as a return for its services in distributing the publications of the association in America; the first volume of the Fossils of the British Islands, presented by the delegates of the Clarendon Press, Oxford; a large volume of Memoirs on Whales and Seals, from the author, Sir William Turner, Edinburgh; a set of nineteen large volumes and pamphlets, catalogues of manuscripts, and special collections of books, from the Royal Library at Berlin; the third section of Vol. 2 of the great Corpus Inscriptionum Atticarum, from the same library; a series of fourteen catalogues of the various collections in the Royal Museum at Berlin; a complete file of the Zeitschrift für Ethnologie, from 1884 to date, from the Berliner Gesellschaft für Anthropologie, Ethnologie, und Urgeschichte; full sets of publications, including charts from the hydrographic offices of Great Britain, Denmark, Italy, and Russia; Vol. 1 of Expéditions Scientifiques du Travailleur et du Talisman, containing the fishes, by L. Vaillant, from the Bureau Française des Échanges Internationaux, which also sent a large series of other important publications of the French Government; a large series of government reports from the Hawaiian Government; Mean Scottish Meteorology, from the author, Prof. C. Piazzi Smyth; Part 5 of Lilljeborg's Sveriges och Norges Fiskar; and a gorgeously illustrated work from his highness the Maharaja of Ulwar, entitled Ulwar and its Art Treasures, by Thomas Holbein Hendley.

Very respectfully submitted.

JOHN MURDOCH,
Librarian.

Prof. S. P. LANGLEY,
Secretary of the Smithsonian Institution.

GENERAL APPENDIX

TO THE

SMITHSONIAN REPORT FOR 1889

ADVERTISEMENT.

The object of the GENERAL APPENDIX to the Annual Report of the Smithsonian Institution is to furnish brief accounts of scientific discovery in particular directions; occasional reports of the investigations made by collaborators of the Institution; memoirs of a general character or on special topics, whether original and prepared expressly for the purpose, or selected from foreign journals and proceedings; and briefly to present (as fully as space will permit) such papers not published in the "Smithsonian Contributions" or in the "Miscellaneous Collections" as may be supposed to be of interest or value to the numerous correspondents of the Institution.

It has been a prominent object of the Board of Regents of the Smithsonian Institution, from a very early date, to enrich the annual report required of them by law, with memoirs illustrating the more remarkable and important developments in physical and biological discovery, as well as showing the general character of the operations of the Institution; and this purpose has, during the greater part of its history, been carried out largely by the publication of such papers as would possess an interest to all attracted by scientific progress, so that the appendices of the annual reports, during the years down to 1880, have been almost wholly so occupied.

In 1880, the Secretary, induced in part by the discontinuation of an annual summary of progress which for thirty years previous had been issued by well-known private publishing firms, had prepared by competent collaborators a series of abstracts, showing concisely the prominent features of recent scientific progress in astronomy, geology, meteorology, physics, chemistry, mineralogy, botany, zoology, and anthropology. Other subjects which might properly have been included, such as those of terrestrial physics, hydrography, microscopy, etc., as well as the more practical topics of general technology, were omitted, both for want of time and want of space, so that from the outset the impracticability of a review of the whole field was recognized.

It has already been mentioned in the annual report for 1888 that these latter provisions seemed justified by further experience until in 1886, the incompleteness of the special record, the discouragements from the increasing delays encountered in the printing of these summaries, the recent multiplication by private enterprise of special books and periodicals devoted to critical summaries, and other considerations, induced a temporary suspension of the project; while it was added that with every effort to secure prompt attention to the more important details of the survey of the annual progress of scientific discovery, experience has shown that it is impracticable to obtain all the desired reports in each department within the time prescribed, that the plan attempted of bringing up the deficiencies in subsequent reports has not proved entirely satisfactory, and that in view of these delays, of the ever-increasing range of complexity of the subjects to be treated, and of other considerations, it is probable that it may be thought advisable to revert to the accustomed, and, it is believed, more widely acceptable plan of publishing yearly papers selected with a principal view to their general scientific interest, rather than to attempt the continuation of summaries chiefly of importance to the professional student.

The earlier established plan of the annual reports is followed in this volume, though not to the exclusion of such summaries as may be connected with the recognized fields of labor of the Institution.

THE NATIONAL SCIENTIFIC INSTITUTIONS AT BERLIN.

Prepared by Prof. ALBERT GUTTSTADT, M. D.*

Translated and condensed by GEORGE H. BOEHMER.

I.—THE ROYAL ACADEMY OF SCIENCES.

(*Physico-Mathematical Class.*)

The Royal Academy of Sciences was established in 1701 by King Frederick I, upon the solicitation of Leibnitz and received the name "Royal Society of Sciences;" the word Academy was substituted in 1744 on occasion of the reorganization of the society under Frederick the Great. The statutes, approved by royal decree of March 28, 1881, explain the object and composition of the society as follows:

The Academy of Sciences is a society of scientists whose object it is to promote science without being required to adhere to any plan of instruction. It comprises four classes of members, but in a more limited sense it is formed by the whole body of regular members, who, under the direction of the secretaries attend to the affairs of the entire academy.

The Academy possesses the rights of a privileged corporation, has its own seal, owns its premises, has its own funds and a regular, guaranteed income which it dispenses according to the adopted rules.

For the conduct of some of its affairs the Academy has formed two sections: the physico-mathematical and the philosophical-historical class. (Formerly four classes existed, but since 1830 they became united into two.)

Each section manages its own affairs. No difference of importance exists as regards the two sections.

The membership is formed of: (1) regular members, (2) foreign members, (3) honorary members, (4) corresponding members. The honorary members are not assigned to any special sections; all other members are assigned to the respective sections and can belong only to that section.

The seniority of ordinary and foreign members is regulated by the time of their election.

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* "*Die naturwissenschaftlichen und medicinischen Staatsanstalten Berlins,*" compiled as a memorial volume of the fifty-ninth meeting of the Association of German Naturalists and Physicians, by authority of Dr. von Gossler, minister of worship, education, and medical affairs. Berlin, 1886.

To ordinary membership only such persons are eligible as are residents of Berlin or live in places the connection of which with the national capital permits them to take part in their regular academic duties. Any such member removing to a place not provided for in the above is transferred to the number of honorary members.

Each class may have twenty-seven regular members. A number of these places is intended for certain specified branches of science; for the remaining places all scientists whose activity lies within that specified section may become eligible.

Vacancies among specialists may be left open, yet, the advantage of the academy requires all possible competition. In that case the class has to decide whether any of its members may be selected for the purpose. Applications for these places can emanate only from regular members.

A proposed class election is to be communicated to the presiding secretary of the academy and then considered by the entire academy at its next regular session when the candidate is elected by ballot.

The result of the election is to be communicated to the minister who obtains the king's approval.

If a scientist, non-resident of Berlin or of any of the places allowed for, receives the election of regular member he is required to remove to Berlin within six months of the date of his confirmation—which time may be increased in special cases. If he fails to comply with this rule he is enrolled among the honorary members.

The regular members are both permitted and required to share the labors of the Academy; they have a seat and a vote both in the general Academy and in the class, and are permitted to attend the meetings of either of the classes.

A member of twenty-five years standing or having reached the age of seventy may be relieved from lecturing or speaking.

The regular members are entitled to all privileges of the royal institutions and collections. They are furthermore privileged to lecture at any university of the Prussian domain and enjoy equal rights with the professors in accordance with regulations to which they are also bound with regard to the lectures.

With regard to salaries the following regulations are in force:

(1) Each of the fifty-four regular members of the Academy receive an annual salary of 900 mark ($225.).

(2) Separate salaries, additional to the above 900 mark are given to two regular members of the physico-mathematical class, of which one has to be a botanist and the other a chemist, and to two regular members of the philosophic-historical class, who are required to be philologists or historians. The salary of the chemist also includes the official dwelling in the building of the academy and the use, for scientific purposes, of any available room in the building not otherwise occupied. The payment of such a salary is made for special services required in the conduct of a certain office or professorship, or in the direction of a scientific institute.

(3) A special salary for special duties may be given by vote of the academy for such a time as may be required in the performance of special duties. The pensioning of the salaried officials is optional.

(4) The two salaries may be granted at once at the time of election, provided the proposition is made at the time of election of the candidate. This requires the sanction of the minister.

(5) The widow or, in her absence, the children of a deceased member continue to draw the salary of their husband or father for the term of one year, commencing with the day of his death.

Foreign (or non-resident) members are such as do not reside at Berlin or at one of the places provided for in the statutes. Of these each class has ten. The Academy is not required to fill vacancies in this number. The non-resident members enjoy all the rights of the regular members, and in case of any visit to Berlin, and upon notification of the fact to the general secretary, they receive invitations to the meeting, etc., the same as the regular members.

Honorary membership may be extended to such resident scientists as are prevented from fulfilling the obligations of regular membership; it may further be extended to non-resident and foreign scientists who have excelled in scientific pursuits and in some way have given evidence of their interest in the welfare of the Academy. There is no limit to the number of honorary members.

The honorary members are entitled to participate in the meetings of the Academy of which they are, in each case, informed by invitation. They are at liberty to make scientific communications and to take part in the deliberations of business affairs.

The corresponding members are composed of scientists, non-residents of Berlin. They retain the corresponding membership in the event of their locating at Berlin. Each class offers one hundred places for corresponding members.

The corresponding members are entitled to take part in the public and other meetings of the Academy and of the class to which they, respectively, have been assigned and to make scientific communications. They are also permitted to be present at business meetings, but have no vote in the same.

The business of the Academy is conducted by four permanent secretaries, of which each class furnishes two.

The secretaries are elected for life and draw a salary of 1,800 mark annually, which amount is also paid to the surviving widow or orphans for the period of one year succeeding the death of the incumbent.

The secretaries range according to the seniority of their election.

Each of the secretaries carries a seal.

Each of the two classes elect their secretaries out of their own members and in secret session. The election has to receive the King's sanction.

It is the duty of the secretaries to conduct the business of the Academy and to execute its orders. The manner of arranging the duties among their number is left to their discretion.

In the presidency and the duties of that office and in the conduct of the affairs of the general Academy, the secretaries change successively every four months in accordance with their seniority, unless they arrange among themselves for some other method of succession. In case of enforced absence of the presiding secretary the last secretary has to take his place. All four secretaries being unavoidably detained from presiding, the senior of the ordinary members assumes the office.

The business secretary is styled the presiding secretary; he carries the great seal of the Academy and supervises the officials and clerks of the Academy. He calls the extraordinary meetings of the members and the meetings of the secretaries; he issues the invitations and presides at all meetings; in case of a tie his vote is decisive; he signs the protocols and arranges for the execution of the various resolutions. He has charge of the correspondence of the Academy, opens all communications, submits them and then takes charge of further action. He is responsible for the observance of the statutes, and for that purpose communicates directly with the minister. In submitting his charge of four months he has to surrender to his successor a complete inventory made in the presence of the archivist.

The presiding secretary, or his substitute, is the only person permitted to institute legal proceedings in the name of the Academy, for which purpose he may receive special identification on the part of the ministry. Money may be paid to the cashier of the ministry.

Within the classes of the Academy the respective secretaries assume the presidency and the execution of all business affairs for the term of from four to four months.

The regular salaried officials of the Academy—at present an archivist, one clerk, one door-keeper and one messenger—are appointed for life or any specified term in general session and by recommendation of the college of secretaries. The appointments have to receive the approval of the ministry.

The following rules are in force regarding meetings, labors, and publications:

The members participate in the meetings according to the rights of their respective grade. Others, not members, may be permitted to attend the scientific meetings; they have to be recommended by a member and introduced to the presiding secretary.

The meetings of the Academy are held every Thursday and alternate with those of the entire Academy and by those of the classes.

At each regular meeting a scientific paper is to be read by one of the regular members, at the expiration of which other members are permitted to make scientific communications or in any way to introduce scientific objects.

The general Academy is empowered to submit questions to the secretary of the respective classes for action or report; or a special commission or commissioner may be appointed for report on some scientific or business question; the appointment of such commission is made by ordinary election, or, if required, by secret ballot.

The general Academy holds three public meetings annually; the one on July 1 in memory of Leibnitz, its first president, a second one on January 24 in commemoration of the birth of Frederick II, the re-organizer of the Academy, and the third on the birth-day of the reigning King. If these days do not fall upon Thursday, the succeeding Thursday is set aside for such public meeting.

The secretaries alternate in the conduct of the presidency on these special occasions, and the presiding officer is required either to make mention of the occasion by a few introductory remarks or to read a special paper on the subject.

In the meetings held in memory of Leibnitz, regular members, elected during the year, make their first speech or deliver their first lecture, each being responded to by one of the secretaries. Eulogies of deceased members are read during the course of the meeting. The business of the public meeting consists of the announcement of prizes, the reading of annual reports on the changes in the personnel, and of other papers explanatory to the works and results of the scientific enterprises or foundations connected with the Academy. Papers read in regular session may, upon consent of the Academy, be read again in these public meetings.

In accordance with the intention of its foundation, it is the duty of the Academy to render assistance to the scientific enterprises of its members or scientists generally which require combined activity of several scientists, or which, on account of their compass or expense, would require the assistance of the Academy. A further duty of the Academy requires it to manage foundations of a strict scientific character, and to encourage or reward, by the giving of prizes, investigations, or researches in certain defined directions.

The Academy publishes "Sitzungsberichte" and "Denkschriften," the editing of which devolves upon the college of secretaries, subject to regulations adopted by the entire Academy. The members receive copies, beginning with the year of their admission.

Explicit permission of the Academy or one of its classes is absolutely required for the publication in the academic proceedings of any scientific paper. The request for publication must be accompanied by the ready manuscript, and the proposition may be voted on at once. If the expense or any other important point should require a further consideration a commission may be appointed for the purpose, or the subject may be referred to the board of secretaries or to one of the classes of the Academy.

Upon the request of one of the members present the acceptance for publication of any paper or any proceeding connected therewith may be voted on by secret ballot.

LIST OF PUBLICATIONS OF THE ROYAL ACADEMY OF SCIENCES.

Miscellanea Berolinensia, t. 1–7, Berlin, 1710–1743, 4to.
Histoire de l'Académie Royale des Sciences, t. 1–25, années 1745–1769; *ibid.*, 1746–1771, 4to.
Nouveaux Mémoires, années 1770–1786; *ibid.*, 1772–1788, 4to.
Mémoires, années, 1787–1804; *ibid.*, 1792–1807, 4to.
Sammlung der deutschen Abhandlungen, 1788–1803; *ibid.*, 1793–1807, 4to.
Abhandlungen, 1804–1885; *ibid.*, 1815–1886, 4to.
Bericht über die zur Bekanntmachung geeigneten Verhandlungen, 1836–1855; *ibid.*, 1836–1855, 8vo.
Monatsberichte aus den Jahren 1856–1881; *ibid.*, 1856–1881, 8vo.
Sitzungsberichte der k Preuss. Akademie der Wissenschaften zu Berlin, 1882 ff; *ibid.*, 1882–1886, 8vo.
Mathematische und naturwissenschaftliche Mittheilungen aus den Sitzungsberichten der k. Preuss. Akademie der Wissenschaften zu Berlin, Jahrg, 1882 ff., Berlin, 1882–1886, 8vo.
Astronomisches Jahrbuch für das Jahr 1776; *ibid.*, 1774, 8vo. The same for 1777–1829; *ibid.*, 1775–1826, 8vo.
Berliner astronomisches Jahrbuch für das Jahr 1830; *ibid.*, 1828, 8vo. The same for 1831–1867; *ibid.*, 1829–1865, 8vo.

The regular annual revenues of the Academy consist of (1) The income from its own endowments; (2) dotation of 62,229 mark ($15,587) given as an annual revenue in place of the endowment of King Frederick William III (Royal decree of August 16, 1809); (3) assistance by the government; (4) its own profits.

The expenditures are: (1) Payment of salaries and remunerations; (2) prizes, publications of the Academy, care and increase of the library, all rendered domestic expenses required, including heating, lighting, and repairs; (3) for scientific purposes. With regard to these all possible equality should be secured for each of the two classes.

Any surplus may be added to the income for the coming year or added to the principal.

The funds available during the years 1886–'87 amounted to 208,982 mark ($52,245.)

The scientific proportion was 194,695 mark ($48,674), of which the following disbursements were made: (1) Salaries, 111,600 mark ($27,900); (2) real expenses, 83,095 mark ($20,774,) as follows: (*a*) Publication of Abhandlungen und Sitzungsbericihte, 23,800 mark ($5,950), (*b*) assistance to scientific enterprises, 53,000 mark ($13,250); (*c*) prizes 3,295 mark ($824); (*d*) increase of the library, 3,000 mark ($750.)

The expenses of administration were 14,287 mark ($3,572) of which 5,715 ($1,429) were for personal, and 8,572 mark ($2,143) for essential expenses.

The financial management provides, as far as possible, an equal share for each of the sections, Separate accounts are kept only with regard to items 2 *b* and 2 *c*. The physico mathematical class may annually dispense over 22,900 mark ($5,725) under 2*b*, and 2,425 mark ($606) under 2*c*. From this amount the class has to meet each eighth year the academic prize of 5,000 mark ($1,250), and in intervals of twelve, twelve, and two years, respectively, the prizes of 2,000 mark ($500) on account of the Eller legacy, 2,000 mark ($500) on account of the Cothenius legacy, and 1,800 mark ($450) on account of the Steiner legacy.

The physico-mathematical class has the benefit of the interest resulting from the "Humboldt Stiftung für Naturforschung und Reisen." This endowment, founded by collections after the death of Alexander von Humboldt, received the royal sanction by decree of December 19, 1860; its management rests in the hands of a special curatorship, and is intended to assist prominent talent of all nations in the direction pursued by Alexander von Humboldt himself and to give pecuniary assistance to workers on natural sciences and in the execution of expeditions.

The following enterprises have been assisted thus far from the interest of the capital to the amounts specified in each case:

Journey of Dr. Reinhold Hensel to the La Plata regions for the purpose of collecting fossil remains (1863–1865, and publication in 1867), 30,657 mark ($7,664).

Expedition of Dr. Georg Schweinfurth for the botanical exploration of the south-western Nile regions (1868–1871), 33,600 mark ($8,400).

Continuation of Prof. Reinhold Buchholtz's zoological exploration of Cameroons (1872), 6,450 mark ($1,612).

J. M. Hildebrandt, expeditions in east Africa and Madagascar in 1876–1877. Allowance, 14,500 mark ($3,625).

Dr. Karl Sachs, journey to Venezuela in order to study the electric eel. Allowance, 1876–1877, and for publication in 1881, 14,500 mark ($3,625).

Dr. Otto Finsch, journey for scientific investigations in Mikra and Melanesia. Allowance, 1878–1883, 36,550 mark ($9,138).

Prof. Gustav Fritsch, journey to Egypt for investigation of electric eel. Allowance, 9,000 mark ($2,225).

Dr. Eduard Arning, journey to the Sandwich Islands for the study of Lepra. Allowance, 1883–1884, 10,000 mark ($2,500).

Continuation of Dr. Paul Guessefeldt's travels in the Chilian Andes, 1883. Allowance, 6,000 mark ($1,500).

Journey of Prof. Georg Schweinfurth in Egypt for the geological exploration of the Arabian desert, 1884. Allowance, 5,000 mark (1,250).

II.—THE ROYAL FREDERICK WILLIAM'S UNIVERSITY.

The document establishing the University was executed by King Frederick William III, at Königsberg, in Prussia, on the 16th of August, 1809.

By the treaty of Tilsit, on July 9, 1807, Prussia had been deprived of a considerable portion of its domain and the territory of the King restricted to about 5,000,000 inhabitants.

By a short but impressive proclamation of July 24, 1807, the King relieved his subjects beyond the river Elbe from fealty. In that proclamation he says: "Inhabitants of those beloved and trusted provinces, realms, and towns, you are well acquainted with my views and with the events of the last year. My army was conquered and peace had to be concluded under the best possible conditions. The tie of love and confidence of centuries is to be severed. Fate decided, and the father separates himself from his children, but no fate, no power may extinguish your memory in my heart." Strong and sincere, too, was the love of the inhabitants of those surrendered provinces for their king. Deep was the sorrow in Halle and among the members of the university which Napoleon had dissolved on the 20th of October, 1806. A deputation, consisting of Schmalz and Froriep was sent to Memel, and in their name and that of their colleagues requested the King, in a petition of August 22, 1807, to consider the establishment of a scientific institution at Berlin. Hufeland, present in Memel, supported the wishes of the delegation.

On September 4, 1807, the King issued an order to Privy Councillor Beyme to the effect that, in view of the loss by the state of the University of Halle owing to the surrender of the domain west of the river Elbe, one of the most important and perfect educational establishments had ceased to exist and that it should be one of the first duties of the government, in the consideration of a reorganization of the state, to provide for the erection of some such establishment in the best possible manner; that the universities at Frankfort and Königsberg were not adapted to compensate for the loss, the former on account of the insufficiency of local auxiliary means and the latter on account of its great distance from the national capital; that Berlin, however, combined all the means adapted for such an educational establishment with the least possible expense and with the greatest possible advantage for its usefulness. In view of these facts the establishment at Berlin of such an institution in connection with the existing Academy of Sciences was decided upon. All the funds which had formerly been devoted to the support of the university at Halle were to be employed for the purpose, and Privy Councillor Beyme was instructed to secure for the new university the services of the prominent professors of Halle before other chances were offered to and accepted by them.

Frederick the Great, in his endeavors to re-model and re-organize the state, was not in position to do much towards universities; he was satisfied with having restored the proper rank to the highest representative of science, and only occasionally he alluded to the need of high schools. On April 7, 1784, he wrote to Frankfort that "the students should receive such instructions as to enable each of them to learn something useful so as to be enabled to render efficient service to state or church, since he thought more of this than of any formalities." All other care he left to his minister, von Zedlitz, who himself had become a pupil of the great Kant. The two universities, Königsberg and Halle, received prompt attention; Forster and Wolff had been appointed, and the necessity of a fixed plan of instruction had repeatedly been pointed out. This being most noticeable in the study of law, a regular schedule was prepared in 1771. In 1787 the higher educational council took the place of the board of curators of universities. The "General Laws for the Royal Prussian Universities, February 23, 1796," provide that applications for public positions can be entertained only of persons who have graduated from the university.

On January 8, 1803, Minister von Massow submitted his report on a suitable arrangement of the universities, in the preface of which he says: "It has long been a recognized fact that the universities, considered as establishments of education or at least of instruction, should be improved and arranged in a way conforming to their principal object. In order to realize such project and to remedy the abnormal conditions, remnants of gray antiquity, two principal obstacles have to be overcome, namely, the dominant character of the scientific man in his one-sidedness (partiality), and the want of funds; the improvement of their own financial condition will have to be the means to overcome their obstinacy."

A number of reformatory orders were issued during the following years: A royal order of April 7, 1804, fixed the academic term at three years. This was made public by circular letter of October 12, 1804, of the minister of justice, who adds that the candidates for promotion could be examined only upon proof of their having completed the prescribed course of studies. A further order of November 27, imposes that condition on all aliens or foreigners, who were applicants for positions requiring academic education.

The want of sufficient means too was a source of great complications. The amounts which, at the time of the establishment of the universities, had been ample, now barely covered the most urgent necessaries; the budgets were insufficient to permit even an approach towards securing the requirements demanded by progressing science. Since the equipment of Halle the grants, by the State, to all universities had been but very small, Frederick William II, during the eleven years of his reign, having been able to spare but 12,270 thaler ($10,200,) for the combined needs of all the universities, and their number having increased to nine in 1802 the vital question of their existence demanded an early settlement.

A commission appointed for the purpose, decided on the abolishment of a portion of the antiquated establishments. Frankfort's income was increased from 12,846 to 15,314 thaler ($9,635 to $11,485); Erlangen from 30,000 to 57,768 florins ($12,857 to $24,757); Halle from 18,116 to 36,113 thaler ($13,587 to $27,085). Means of instruction were provided and collections purchased for Frankfort and Königsberg. Halle was enriched by the appointment of professors of repute, and the salaries were increased by allowances from the royal treasury. In 1805 the number of students had increased at Frankfort to 307, Königsberg to 333, and at Halle to 944.

The surrender required by the treaty of Tilsit of the Halle University, notwithstanding the oppressed condition of the State, demanded immediate action with regard to the establishment of a new university which could then be commenced in conformity with the experiences gained by the long-continued inquiries into an improved organization.

Although it can not be established by documentary evidence, it is sufficiently well known that long before the unfortunate events of 1806 the cabinet of Frederick William III had already considered the advisability of founding a university at Berlin. Privy Councilor Beyme therefore was well informed on the subject when in 1807 he was requested to formulate plans for the establishment and organization of a university at the capital of the Kingdom. It may even be positively asserted that it was Beyme himself with whom the project originated.

He was deeply interested in universities; he was an adherent and friend of Fichte; he had induced the King to grant him asylum at Berlin; he was instrumental in having him appointed at Erlangen, and was responsible for the appointments of Schleiermacher and Steffens to Halle

At a large number of scientific institutions, established at an earlier day—the Academy, the Military Academy for Officers, the Artillery and Engineers' School, the Military Cadet Establishment, the Mining School, etc.—scientifically educated teachers were employed. The science of medicine was the best provided for. The "Collegium medico-chirurgicum" represented a medical faculty for the education of young army physicians. In 1806 the staff of that establishment was formed by twenty professors (eighteen regular and two assistant).

Since Frederick's time lectures were held on other scientific subjects for purposes of practical instructions; thus, on law and legal proceedings in the department of justice, and in forestry and technology by the general directory. TheAcademy of Arts instituted courses of lectures for the development of the artistic taste. There were high schools conducted by teachers of repute, some of them members of the Academy. Since however an intermediate step was wanting between the two, greater demands were made on both teachers and pupils which elevated them almost to the dignity of an university. There were further, the library, the botanical garden, the observatory, the natural history collection of the academy, the collections of the mining and smelting department, the anatomical theater, the collection of physical, astronomical, and chirurgical apparatus, the royal and the academic coin collections and the picture gallery in the royal castle.

Since the beginning of the reign of Frederick William III additions had been made to the number of the existing older establishments. In 1798 the Eschke Institute for deaf-mutes was enlarged from means furnished by the royal treasury; in 1799 the Academy for Architects was founded and the military establishment enlarged; in 1803 attempts were made for the improvement of military education, and lectures were instituted for artisans. In 1804 the academy for young officers was founded, in 1805 the statistical bureau, and in 1806 the Institute for the Blind and the Agricultural Institute. All branches of knowledge were cared for.

Soon after Fichte's appointment Jena furnished a second celebrated teacher. In 1800 Hufeland was appointed professor and director of the "Collegium medico-chirurgicum." The academy elected him to membership; the medical affairs were intrusted to him, and in his position as physician to the King opportunities were not wanting which enabled him to render decisions on scientific questions.

For the improvement in agricultural knowledge Thaer was called in 1804, and he at once prepared for the establishment of his "Agricultural Institute."

Alexander von Humboldt, upon the return from his expedition around the world, on September 3, 1804, declared his intentions to enter the services of the state. It was about this time that Beyme expected to organize the new establishment. The programme of Göttingen—or rather the spirit of the programme freed from all abuses,—a general scientific educational establishment, was his plan for the Berlin University.

Owing to the threatening conditions, however, the project was not consummated. Soon crushing blows demoralized the state; on October 27, 1806, Napoleon entered the city of Frederick the Great.

After the treaty of Tilsit, when the King and his council prepared the organization of the great reform, the plan for the new university formed one of the points under consideration and this gave rise to a multitude of opinions, objections, and deliberations.

During all these deliberations the patriotism and the scientific zeal of the professors who had already received their commissions, had been demonsrated; they had entered on their course of lectures. The University existed, although not by official recognition. It was formed by the four professors: Schleiermacher, Schmalz, Fichte, and Wolf, each of whom represented a faculty.

On December 3, 1808, the French evacuated Berlin. Among the changes which took place in the national administration was the appointment of Wilhelm von Humboldt to take charge of the public instruction.

In April, 1809, Humboldt left for Königsberg in order to personally urge before the King a final determination. A building became necessary, both in order to enable the professors to appear as public teachers and to secure an appreciation of the scheme by the inhabitants. The palace of Prince Henry had been repeatedly suggested for the purpose. Frederick the Great had constructed it during the years 1754-1764, and by death it had reverted to the crown. Frederick William III gave favorable consideration to Humboldt's wishes and donated the palace "for all time to come."

Greater difficulties appear to have presented themselves in obtaining security for the required means. Hufeland in 1807 already had shown the desirability of endowment by real estate; Humboldt shared his views and endeavored to gain the annual means by obtaining donation in the form of private domain belonging to the crown. In his memorial on the subject, of July 24, 1809, Humboldt says: "It may appear strange that the section of public instruction at the present moment of time ventures to advocate a plan, the execution of which would lead one to the supposition of quieter and happier days." He continues by saying that only such high educational establishments as the Berlin University is intended to be, can exert an external influence, and that by such a foundation the King would be instrumental in combining with him firmly everyone throughout Germany interested in education and enlightenment; he would instill new zeal for the rejuvenation of his realm and offer to German science a never-hoped-for asylum at a time when part of Germany had been destroyed by war and another part was governed over by a stranger. Thus the patriotic ideal became prominent in the plan for the foundation of the new university.

Wilhelm von Humboldt finally recommended formally its foundation at Berlin, to bear the time-honored name of University, since the nature of things requires a division of scientific institutes into schools, universities, and academies. He asked in the name of the University for a fund of 60,000 thaler, ($45,000,) and for the two academies—the Academy of Sciences and the Academy of Arts—a fund of 4,000 thaler ($3,000) additional to their present means.

By order of August 16, 1809, the King proclaimed that he considered the plan for higher education within and without the limits of the realm of such importance as to prohibit any further delay in the foundation of a University at Berlin which should be endowed with the privilege to confer academic honors.

An annual amount of 150,000 thaler ($112,500,) was granted to all the scientific establishments at Berlin, and the palace of the late Prince Henry deeded under the name of the "University building."

At the time of financial trouble the establishment of a new university presented a grave economic problem. The King however did not withhold his private fortune to aid the state or the people. The gold plate was withdrawn from the royal tables and coined and the remains were sold.

At last the stage for the settlement of the question of internal administration was reached. A royal decree of May 30, 1810, appointed a commission for the purpose.

The appointment of professors continued at salaries averaging between 1,200 to 1,500 thaler ($900 to $1,125), with from 200 to 500 thaler ($150 to $375) added for travelling expenses.

On September 22, 1810, the section of public instruction submitted to the King its final report, in which it was stated that "Thus this important institute has been opened in accordance with the will of your Majesty, and the section recognizes with respectful thanks the powerful protection and grateful privileges accorded the university to which alone it owes its rapid and healthy establishment. For among all the renowned universities of Europe there is not one possessed of such a number of tried teachers, with such scientific means, and with such splendor in building."

Indeed, it was an important moment when the section submitted the first programme of the lectures which contained such celebrated names. It was the often-promised seed which at last was being sown.

The staff was formed of fifty-eight teachers, of which twenty-four were regular professors, nine secondary professors, fourteen private lecturers, six members of the academy, and five teachers of modern languages. One hundred and sixteen courses of lectures were announced, of which the theological faculty had ten, the medical thirty-four, philosophical sixty-two, and the faculty of law ten.

The general specialties of science were pretty well represented; the introduction of German antiquity as a subject for historic philological study was new. Heindorf, in his introductory address, impressively urged on the students the expectations held of them, for the improvement of the newly created university.

By order of September 28, Schmalz was nominated rector, and Schleiermacher, Biener, Hufeland, and Fichte, deans. On October 1 the section requested the rector to begin matriculations, and on October 6 this act was performed on six students.

On October 10, 1810, upon the invitation of the rector, the first assembly took place, at the university building, consisting of sixteen professors. It was opened by the rector with an address; in place of oath of office he bound the professors to this duty by pressure of hand, whereupon the senate of the university was declared constituted.

The senate ruled that each faculty should confer honors upon the graduates, and that the use of the lecture-rooms was to be arranged according to a compensating table. The lectures were set to begin on October 19, to which general rule, however, exceptions were permitted, thus Hufeland commenced his lectures and the instructions at the policlinical institute on the 15th, Gräfe on the same day, Fichte on the 21st of October, while a few did not commence until the beginning of November.

As an external mark of distinction the following epigram was proposed by Wolf:

"Universitati Litterariæ Fridericus Guilelmus III rex. A. CIↃIↃCCC VIIII." It was recommended by Battemann and sanctioned by the King.

A change in the administration occurred at about the time of the opening of the university. By decree of November 20, 1810, a new president was appointed for the department of public instruction, and the decree was communicated to all German universities. The acts of the Berlin University begin with it. It stated: "You will be convinced yourself of the importance which the department of worship and public instruction now intrusted to your keeping exerts upon the welfare of the state and its inhabitants, even upon that of humanity. The object which the section of worship must always have in view is the advancement of true religiousness without compulsion or mysterious fanatism, and liberty of consience and toleration without public offense. In its position as leading office it should direct its efforts toward enabling all classes to obtain a thorough training in science and general knowledge, and to disseminate clear conception and such opinions as tend to create usefulness in practical life, true patriotism, obedience to and confidence in the Government and the constitution. Most especially however it should guard against the introduction, into science, of the spirit of exclusiveness, which is nowhere more reprehensible than in objects pertaining to human knowledge."

The winter term of 1812–'13 began under increasing excitement. The first news of the destruction, in Russia, of the French army, reduced the number of participants to the lectures. Teachers and pupils were seized by irresistible desire to regain the fatherland and its most holy possessions.

On February 3, the King called his people to arms; the word had been given and all restraint ceased; the lectures were abandoned, many professors dismissing their pupils with impressive words. On March 28, Schleiermacher read from the pulpit the King's "call to arms."

Quiet again reigned in the halls of learning; as far as the excitement permitted, the remainder re-commenced their labors. In the bulletin of March 18, the rector announced, that notwithstanding the small number of students remaining—most of them being foreigners—the lectures interrupted during the exciting days would be resumed in the coming summer. Only fifteen professors resumed their lectures.

Upon the re-entry at Berlin, on March 31, 1814, of the returning victors, the thought at once was expressed to erect a monument to the memory of those who had perished for the good of the country. On July 16, the senate resolved to engrave their names upon a monument to be erected in the large hall.

The University however gave a further proof of its appreciation and gratitude, by conferring the honorary doctor title upon the following: Hardenberg—patriæ in discrimine positæ sospitatorum felicissimum; Blücher—Germanicæ libertatis vindicem acessimum, gloriæ Borussicæ recuperatorum in victum, felicem, immortatum, Tauenzien, York, Kleist, Bülow,—victoriis, præclarissimis de patris immortaliter meritos, Germanormum libertatis vindices; Gneisenau—consiliis sapientissimis, promptissimis, saluberissimis in procliorum discrimine de patria immortaliter meritum, Germanorum libertatis vindicem.

On February 9, 1815, the anniversary of the war-like action of the students had been celebrated, and on April 7 the King called to arms again. A second hot and bloody summer followed and for a second time Paris surrendered.

During the Franco-Prussian war of 1870–'71, eight hundred students and professors joined the army, and of this number thirty-eight students and one private lecturer lost their lives. On August 3, 1875, the rector unveiled a tablet erected to the memory of the brave young men.

The oration of Prof. Dr. Mittermaier, of Heidelberg, delivered in the name of the representatives of the German and Swiss Universities on October 15, 1860, the fiftieth anniversary of the Berlin Alma Mater bears witness to the fact that its scientific development fully realized the expectations expressed in the rescript of November 23, 1810.

The University preserves a grateful memory to all who principally contributed to its developments. Thus the birthday of King Frederick William III and of the reigning King are celebrated by orations.

In addition to the busts of Kings Frederick William III and IV busts of thirty-three rectors and professors adorn the aula.

As regards the organization of the University the propositions of Schleiermacher were adopted. The faculties of the present day were considered the fundamental columns of the structure. His memorial with regard to the organization of the theological faculty served as a basis for the others.

On December 28, 1810, the regulation of the academic jurisdiction was issued as fundamental law for all Prussian Universities. As a means of protection, the Department of Instruction, on February 8, 1811, issued to the students a "card of recognition." On February 20, 1811, rector and senate informed all universities of the opening and joined the union. The present statutes were sanctioned by the King on October 31, 1816, and delivered on April 26, 1817.

Based on these statutes a later order of January 29, 1838, gave special statutes to each of the faculties. Those of the medical and philosophical faculties have repeatedly been altered since.

The decrees of 1819 and 1834, based on the resolutions of the German Parliament, had originated under influences of principles and conditions which in consequence of the political movement of 1848 had experienced such modifications as to induce the Government to relieve the Prussian universities from the unjust suspicion expressed in those decrees and to return to them the independence required for the development of an active corporate life.

Upon request the universities furnished reports as to a comfortable change with regard to academic jurisdiction and discipline.

On October 29, 1879, a law was promulgated relating to the question of jurisdiction and discipline at the national universities.

At the opening of the University fifty-eight professors were appointed; during the summer term of 1886 their number had increased to two hundred and eighty-three, distributed among the faculties as follows: Theology, seventeen; law, twenty-two; medicine, one hundred and two; philosophy, one hundred and forty-two.

The salaries for the regular professors range between 3,000 ($750) and 12,000 mark ($3,000) annually, and for the secondary professors from 900 to 4,800 mark ($225 to $1,200.)

An almost regular increase has been noticed in the number of students. It may suffice here to state that while for the winter term of

1810 one hundred and ninety-eight students were received at the University (and for the summer term twenty-nine), the number of matriculations for the corresponding terms of 1886 were two thousand one hundred and sixty-seven, and one thousand and seventy-one, respectively.

Notwithstanding the comparatively short period of its existence, the University has already become the recipient of many rich bequests for the benefit of worthy and needy students.

In order to promote diligence among the students prize questions are propounded annually in accordance with the following ministerial regulations:

REGULATION OF SEPTEMBER 16, 1824, WITH REGARD TO PRIZE QUESTIONS.

(1) The faculties of the Royal University are to publish annually prize questions for solution by the students.

(2) These prize questions are to relate to strictly scientific subjects, and, although the fundamental knowledge may have become known in the academic lectures, they must be of such a character as to demand thorough study and independent research in order to show, in the answers, the amount of education received and the individual judgment.

(3) One prize question each is to be published annually by the theological, juristical, and medical faculties, and two by the philosophical faculty, the latter alternating from year to year between one general philosophical and one historical, against one philologhical and one mathematical or physical.

(4) Each faculty selects its own questions alternately from its various branches. The member to whose specialty the question belongs is the privileged questioner. The proposition has to be made in writing and be submitted to the faculty in regular session on the 20th July, and is accepted with two-thirds majority.

(5) All prize questions are published annually, on the birthday of the King, by means of a Latin programme.

(6) Only students of the Berlin University are admitted to competition, and the essay has to be written in Latin.

(7) Nine months are allowed for the essay, viz, from August 3 of one year to May 3 of the following year.

(8) The replies have to be delivered to the University secretary in sealed envelopes and addressed to the respective faculty. Each essay is to contain a sealed slip bearing on its inside the name of the writer and on the outside the motto which has to be written in the essay underneath the title. These essays have to be delivered to the faculty unopened. Before a decision can be made it is necessary that the essays circulate among all members of the faculty; the member who has propounded the question then has to make an explicit report of all the essays and submit the same to the faculty at the latest on the 20th of July, when the papers will be discussed as to their merits. Every regular

professor is required to be present at that meeting or to make a satisfactory excuse. The majority decides as to the award.

(9) A number of essays of insufficient value having been received only by any faculty, it retains the award until the year following, when two questions will be propounded. In the case of unsatisfactory results to the second issue, the ministry reserves the right for future action.

(10) The prize consists of a gold medal in the value of 25 ducats ($60).

(11) The festive proclamation of all the prizes takes place on August 3, the birthday of the King, following immediately upon the oration. The public speaker of the University is required to announce, in brief, the decisions of the faculty of each of the essays. Thereupon the envelope containing the motto of the victorious student is opened and read, together with the name of the essayist.

(12) The envelopes containing the names of the unsuccessful candidates are not opened, but may be withdrawn from the secretary, together with the essay. The crowned essay is also returned to the writer after a copy has been made of it for the archives of the University, and may be published by the author for his own benefit.

The same rules are adopted with regard to the municipal prizes, of which one to the value of 225 mark ($56) is placed at the disposal of each faculty. The prizes were founded, together with stipend, on occasion of the fiftieth anniversary of the University.

Statistics of prize questions for the years 1825–1885 show the following results: 537 questions were propounded during the past sixty years; 779 essays have been submitted, and of these 292, 37.5 per cent. have received the prize; 25, 3.2 per cent., the second reward; and 108, 13.9 per cent., public acknowledgment. With regard to the distribution by faculties the following result is shown:

Faculty.	No. of questions.	Essays.	Prizes.		Second award.		Acknowledgment.	
			No.	Per cent.	No.	Per cent.	No.	Per cent.
Theology	111	151	60	39.7	4	2.6	22	14.6
Juris prudence	111	225	59	26.2	4	1.8	36	16.0
Medicine	110	115	64	55.7	4	3.5	23	20.0
Philosophy	205	288	109	37.8	13	4.5	27	9.4

Institutes connected with the University.

The University Library.—The first impulse towards establishing an independent library for the University was given in 1829, both by the rector and the senate of the University and by the chief librarian of the Royal Library, Prof. Dr. Wicken. In a report to the proper department of the Government it was stated that the Royal Library had become insufficient for the wants of the professors and students of the University, and that for those a separate library had become necessary. The establishment of such a library was then decided by royal decree of February 20, 1831.

The resources of the library were at first very moderate, and consisted of 500 thaler ($375), collected from the students; furthermore, it was decided that each doctor upon promotion, each private lecturer upon his qualification, and each professor upon receiving his appointment, was to pay 5 thaler ($3.75) towards the support of the library. For its location some rooms were allotted in the Royal Library. The chief librarian of the Royal Library was designated as principal librarian, and two officials given him for the performance of the administrative work.

The establishment prospered, notwithstanding the many difficulties presenting themselves. The moderate means were carefully invested in suitable books, and the library further increased by many donations and by the compulsory additions exacted from the publishing houses of the palatinate Brandenburg.

The library lends books for home perusal and is also used as a reading-room.

For the lending of books the library is open daily from 9 A. M. to 2 P. M.; on Saturdays, only to 1 P. M. The reading-room is open daily from 9 A. M. to 7 P. M.; on Saturdays to 1 P. M. only. During the summer vacations the library is open from 11 A. M. to 1 P. M., but the reading-room remains closed.

The budget of the library, exclusive of salaries, is put at 10,500 mark ($2,625) for books and binding, and 4,300 ($1,075) for incidentals.

The personnel consists of a librarian, three custodians, two assistants, two auxiliary helpers, two library messengers, and one porter.

The Mathematical Seminary.—The first "seminary act," the request for the establishment of a mathematical seminary, originated on April 6, 1860, and is worded as follows:

"In the mathematical sciences more than in any other branch of science it is necessary that not alone the substance of the lecture is understood, but that the students, and especially the more advanced, should have an opportunity for instruction in the application of the object of their studies. For that purpose the establishment of a mathematical seminary in connection with the University appears to present the best solution. In the opinion of the petitioners such a mathematical scientific seminary would tend to promote the mathematical education of the students and exert a great and favorable influence upon their practical training as teachers."

An annual appropriation of 500 thaler ($375) was requested for the support of the seminary, of which sum one-half was to be devoted to the acquisition of a special library and the other half to prizes.

On April 23, 1861, the ministry authorized the announcement of seminary exercises under certain provisional regulations, and the sum of 250 thaler ($187.50) was allowed for the purchase of books. On April 26, 1861, the students were invited to participate. The alphabetical list of the members of the first mathematical seminary is dated May 5, 1861.

On October 15, 1861, the draft of regulations for the mathematical seminary was submitted to the University and accepted October 7, 1864.

The regulations of October 7, 1864, are as follows:

(1) The mathematical seminary is a public institute established in connection with the University and has for its object the instruction of such students of mathematical sciences as have already obtained a certain degree of proficiency by aiding them in the independent application and by affording them literary assistance, thus enabling them later on to promote and increase mathematical studies.

(2) The minister of education has the appointment of two professors of the philosophical faculty to supervise the exercises of the students.

(3) Only those matriculated students can be admitted as ordinary members who devote themselves especially to the study of mathematics and have been engaged in that study for at least one year at some university. Foreigners are eligible on the same conditions.

(4) The admission is granted upon the presentation to the director of a discourse and an essay, the examination of which will prove whether the applicant possesses sufficient knowledge and interest to advantageously partake of the privilege. The essay may be omitted upon special occasions in which the director's testimonial is sufficient guaranty for the efficiency of the applicant.

(5) The number of ordinary members is limited to twelve. The directors, however, are empowered to exceed that number by the appointment, as extraordinary members, of a few students possessing the necessary requirements for admission.

(6) Any remiss member may, after having been cautioned and admonished by the director, be excluded from attending the seminary.

(7) The meetings of the seminary take place weekly, at such a time as will permit its extension to two hours or more.

(8) The scientific exercises of the seminary are both oral and in writing. The oral exercises consist in the free discussion of known mathematical problems or of questions propounded by the director, or, at times, by some of the students, and in addresses by the students on the results

of their own experiments or on the results of their studies. The exercises in writing consist in the execution of problems given by the director, and are arranged in such succession that they will cover the entire field of mathematics, and combined, tend to its better understanding, and also in the preparation of larger essays or demonstrations, the subjects of which are given by the director or are selected by the students themselves. The board of directors examine and judge these essays.

(9) The students who excel in both oral and other exercises are—toward the close of each course—to be reported to the minister of education with recommendations for the prizes set apart for the purpose. These semi-annual reports contain also a statement of the exercises held and the general state of the seminary.

(10) A library of the best and most useful mathematical works is to be maintained for the free use of the students and for use in the meetings of the seminary.

The annual appropriation for the seminary, since April 25, 1864, has been 1,200 mark ($300), of which 750 mark ($187.50) are expended for the library and 450 ($112.50) for prizes. The latter however were established by order of March 14, 1884.

The Observatory and Computation Institute.—The first impulse for the establishment of the Berlin Observatory was given toward the end of the seventeenth century by the acceptance, on the part of the Protestant powers of Germany, of the Gregorian calendar. King Frederick I, in order to emancipate the country from foreign researches and labors which had largely entered into consideration on important occasions, resolved to utilize this change which affected all domestic affairs, by establishing an observatory and a society of sciences. He therefore ordered the erection of a square tower, 84 feet high and 40 feet a side, the second floor of which was to be reserved for the society of sciences, while the third floor was to be utilized by the astronomer of the society for purposes of observation. The building was dedicated on January 19, 1711.

The first astronomer of the society, Gottlieb Kirch, had been appointed in July, 1700, but he died (July 25, 1710) before the completion of the building.

On October 15, 1828, King Frederick William III granted a request of Alexander von Humbold for the purchase of a Fraunhofer refractor. The instrument was received in March, 1829, but remained in the packing cases.

On August 10, 1830, permission was given for the purchase of a site for a new observatory which was to be located in sufficiently close proximity to the academy and the university to enable employés of the observatory to continue their connection with those establishments. The building was completed in 1835.

In 1868 a new meridian circle by Pistor and Martius was purchased; in 1869 a hermetically sealed pendalum clock by F. Tiede and in 1879 a universal transit by C. Bamberg.

The publication of the "Berliner Astronomisches Jahrbuch," which had been under the care of the director of the observatory, was in 1874 transferred to a special division of the observatory, known by the name of "Astronomisches Rechen-Institut" (Astronomical Computation Institute).

The Computation Institute, in addition to its regular staff, gives employment to five regular and to a varying number of temporary assistants.

The Institute also contains rooms for the exercises of the University Seminary for the instruction, in scientific calculation, of a number of students.

The budget of the observatory for regular expenses is fixed at 11,340 mark ($2,835), and that of the Computing Institute at 8,800 mark ($2,200), to which for the latter are added 15,000 mark ($3,750), to be used in the publication of the "Astronomisches Jahrbuch," together with the compensation of any computers required for the same.

In close connection with the "Astronomisches Jahrbuch," the publication of which was begun in 1772 by order of the Royal Academy (and the one hundred and thirteenth annual volume of which has just been published), the observatory, since its reconstruction in 1835, has employed itself principally with the determination of positions of fixed stars, planets, and comets.

The results of these observations are published partly in five folio volumes, entitled "Beobachtungen der königlichen Sternwarte zu Berlin," and partly in the "Astronomische Nachrichten."

Five planets belonging to the group between Mars and Jupiter and thirteen comets have been discovered at the observatory.

A remarkable fact is to be recorded in the annals of the observatory in that the planet Neptune, the existence of which had been surmised by Bessel in 1823 from some inexplicable irregularities in the motion of Uranus, and the location of which had been calculated by Le Verrier and Adams on the basis of these disturbances was really discovered in the calculated place on September 23, 1846, by Galle, with the aid of the Fraunhofer refractor.

The Meteorological Institute.—This Institute owes its existence to the exertions of Alexander von Humboldt, as a result of which, the King by order of October 17, 1847, caused its establishment under the direction of the Royal Statistical Bureau, of which it formed an independent scientific division until March 31, 1886.

Negotiations for the re-organization of the meteorological bureau pending for ten years at last terminated in the spring of 1885, and Dr. von Bezold, professor at the technical high school at Munich, who had been induced to accept the newly created chair of meteorology at the Berlin University, was appointed director of the Meteorological Institute, The Institute is to be regarded as a central point for the collection, computation, and publication of meteorological stations of North Germany, the meteorological systems of Oldenburg, Mecklenburg, Hesse, the Saxon states and other smaller states having combined with it.

The system represents the following arrangement of stations:

(*a*) One hundred and thirty stations of Class II, that is, such stations making three observations daily of all the instruments.

(*b*) Fifty stations of Class III, at which a limited number of instruments is observed twice daily.

(*c*) Eighty rain-fall stations.

At present a plan is under consideration for the incorporation in the system of the stations (about one hundred and fifty) of the "Society for Agricultural Meteorology in the province of Saxony and in the Uckermark," and of the stations of the "Agricultural Central Association of Lithuania and Masuren."

The appropriation for the Institute for the administrative year 1886–'87 amounted to 73,060 mark ($18,265), of which 32,560 ($8,140) were intended for salaries of officials, assistants, and computors, 21,000 mark ($5,250), for the payment of observers on stations, and 19,500 mark ($4,875) for other expenses. For architectural changes within the rooms occupied, and for the purchase of instruments, 44,000 mark ($11,000) were allowed by special act and further amounts promised during the coming year.

The Physical Institute.—Upon the extensive space lying between the Neue Wilhelmstrasse, Schlachtgasse, Dorotheenstrasse and river Spree two large buildings are located, each of 108 meter frontage, of which the one along the Dorotheenstrasse has been fitted up for the Physiological and Pharmacological Institutes, while that facing the Spree is occupied by the Physical and the Second Chemical Institutes, all being provided with the required directorial dwellings.

The total cost of the entire structures is 4,500,000 mark ($1,125,000), of which 200,000 ($50,000) were paid for the foundations of the Physiological Institute, 310,000 ($77,500) for that of the Physical Institute, 120,000 ($30,000) for that of the Pharmacalogical Institute, 110,000 ($27,500) for that of the Chemical Institute and 60,000 ($15,000) for those of the dwelling houses, representing 800,000 mark ($200,000), or almost one-fifth of the entire cost of construction.

Until the year 1833 the University did not possess any proper collection of physical apparatus, though a few instruments employed in the course of lectures by professors had been purchased and placed in the hands of professors using them for scientific investigation. It is true that the University had allowed 500 thaler ($375) annually for increasing the collection of physical apparatus, yet the money was generally employed for other purposes. The want of proper apparatus became apparent when Professor Magnus, the late director of the collections, desired to employ a number of them in illustrating some physical lectures. On that occasion Baron von Altenstein, the minister of instruction, proposed to professor Magnus to purchase the required instruments from his own means and suggested a repayment by the state, of 500 thaler ($375) for four successive years, in consideration of which a certain proportion of the apparatus was to become national property.

The proposition was accepted and the instruments thus purchased formed the nucleus of the present physical collection. At the expiration of the above contract a similar arrangement was made, being renewed annually until 1843, when, upon the recommendation of Minister Eichhorn, the collection was placed in possession of a certain allowance per year, which formed the only means ever placed at its disposal with the two exceptions of the donations of the collection used at the university for illustrating Goethe's color theories, and of a collection in the hands of Prof. Paul Erman, and transferred to the institute upon his death. Both collections combined represented only twenty-seven pieces, so that almost the entire collection may be said to have been procured from private means.

Since 1844 the collection had its rooms in the university building, but space was wanting to enable physical researches to be executed.

The personal collection of apparatus and the library of Professor Magnus, bequeathed to the university, formed the foundation of the physical laboratory of the university. Rooms were assigned upon the first floor of the east wing and connected with basement rooms containing the collections by means of winding stairs.

The present building was begun in 1873, and in 1878 had progressed sufficiently to justify the removal from the university building.

After the first few terms of instruction all available space had been occupied, and further applications for admittance had to be rejected.

The present director of the institute is Privy Councillor of Government, Professor Dr. von Helmholtz.

The Mineralogical Museum.—The collection of minerals established by Privy Councillor of Mines Dietrich Karsten, in 1789, by order of Minister Heinitz, consisted of Karsten's private collection, which he had presented to the State in 1781, and of the purchased collections of Councillor of Mines Ferber and Privy Councillor of Finance Gerhard. In 1801 it had been placed in the mint building, and by royal decree of October 18, 1810, became incorporated in the collections of the university under the conditions that the mining department should be recognized as co-partner and should be consulted in case of required changes. In September, 1814, it was placed on exhibition in the university building after having been named, in May, 1814, the Mineralogical Museum of the University.

For the support and increase of the museum 1,000 thaler ($750) were allowed annually since August, 1816, which amount has since been increased to 5,020 mark ($1,255), not to include the personal expenses.

The museum contains the following divisions:

(1) Systematic mineralogical collection.

(2) Display collection of large specimens.

(3) Collection of cut stones and rocks.

(4) Meteorite collection.

(5) Systematic geognostic collection.

(6) Geographical collection, or the geognostic collection of the various countries of the earth.

(7) The palæontological collection.

(8) The library, with collection of charts, well supplied with topographical maps and geognostic maps by the L. v. Borch collection.

The First Chemical Institute.—It is certainly a very remarkable fact that of all the German universities that of Berlin should have been the very last to organize a chemical institute, comprising everything required for the present state of science, since the chemists connected with the University during the first fifty years of its existence occupy a prominent place among the most celebrated investigators of the present century.

But if, notwithstanding such illustrious representations, a great chemical institute was not established until about twenty years ago, it must be considered that at the time of the foundation of the University chemistry was already existing in the academy of sciences, and that the chemical chair at the University was generally occupied by the academical chemist, and hence the University was relieved, in a measure, from the responsibility of providing laboratories for the chemical professors.

The Chemical Institute of the University owes its existence to the energy of the Minister of Education in demanding the appointment of a university professor for the chemical chair.

The selection of a proper site was the next difficulty to overcome, and this was accomplished by the purchase, for the sum of 72,000 mark ($18,000) from the Academy of a portion of its own estate, to which sufficient additional ground was obtained for the erection of an edifice, which was begun in 1865 and completed in 1867.

In addition to the sum estimated, 75,000 mark ($18,750) were expended on the internal arrangements; thus, counting all necessary expenditures, including the 72,000 mark ($18,000) paid the academy and two-thirds of the purchase money paid for the additional lot (only two-thirds of the ground having been used in the erection of the building), 954,000 mark ($238,500) were willingly paid by the Prussian Government for the erection of the new institute.

The Second Chemical Institute.—This institute was established simultaneously with the Pharmacological Institute, and opened on Easter,

1883. It serves specially for the study of inorganic, analytic, and mineral chemistry, and stands under the direction of Prof. Dr. Karl Friedrich Rammelsberg.

For the practical teaching two divisions have been established. In the synthetic laboratory the students are employed in the preparations of chemical substances and the easier problems of quantitative analysis, while in the analytical division quantitative analysis forms the principal subject.

The budget for regular expenditures of the institute, including the use of water and gas has been fixed at 11,285 mark ($2,821).

The Technological Institute.—This Institute originated in the private laboratory of Professor Wichelhaus. To this were added the technological collections of the late Professor Magnus, and by decree of September 11, 1873, the first means were provided for the "establishment of a technological laboratory and for the technological collection of the University." In April, 1883, the newly created Technological Institute was removed to its present quarters under the direction of Prof. Dr. Karl Hermann Wichelhaus.

The publications of the institute are printed principally in the "Berichte der deutschen chemischen Gesellschaft," in the "Verhandlungen des Vereins zur Beförderung des Gewerbefleisses," and in the "Patent-Schriften."

The Botanical Garden.—The greater part of the present Botanical Garden was, at about the middle of the seventeenth century, employed for the growing of hops, to be used in the electoral brewery. In 1679, on occasion of the abolishing of the brewery, Elector Frederick William ordered the garden to be planted in fruit trees and garden truck.

Under the reign of King Frederick I the entire internal arrangement was changed. Glass-houses were erected, oranges were raised, and the kitchen garden changed into a royal pleasure garden.

Under Frederick William I the plans were changed; the garden began to expand and to assume a really botanical character; but the reform had barely begun when the garden was transferred to the keeping of the Society of Sciences. It again lost its botanical character, since, in planting medicinal herbs and plants for the royal pharmacy, the practical king had sought to utilize it to the fullest extent. The society could not afford to expend more than 600 mark ($150) a year on the garden, and furthermore, its great distance from the city rendered it difficult to find a suitable person to supervise it.

In 1809, on occasion of the founding of the University, the Academy of Sciences was relieved of the Botanical Garden, which was then placed under the University, with a guarantied income of 13,000 mark ($3,250).

In 1820 the present winterhouse was erected, and in 1821 the oldest palm house; the latter, however, proving too small it was replaced by the present succulent house. In 1832 the garden possessed eighteen greenhouse divisions, representing a combined length of 350 meters (1,148 feet) with 7,920 kilometer cubic contents.

On July 22, 1852, the Victoria Regia bloomed for the first time in a building erected for its exclusive use.

In view of the principal object of the garden, the advancement of scientific botany, it should be the effort of the director to collect in his garden extensive material for scientific botanical research, and to see that it represents the entire vegetable kingdom to completeness.

The scientific means at the command of the garden are the library, the microscope with auxiliary apparatus, all of which, together with the catalogues of plants, are preserved in the offices of the palm house.

The working force consists of two principal assistants (foremen), fifteen regular assistants, ten younger assistants, some of them voluntary assistants without compensation, one overseer, one engineer, one mason, one cabinet-maker, one carpenter, one glazier, one house-keeper, seventeen laborers, seven to ten char-women, and ten to twelve boys.

The plants cultivated in the garden during the year 1886 comprise 18,837 species, varieties, and forms. The budget is fixed at 85,365 mark ($21,341).

The garden is open to the public every day (except Saturday, Sunday, and legal holidays) from 8 A. M. to 7 P. M. (in winter until dusk). Strangers are admitted at any day.

The public makes very good use of this permission, more especially during the period of blooming of the Victoria Regia, and also when the plantation of gourds is at its height. From six to seven thousand visitors have been recorded in a single day.

Special permission by card to visit the grounds is given to any one desiring to investigate or study, and this special permission includes the privilege to visit portions closed against the ordinary public, and it also entitles the bearer to receive flowers or other material for investigation. Plants or parts of plants are also furnished to non-resident botanists. The garden supplies the University and Royal Schools with the material required for botanical lectures.

The Botanical Museum.—Collections of curious objects from the vegetable kingdom as well as of dried and mounted plants had been commenced by the Society (later Academy) of Sciences in the last century. The first herbarium presented to the society which possessed a really scientific value was that of Andreas Gundelsheimer, consisting of oriental specimens. Another important collection was begun by Ludwig Stosch in the Netherlands, France, and the Pyrenees by order of King Frederick I. The Royal Library, too, and the Art Collection contained a few collections of plants bound up in book form, of which the oldest and most interesting is that of J. S. Elsholz, the court physician of the great elector and director of the pleasure garden. The "Gesellschaft naturforschender Freunde" at Berlin, too, had its own cabinet of natural curiosities, but all these divisions were gradually transferred to the Botanical Museum.

The Royal Herbarium proper, which did not receive the designation "Royal Botanical Museum" until 1879, existed since 1818, when the Willdenow collection of plants was purchased for 36,000 mark ($9,000.)

The collections were at first deposited in the rear portion of the building (Dorotheenstrasse 10) belonging to the Academy, and in 1822 were transferred to a house which had been purchased as a dwelling house for the officials of the garden, but which had been let to the "School for Gardeners."

In 1824 the herbarium of Inspector Otto was purchased, comprising between fourteen and fifteen thousand species and Leopold von Buchs presented his collection made at the Canary Islands.

A new feature was now introduced, that of the lending out of collections. Until then they had been used and studied in the building by but few people; now any botanist employed in morphological and floral studies could have the required material sent him; thus the herbarium obtained a number of collaborators who voluntarily undertook the determination of species, which resulted in the acquisition of a number of original specimens. Upon the return of a collection the duplicates were divided and employed in exchange with the leading establishments of London, St. Petersburg, Paris, etc. The budget for the purchase of plants being limited to 720 mark ($180), only collections of the greatest importance could be procured from these means.

Although the means for running expenses were thus limited, the Government provided liberally on extraordinary occasions to secure the acquisition of large and important private collections.

By such means the Kunth herbarium was obtained, which consisted of: (1) A general collection of about 44,500 species in about 60,000 specimens, comprising many duplicates, from the Paris Museum, and about 3,000 originals to "Humboldt, Bonpland, and Kunth Nova Genera et Species;" (2) a collection of dried plants from the botanical garden at Berlin, comprising 10,300 species; and (3) a collection of woods. The price paid was 24,000 mark ($6,000).

The Link herbarium purchased in 1852 for 4,500 mark ($1,125) enriched principally the European flora, especially by specimens collected by himself in Portugal and Greece. It also increased the collection of fungi. The arrangement of garden plants, too, was of great importance. The entire collection represented 3,113 cryptogams and 16,382 phanerogams.

The von Nees von Eserbeck collection of glumaceæ was purchased in 1855 for 2,700 mark ($675). It contained 9,559 species.

The collection of lichens of Major von Flotow was purchased in 1857 for 6,000 mark ($1,500).

Two donations were received which, aside from the transportation, did not cause further expense. One of them, the herbarium of Lieutenant General von Gansauge, obtained in 1871, contained about 15,000 species, while that of Professor Laurer, received in 1874, contained a rare collection of lichens and of mosses.

Upon the death, in 1877, of A. Brauns, the state purchased his collections for 21,000 mark ($5,250), the Academy of Sciences his scientific manuscripts for 4,000 mark ($1,000). They were transferred to the museum on condition that they were to be preserved and made accessible to specialists. The botanical collections consisted of: (1) a morphological herbarium of forty-three maps; (2) an herbarium of phanerogams of considerable extent, which excelled by its wealth of forms and localities; (3) a valuable herbarium of cryptogams; (4) a collection of fruits and seeds, among which the cycadæ, coniferæ, and juglandæ deserve special mention.

Owing to the want of sufficient accommodation the herbarium was, in 1857, transferred to Berlin, and assigned rooms in the east wing of the University. Here the collections remained until March, 1880, when they were removed to the new building, erected at a cost of 280,000 mark ($70,000) for the building and 80,000 ($20,000) for its internal arrangement.

At about that time the large and precious Mettenius collection of ferns was purchased for 6,000 mark ($1,500).

The most valuable collection of Dr. Georg von Martens was presented by his heirs. It comprised 12,439 species and contained among others the originals employed in "Schübler and Martens flora of Würtemberg," valuable collections made by the Würtemberg Travelers Society, and also 4,101 species of salt water algæ in the best possible state of preservation and fully described by the former owner.

Finally the herbarium of the late Professor Lorentz was received (who died in the Argentine Republic), which contained a large and critical collection of mosses as well as a rich herbarium of the Argentine flora, being largely the originals employed in "Griesebach's determination of the Argentine plants."

Access to the collections is granted to any one personally known or properly introduced. Any one desiring to compare or study plants or other objects receives permission upon application, and is furnished desk and temporary desk-room. Responsible botanists within the Prussian domains can obtain the use of objects at their respective homes for a limited period of time. Non-resident botanists can obtain that privilege only upon special permission from the department.

The collections of the museum are open to the general public in summer on Monday and Thursday afternoons.

The University Garden.—Owing to the great distance of the botanical garden the establishment of a garden as auxiliary means in the botanical instructions received early consideration soon after the founding of the University, and in 1820 the space in the rear of the University was employed for the purpose. The garden was intended to contain the principal officinal plants and those resembling these; and also, as far as practicable, economical, technical, and commercial plants; while in the surrounding vacant spaces, ornamental trees and shrubs were to be planted. The establishment was completed in 1821–'22. It was provided with a green-house containing a cold and a hot division. The plants were furnished by the botanical garden, under the care of which the new plantation was placed. In 1837 it was made independent, by the appointment as University gardener, of Mr. Sauer, of the botanical garden. As long as but one regular chair existed at the University for botany, the incumbent always held the appointment of director of both the Botanical and University gardens.

The garden has not increased in extent. A small earth-house was added to the existing green-house. Another small dirt-house without furnace serves for the wintering of less sensitive plants. The heating of the other houses is effected by means of hot water through copper pipes. In the selection of green-house plants special attention was given to those used for officinal and domestic purposes.

The Botanical Institute.—This Institute was established in 1878 on the top floor of the old Exchange Building. A large hall with favorable light was set aside for microscopical examinations by beginners, and several other rooms were given to the more advanced students. A small chamber served as dark-room and a large corner room was fitted up as a physiological laboratory. The director, the assistants, and the messenger had each one room assigned; an assembly room and a lecture hall seating from thirty to thirty-five were also provided.

In the autumn of 1883 the Institute was transferred to its present location, the situation of which, in the vicinity of the University and of the University garden, may be pronounced as very favorable.

The Institute possesses at present nineteen large and twenty-eight small microscopes, together with all the required auxiliary apparatus (prisms, micrometer, goniometer, etc.), twenty-six demonstration microscopes for use in lectures, one microtom, one micro-spectroscope, one solar spectroscope, one large spectral apparatus by G. & S. Merz, one heliostate by Heele, one achromatic lens by Steinheil (81 millimeters aperture), one cathetometer by Heele, one compression-pump with lever by Pfeil, one double-action air-pump, one double aspirator by Warmbrunn & Quilitz, one gas-regulator, one gasometer, one astatic mirror galvanometer by Siemens & Halske, one immersion battery, three klinostats, among which one very large and powerful by Heele, one auxanometer with self-registering clock-work, two pair of balance-scales, and numerous smaller apparatus and models for physiological research.

The budget, exclusive of administration and salaries of assistant and messenger, was 5,930 mark ($1,482.50) until 1885 when, in consideration of lessened necessities, it was decreased to 3,930 mark ($982.50).

The direction of the Institute is combined with the chair of anatomy and physiology of plants at the Royal University.

The Institute of Vegetable Physiology.—The Institute was created in 1873, and in 1880 united with the botanico-microscopical laboratory of the Agricultural High School in such a manner that the use of the scientific apparatus and inventory belonging to the University is available to the students of either of these establishments, while the means of support are to be furnished by the Department of Agriculture exclusively.

The object of the Institute is the study of morphology, development, and physiology of the plants. For this purpose lectures are delivered and practical instruction furnished; the students also have an opportunity for making individual examinations.

The Institute contains (1) a hall for the students in microscopy, seating twenty, and facing north; (2) a room for the director; (3) a room seating four for chemical work; (4) a large room for the assistant and six of the advanced students. This room also contains the library. (5) A dark-room; (6) a room for physiological research and containing a Pfeffer rotary apparatus; (7) two greenhouses, in which the objects for microscopical and physiological research are produced; (8) a small experimental garden.

The Institute is well provided with optical instruments and physiological apparatus; it also contains extensive collections for instruction, comprising the subject of morphology, production and development, and the physiology of plants.

The Zoölogical Museum.—The establishment of the Zoölogical Museum is contemporaneous with the foundation of the University. The Museum was located in the University building since its erection, but from the beginning the plan and execution exceeded the material required for demonstration, and that in the direction of creating a basis for the systematic knowledge of all living animals, thus to form a zoölogical center for Germany similar to that at Paris, London, Leiden, Vienna, for their respective countries. The Museum was planned by Count Joh. v. Hoffmannsegg, who made the first donation, consisting of several thousand specimens of Brazilian mammals, birds, and reptiles. The principal and fundamental stock was formed by the donation, by the royal collections, of a number of natural history objects, consisting of mammals, birds, insects, and shells; to these were added the following original collections:

(1) The collection of fishes of Dr. Marcus Elieser Bloch, practicing physician at Berlin. Originals (partly dried, partly in alcohol) described in his "Naturgeschichte der Fische Deutschlands," 1782–1785, and "Naturgeschichte der ausländischen Fische," 1785–1795.

(2) The collection of crustacea, of Joh. Friedr. Wilh. Herbst, pastor at the Church of St. Mary, and purchased for 447 thaler ($312.75). Originals used in his "Naturgeschichte der Krabben und Krebse," 1790–1804.

(3) A large collection of corals, presented by counsellor of the court, Dr. Gerresheim of Dresden, whose oil portrait has been placed in one of the halls of the Museum. The collection serves as basis to Ehrenberg's systematic classification of corals.

A few but highly important original specimens of fishes and conchyliæ from Northeast Asia were presented by P. S. Pallas, explorer of Russia.

Of other collections added by explorers at the earlier times the following may be mentioned: Krebs and Bergius, South Africa; C. G. Ehrenberg and Hemprich, Egypt, Nubia, and the coasts of the Red Sea; Sello and v. Olfen, Brazil and the La Plata regions; von Sack, Deppe, and Schiede, Mexico; Carl Ehrenberg, West Indies, principally St. Thomas; J. Cabanis and Zimmermann, southern portion of North America; v. Minutoli, Canary Islands; v. Sack, Cyprus; Eversmann, Bokhara; Lamare-Picquot, Maskarena, and Bengal (purchased in 1836 for 6,000 thaler) ($4,500); Lhotsky and Schayer, Australia, etc.

The museum is provided with a fair library, which is placed in the rooms of the director and custodians.

The budget was, in 1810, fixed at 2,200 thaler ($1,650), of which 1,900 ($1,425) were paid for salaries (exclusive of that of the director). In 1837 the budget was increased to 3,550 thaler ($2,662.50) (salaries 3,400, ($2,550)); in 1843 to 5,565 thaler ($4,173.75). At present the amount allowed is (exclusive of the director's salary) 1,800 mark ($450) 54,670 mark ($13,667.50), from which salaries are paid to the curators 3,300 ($825) and 4,800 mark ($1,200), together with allowance for dwelling to assistants, and taxidermists from 1,200 to 2,400 mark ($300 to $600), while 22,960 ($5,740) are set aside for incidentals, of which amount one-half is reserved for purchases.

The museum is open to the general public every Thursday and Friday from 12 to 2 P. M. Owing to the fact that the exhibition halls are not heated, the attendance varies with the season; it is most numerous on holidays. Classes of schools may be admitted any day upon proper application. Students are supplied with tickets which admit them every forenoon. Artists, upon receiving permission from the director, have an opportunity of drawing and sketching. Scientific geologists are admitted at any hour of the day

The Zoological Institute.—When, upon the death of Prof. Wilhelm Peters, director of the museum, a separation of the University professorship and direction of the Royal Zoological Museum had been decided on, the establishment of a Zoological Institute, devoted exclusively to the needs of academic instruction and to scientific investigation, became urgent.

The Natural History Museum.—The Zoological Museum and the Zoological Institute were, in 1888, removed to a new building, which bears the name "Natural History Museum."

The building consists of three stories above the basement. The front portion is intended to contain the Mineralogical Museum; it forms almost a square around a glass-covered court, which serves as an entrance to the rear portion of the building containing the Zoological Museum.

The Zoological Garden.—The history of the Berlin Zoological Garden antedates that of any similar establishments of Germany.

Its nucleus was the collection of live animals of King Frederick William III, located upon the "Pfaueninsel," near Potsdam. Upon his death Professor Lichtenstein, the naturalist, influenced King Frederick William IV to consign the animals, as a beginning, to a zoological garden, the erection of which, in the neighborhood of the capital, had already been considered.

The former pheasant garden (Königliche Fasanerie) was given up to the enterprise, which soon was called into existence by a syndicate, with the assistance of money grants from the Government.

For a number of years the garden served principally for scientific investigations until, in about 1865, by the establishment of gardens elsewhere the general attention was drawn toward it. A comparison with the more recent establishment proved rather unfavorable to the home garden. A visit of the Queen Augusta to the zoological garden at Cologne gave rise to the consideration of important improvements, suggested with the view of making the Berlin garden worthy of the distinction of being located in the national capital.

The first step was the formation of a company which issued shares to the amount of 300,000 mark ($75,000) and effected an additional loan of 1,500,000 mark ($375,000). The ground was donated by the Government, and Dr. H. Bodinus, former director of the Cologne garden, was invited to assume the directorship. So energetically did he proceed that the first concert could be arranged for in the garden in the summer of 1870, before the commencement of the Franco-Prussian war. The new establishment had extended its usefulness and combined the scientific investigations with amusement.

In rapid succession the various buildings were now erected which were to serve as quarters for the many animals, so that, in addition to the old buildings, accommodations were prepared for the mammals, animals of prey, birds of prey, for the antelopes, birds, elephants, etc. Considerable additions were made to the stock of animals, and thus, within a remarkably short space of time, an establishment was created which, while it could favorably compare with any other existing garden, was also worthy its rank, and took a conspicuous place among the points of interest of the residence of the German Empire.

With regard to the collection it may be said that it comprises representatives of most all the important mammals and birds, in as many species and specimens as can successfully be provided for.

The more recent construction of a house for monkeys has offered an opportunity for a rich and carefully selected collection of those animals.

The Aquarium.—The aquarium, constructed during the years 1867–'69 (by the genial architect H. Luer), is an original and interesting structure, rich in grottoes and caves, and is intended to harbor animals of other classes besides aquatic animals. The marine invertebrates are principally represented.

The Berlin Aquarium, in its arrangement and facilities for the observation and study of the lower classes of animals, offers considerable means for natural history instruction, and thus aids materially in the development of natural history study.

To scientists it offers facilities by placing at their disposal the material collected from many places and seas.

III.—THE MILITARY MEDICAL INSTITUTES.

[Omitted here.]

IV.—THE AGRICULTURAL HIGH SCHOOL.

With regard to agricultural instruction in the margraviate Brandenberg it may be stated that a chair for agriculture was founded in 1727 at the University of Frankfort on the Oder.

In 1806, by invitation of King Frederick William III Albrecht Thaer established the first German agricultural academy under the name of "Royal Academic School of Agriculture at Möglin."

In 1810, the trying time for the Kingdom, when the development of economic and industrial resources assumed the greatest importance in the struggle to supply means for a successful warfare, the agricultural school became incorporated in the University; the distance, however, preventing the close relationship anticipated, a separation again took place.

Notwithstanding the discontinuance of agricultural lectures, some few agriculturists continued in their attendance to the University, and the authorities kept up their relationship to the agricultural academy in order to enable students in the branches of political sciences to complete their studies by participating in the practical course of instruction.

While in most of the Prussian provinces special agricultural schools were established, the agricultural central union of the district of Potsdam insisted on having the connection kept up between the Royal Academic School of Agriculture and the University at Berlin.

In 1860 the old agricultural academy at Möglin ceased to exist and an agricultural school was established at Berlin, which at first, according to sections 1, 3, 4, and 5, of the programme illustrating the object of the institute, was depending on the University in so far as being open to all matriculated students.

The present requirements exact of an agriculturist a more thorough scientific training than was expected at the agricultural school at Möglin.

Since 1866 the students are required to register at the central bureau of the Agricultural Department instead of at the University as formerly.

In 1867 the Department of Agriculture began the establishment of an agricultural museum, the nucleus being formed by the exhibits returned from the Paris exhibition.

The demand for increased accommodations becoming felt more pressingly each year, the east portion of the site formerly occupied by the Royal Iron Foundry, was assigned to the erection of an agricultural building while the remaining portions were to be devoted to buildings for the Geological Institute and Mining Academy, and for the natural history Museum of the University.

The agricultural building was erected during the years 1876–'80 and opened in 1880, with the International Fishery Exhibition, which was held in the portion of the building intended for the agricultural museum. The opening of the museum, therefore, had to be deferred until after the close of the exhibition.

By royal decree of February 14, 1881, the united institute and museum received the name "Agricultural High School." Its constitution was arranged for by provisional statutes of May 27, 1881, of the Minister of Agriculture.

The constitution provides for the appointment of a commission for the decision of points of organization and to submit propositions.

The directors of the various divisions have been granted considerable freedom of action and separate funds have been placed at their disposal.

The corps of teachers is presided over by a rector whose election rests with the above-named commission, subject to confirmation by the ministry.

The business affairs of the establishment are conducted in part by a board of trustees and partly by the Ministry of Agriculture.

The degree of education required for admission is that demanded to secure the privilege of voluntary service in the army.

An addition was made to the scope of the institute by the establishment, in 1883, by ministerial decree of October 10, 1882, of a geodetic and technical course for surveying. Special teachers have been employed for the purpose. The students of geodesy are required to possess the degree of the highest class of a gymnasium or real school.

The museum is to serve the double purpose of academic instruction and of being the central institute which enables investigators to engage in special studies; by its exhibits it also serves for the education of the people.

The budget for 1886–'87 fixed the expenses of the high school at 224,970 mark ($56,242.50): its income is 39,328 mark ($9,832); hence an appropriation of 185,642 mark ($46,410.50) is required.

Examinations take place for (1) agriculturists. (2) land surveyors, (3) technical culture, (4) agricultural teachers.

Three prize questions are propounded at the close of each summer term, which pertain (1) to agriculture, (2) fundamental sciences, (3) technic of cultivation. The award is 150 mark ($37.50) in each case. Essays of merit, though not quite up to the standard of perfection, receive honorable mention.

The Central Library.—Its object and purpose is to offer to professors and students of the agricultural high school all necessary scientific means, and in its capacity as the most complete specific library, to aid all interests. For this purpose much attention is given to its completion in all branches of agriculture, forestry, etc.

The Physical Cabinet and the Meteorological Observatory.—The collection contains the apparatus required for instruction in physics. They are employed by the students in the course of lectures and in investigations. A series of meteorological apparatus has been provided, which serves the purpose of instruction. Three daily readings are made of the instruments prescribed for stations of the second class; the results are published by the Prussian Meteorological Institute. A number of automatic apparatus register the progress of pressure, temperature, precipitation, force, and velocity of wind, and these automatic records serve in the preparation of essays on climatology, etc. The entire arrangement is that of a station of the second order. A regular exchange of barograms has been arranged with Hamburg, Magdeburg, Wien, and Copenhagen

The Chemical Institute.—The principal object of the institute is the instruction of the students in general chemistry, qualitative and quantitative analysis. It is located in a building adjoining the Agricultural High School, and also accommodates the laboratory of the Society for the Production of Beet Sugar in the German Empire. The ground floor contains a large work room arranged for fifty students, and is provided with skylight; furthermore an auditory, having a capacity of one hundred and forty. The upper floor accommodates the private laboratory of the director and a few rooms intended for special investigations.

The chemical instruction comprises: (1) In winter, lectures on inorganic, and (2) in summer, on organic chemistry; (3) practical exercises in qualitative and quantitative analyses, and other chemical experiments, for which purpose the laboratory is open daily from 9 A. M. to 5 P. M.

The Mineralogic-Pedological Institute.—In view of the existing connection between mineralogy, geology, and agriculture a chair of mineralogy and pedology (πέδον, earth, soil, and λόγος) was provided at the reorganization, in 1881, of the agricultural high school. The occupant has also the charge over the recently created mineralogic-geologic-pedological museum.

Its collections resolve themselves into the two classes for (*a*) education and (*b*) exhibition.

(*a*) The educational collections contain minerals, rocks, and soils of importance in agriculture; the collection has been made instructive by the selection of specimens showing the changes produced by exposure.

(*b*) The exhibition collections are placed in horizontal and perpendicular glass cases, and represent a well organized and described material for the instruction in and contemplation of the composition and variety of the native soil.

The mineralogical collection is arranged according to the Zirkel system, and shows the more important minerals in their characteristic forms.

The petrographic collection is arranged according to Creduer's system, and exhibits characteristic forms of rocks. The exhibition hall also contains several geological charts, of which may be specially mentioned that of the Harz Mountains and the Thuringian forest, which is composed of eighty-one sheets.

The geological and pedological collection is exhibited in the adjoining hall. Commencing with the most recent formations, it passes through the various layers of humus, clay, lime, gravel, sand, and ferriferous soils, including the organic inclosures of alluvium, diluvium, and tertiary periods which connect with the mesozoic, paleozoic, and archaic periods.

Chemical analyses, microscopic preparations, solutions, reliefs, profiles, and tableaus complete the objects. Illustrations of sceneries belonging to the various geological periods and illustrating their peculiarities are suspended above the exhibition cases.

The pedological division has found accommodations along the light court of the museum. It contains apparatus for examination of the soil, the mineral fertilizers, fuel, representations of Thuringian brown coal industry, manipulations of asphaltum, flint, and peat, and industries.

The Agronomic-Pedological Institute.—The Institute embraces two divisions, the agronomic-pedological and agricultural-chemical laboratory, and the division for soil, fertilizer, and irrigation and drainage.

The agronomic-pedological laboratory purports to promote the scientific explorations of the soil in its relation to the structure of plants and to its cultivation, and to offer facilities for the study and execution of agronomic-pedological and agricultural-chemical experiment.

The agronomic-pedological laboratory consists of a large room for the accommodation of the assistant and twenty students, and contains all apparatus for agricultural-chemical and physical examination and analysis.

The Institute of Vegetable Physiology.—The Institute was established in 1881, and comprises a laboratory for physiological work, with dark room, microscopic room, chemical division, green-house, and experimental garden. It possesses all the necessary apparatus, instruments and collections, and a library. The Institute prepares the physiological experiments required in the lectures, and offers opportunity to the student to practice vegetable physiology and pathology. To the director, his assistants, and the practitioners it affords means for special investigation in vegetable physiology and the diseases of cultivated plants.

The Vegetable Division of the Museum.—When the request was made on the agriculturists to furnish exhibits for the Paris Exposition of 1867 they readily responded, but expressed themselves desirous of transferring their exhibits, after the close of the exposition, to the Government, to serve in the foundation of an agricultural museum. This proposition was accepted, and many objects were added to those exhibits, by donation or by purchase. The products of agriculture, of course, received the first consideration, and thus the wealth of the division may be explained. Corresponding to the enlargement of the agricultural administration to a department of agriculture, lands, and forests, the science of forestry has received consideration, and the collection of woods has become a very complete one, owing to donations of foreign governments, especially of those of India, Japan, and of the French colonies.

The Zoological Institute.—The collections of the Institute are relatively complete, especially with regard to the osteology of mammals; it contains the collection of skulls and skeletons of Herm von Nathusius and those of the abandoned agricultural academies of Eldena and Proskau. In addition to the skulls of other mammals it contains large collections of the skulls of horses, hogs, cattle, sheep, and dogs of the various breeds, domestic and foreign, which in completeness can not be excelled by any other museum. The remaining divisions of the zoological collections are generally restricted to the European animals of interest or importance to the agriculturist, without, however, excluding such foreign animals as may be required in the systematic study of the animal kingdom. The Institute proposes to investigate all zoological questions relating to agriculture, and to carefully trace the derivation and evolution of all domesticated animals, for which purpose a special interest is given to the fossil and subfossil remains of the domestic animals and to their relatives.

The Institute of Animal Physiology.—Since the enlargement of the last year, the institute has the use of two large and four small laboratories, which are provided with all improvements required for the analytical and vivisectory labors of the institute. It also controls a stable with divisions for the various animals and some basement rooms which, on account of their good light, are well adapted for the keeping of animals under observation. A small respiratory apparatus has found room in the basement.

The institute possesses the required apparatus for microscopical work and for the study of material changes of the mechanism of the nervous system, of respiration and circulation. Additions to the apparatus are made whenever needed, as far as the means at disposal permit.

The institute serves for instruction in so far as it aids the lectures on animal physiology, sanitation, etc., by actual demonstration and experiments.

The Zoo-technical Institute.—The means by which the Zoo-technical Institute expects to succeed, excepting the necessary apparatus, etc., are found in the representations, models, and, as regards sheep-raising, of samples of wool. The wealth of the collection and the correctness of the representations render this portion of the museum a permanent animal exhibition, with however the additional advantage that the model does not represent one single act, but that each typical appearance of the agricultural domestic animal has been shown therein. It is a further object of the Zoo technical Museum to keep before the public all possible phases of animal industry of the nation.

The Institute is in charge of Privy Councellor, Prof. Dr. H. Settegast.

The Institute of Geodetic Instruction.—The geodetic collection, founded in 1883, contains ten theodolites (three with microscopes), two compass apparatus, eleven levelling instruments, of which two are for exact work having all auxiliary apparatus, one sextant, one prism circle, one engineer's table, one Fortin barometer, four aneroids, three Amler planimeters, two precision planimeters, two pantographs, apparatus for the examination of the above instruments, demonstration apparatus, and surveying apparatus. Available for instruction are further a geodetic library, a collection of plans, and a series of forty-two wall-diagrams representing geodetic instruments.

The surveying exercises are—during the summer—executed in the open air; in winter, indoors in a large hall which has been constructed for the purpose. Two large rooms are used in geodetic computations and drawing; adjoining these is the room of the assistant, which contains the collection of plans, and that of the professor, which contains the library.

Division of Machinery and Models.—The collection, located in a hall of the ground floor, consists of measuring apparatus, actual machines, or models. Among the latter may be mentioned the Rausch models exhibited in the southern vestibule and show the historical development of hand implements and of the plow. The requirements of instruction and of practical use determine the enlargement of the collections; the additions, therefore, confine themselves principally to elements of construction and to apparatus for the testing of useful machinery.

A collection of serviceable machinery in complete working order is employed for instruction. For that purpose an engine of 35 horse-power has been mounted in the hall. A water reservoir located in the hall contains a pulsometer, a centrifugal pump, an open archimedian screw, an overshot and an undershot wheel. Exhibitors have free use of space for terms of six months; the articles exhibited are published annually in the Deutsche landwirthschaftliche Presse. The space will admit of the exhibition of about two hundred objects, including three to four locomotives and as many combined threshers.

The Division of Fermentation and Starch-making.—This division represents the above industries and belongs to the State only in so far as it occupies an official building and its director is one of the teachers of the agricultural high-school. The management is in the hands of the following technical associations: The Society of Distillers in Germany, the Society and Experimental Institute for Brewing, and the Society of Starch Manufacturers in Germany. These associations, with a membership of about three thousand, pay 100,000 mark ($25,000) annually towards the support of the division.

The Division of Sugar Industry.—This division, supported by the Society for Beet-Sugar Industry in the German Empire, receives its rooms from the state free of expense. Its objects are: (1) the education of chemists for the sugar industry; (2) any examination required by a member of the society and the testing of instruments of precision; (3) the opening up of new fields regarding the composition of raw substances, auxiliary substances, and products of manufacture, as well as the development of technic and supervision of management. These objects are attained by the labors of the very completely organized laboratory, by lectures, and by attendance to the periodical meetings of the industrials.

The Royal School for Gardening, and the National Nursery.—Although occupying separate localities, but closely related, both establishments owe their existence to the exertions, in 1823, of the general director of the gardens, Dr. Lenné, who secured the means necessary for their establishment from the munificency of King Frederick William III, from the interested departments of the government, from the society for the promotion of horticulture in the Prussian states and from the stockholders of the national nursery. The royal decree of August 20, 1823, granted them the privileges of a corporation.

The Gardening school, whose principal object was the education of the various grades of gardeners, had its practical division at Schöneberg, a suburb of Berlin, while the division for scientific and artistic instruction was located upon the "Pfaueninsel," near Potsdam. Owing to insufficient results caused by the separation the divisions at Schöneberg were abandoned and combined with the establishment at Potsdam.

The principal building of the establishment, in addition to the apartment set aside for the inspector, contains halls for instruction and for the collections, as also accommodation for twenty-seven apprentices which number, however, is generally exceeded. This arrangement, together with the permission of the Emperor of the freedom of the imperial gardens for purposes of instruction, and the connection with the national nursery, affords great facilities.

The National Nursery is located upon a territory of 200 acres in extent. In addition to the training of young gardeners it has engaged in the examination of important pomological questions and subjects and is testing and supplying to fruit-growers reliable standard trees.

Both establishments have been placed under the care of the Department of Agriculture and are managed by a board of trustees composed of a representative of the Department of Agriculture, lands and forests, who is the president, a representative of the royal garden intendancy and a member of the society for the promotion of horticulture in the Royal Prussian state. The immediate administration is in the hands of Royal Garden Inspector Jühleke, assisted by two inspectors and one secretary.

V.—THE GEOLOGICAL INSTITUTE AND THE MINING ACADEMY.

The Mining Academy.—This was called into existence by royal decree of September 1, 1860,. In its organization the same points were considered which for some time prior had been adopted in the training of candidates for technical positions.

The mining officials requiring an education which involves a knowledge of law, national economy, and the natural sciences, in addition to their technical attainments were, until then, required to finish their studies at some national university. In the organization of the Berlin Academy all branches had been provided for thus enabling the student to complete his studies without being obliged to visit a university. The programme of instruction therefore provided for the following branches:

(1) Science of mining. (2) Mining, surveying and mathematical geography. (3) Practice of mine surveying and in drafting. (4) Science of salt mining. (5) Science of manufacture. (6) General metallurgy. (7) General assaying. (8) Blowpipe. (8) Iron mining. (9) Projecting of iron-works. (11) Assaying of iron. (12) Metallurgical technology. (13) Chemical technology. (14) Science of machinery. (15) Machines for mining and smelting works. (16) Construction. (17) Architectural construction. (18) Drawing. (19) Plane and spherical trigonometry, stereometry, and analytical geometry. (20) Descriptive geometry. (21) Differential and integral calculus. (22) Mechanics. (23) Mineralogy. (24) Mineralogical practice. (25) Petrography. (26) Petrographic practice. (27) Paleontology. (28) Paleontological determinations. (29) Fossil plants. (30) Geognosy. (31) Geology of quaternary formations. (32) General geology. (33) Analytical chemistry. (34) Chemical laboratory. (35) Mining laws.

The Geological Institute.—The first beginning of the present institute may be traced back to the year 1862. The office of the royal director of mines had commenced the construction of geological maps of the Rhine province and of Westphalia at a scale of 1:80000, of Nether and Upper Silesia at a scale of 1:100000, and for the province of Saxony it had undertaken a continuation of von Strombeck's map of the district of Magdeburg.

When, in the execution of that work, the maps of the general staff, which were prepared to a scale of 1:25000, were compared, it was shown in a prominent manner that geological maps constructed according to that scale would be of infinitely greater value both for scientific and practical purposes than maps constructed at a scale of 1:100000. It was, therefore, decided to accept the scale of 1:25000 as a basis for all the maps of the entire State.

The execution of the survey and the construction of the maps on the prescribed scale was commenced in various portions of the State. First of all, attention was given to the provinces of Hesse and Hanover, which, in 1866, had been added to the Kingdom, because they would form a continuation of the survey already begun of the Harz Mountains and the Thuringian Forest. It then was extended to the former Duchy of Nassau, the southern portion of the Rhine province and to the plains of North Germany and to the provinces of Silesia.

The Kingdom of Saxony, Alsace-Lorraine, and the Grand Duchy of Hesse have since adopted the Prussian plan and have consented to the construction of a geological map on a scale of 1:25000.

As regards the organization of the geological survey, it may be stated that since 1862 geological surveys were made by teachers of mineralogy at the mining academy at Berlin. The building of this academy contained the geological and mineralogical collections intended both for instruction and for explorations, and the results of the surveys were worked out in that building.

On January 1, 1873, the Royal Geological Institute was established, and on April 8, 1875, received the final statutes, as follows:

SEC. 1. It is the object of the Geological Institute to execute the geological examinations of the Kingdom of Prussia and to digest the results in a manner to make them available and useful to science, as well as to the economic interest of the country.

SEC. 2. In accordance with the above the Geological Institute will execute the following work: (1) The construction and publication of a geological map of the entire state, on the basis of the original surveys of the General Staff, on a scale of 1:25000. This chart is to contain a representation of the geological formations, condition of the soil, and the occurrence of useful stone and minerals, and is to be accompanied by descriptive text. (2) The construction of a geological chart on a basis of 1:100000. (3) The publication of monographs on geological or mineralogical objects of special interest. (4) The publication of essays on geological, paleontological, or montanistic contents, supplementing the geological chart. (5) The collection and preservation of all documents obtained in the construction of the publications. All these, together with profiles and other representations and illustrations, will be combined in the Geological Museum, to which are to be connected the technological collections of the "Museum of Mining and Metallurgy." These combined collections will afford a very

Each division is complete in itself. It is the duty of the committees of teachers to oversee the scientific instruction of the students within their own section. Its president, elected from among their own number, communicates with the rector and the senate.

The senate consists of the rector, as presiding officer, the retired rector (prorector), the chiefs of the divisions, the chief of the section of naval architecture, and a number of teachers appointed by the various committees for the term of two years.

The position of private lecturer may be obtained by adhesion to one of the existing divisions. The applicant is required to furnish the following documentary evidence: (1) a curriculum vitae; (2) graduation from a high school (gymnasium or realschule); (3) testimonial of a three-years academic study and proof of the successful performance of the first technical examination required by the state or of the diploma examination at some German technical high school, or of the doctor degree of some German university; (4) proof of a continued three-years course of scientific or artistic study following the university term; (5) a manuscript or printed essay on the specialty of the applicant (architects may replace the essay by plans or by bringing satisfactory proof of having had charge of some important construction); (6) an official testimonial of character, and in case the applicant is a German, proof of his having performed his military duties. All the above conditions having been complied with to the satisfaction of the division, the applicant is required to hold one lecture and to subject himself to an examination.

The corps of teachers at present consists of fifty-seven professors and twenty-four private teachers.

The Mineralogical Institute.—In addition to the lecture-rooms the Institute comprises: (1) The laboratory for crystallographic-physical, and chemico-mineralogical experiments; (2) a mineralogical collection; (3) a geological collection; (4) the mineralogical museum.

The lectures combine a course for the practical determination of minerals by microchemical tests and by blow-pipe, determinations of rocks, and instructions for geological surveying.

The public museum, comprising two halls, contains: (1) The systematic mineralogical collection (Tamnau); (2) a mineralogic-technical collection; and (3) the geological collection.

The geological room contains a collection from the Gotthard Tunnel, together with geological profile of the Gotthard in the plane of the axis of the tunnel; projection, 1:2000; a geological collection arranged according to formations.

The Laboratory for Inorganic Chemistry.—The laboratory has room for sixty-six operators; they occupy two larger and two smaller halls. Separate rooms are provided for special work.

An auditory of one hundred and seventy seats is situated within the middle section; it connects with rooms for preparation and collections.

machinery, etc., while the second division has to test on the principle of Wöhler-Spangenberg duration experiments, and the third is confined to the official testing of paper. A mechanical workshop has been placed under the charge of the second superintendent.

The Institute owns two excellent machines for the testing of finished articles, having a power of 100,000 and 50,000 kilograms (system Werder and Marten's), six smaller machines for the same purpose, eleven machines for duration tests, photographic, microscopic, etc., apparatus.

The Royal Testing Station of Building Materials.—This was founded on March 1, 1871, in connection with the Technical High School; it decides as to the quality of cements and other building material furnished to the Government. The station owns apparatus for the testing of durability and other physical properties of burnt and unburnt artificial stone, natural stone, cement, plaster, lime, clay pipes, and all other building material.

The hydraulic press of the station, of 140,000 kilograms (308,646 pounds) pressure, permits the test of bodies (including pillars of brick or natural stone) of 1 meter height and of 55 by 55^{cm} diameter. Tests can be made both as to the stone and to the binding material.

A 20-fold lever is employed in testing the resistance of cylindrical bodies, and a 30-fold lever is used in the testing of elasticity of roofing paper and of the adhesiveness of mortar.

Tests of clay pipes are made by horizontal pressure of from 20 to 30 atmospheres to 1 to 3 meters internal diameter.

The tests of cement are made in accordance with the rulings of November 12, 1878, of the Ministry of commerce, industry, and public works.

With regard to adhesiveness of cement and cement mortar, a 50-fold normal lever apparatus is used with sample piece of 59 centimeters at the point of rupture; in pressure tests and break tests the hydraulic press, a 500-fold lever and a 20-fold lever are employed.

With regard to fineness of grain, sieves of from 600, 900, and 5,000 meshes to the square centimeter are employed. In all cement tests the officially introduced normal sand alone finds employment.

A horizontal disk of emery, operated by a gas-motor, which also operates a diamond plane machine used in finishing the bodies for pressure tests, is employed in the experiments as to the wear of the building materials.

VII.—THE VETERINARY COLLEGE AND THE MILITARY SCHOOL OF FARRIERY.

[Omitted here.]

VIII.—OTHER SCIENTIFIC AND MEDICAL INSTITUTES.

The Astro-physical Observatory.—Until the first quarter of the present century astronomy employed itself almost exclusively with the discovery of mechanics of the heavens; and the physical conditions of the stars received but occasional, and more accidental than systematic, attention from a few astronomers. Astro-physics had not fully developed itself as a branch of astronomy. When, then, later, more attention was given to certain changes on the surface of the sun and to other phenomena, the observations were restricted to the use of such apparatus as was then found in a well-equipped observatory, and not much difficulty was experienced in combining new requirements with the existing apparatus. It was not until the more extending field of study brought physical and chemical examinations in contact with the astronomical, and more especially since the application of the spectral analysis upon the astro-physical investigations proved the most powerful means for the discovery of the substances of which the heavenly bodies are composed, and since photography had begun to be employed in fixing certain events observed in the skies, it was not until then that the needs of separate establishments was fully recognized, equipped with suitable instruments not required by observatories of the older kind. Such an establishment is now found in the Astro-physical Observatory at Potsdam.

As early as 1860 it was suggested to establish, in the vicinity of Berlin, an Observatory for the physical examination of the sun. The conditions however were then not favorable, and it was not until 1871 that the first steps were taken towards the realization of the project. The Crown Prince, whose attention had been called to the effort by Prof. D. Schellbach, managed to have propositions invited with regard to the subject from the director of the Berlin Observatory to which it was planned to connect the proposed solar observatory. To this the director responded on September 30, 1871, by submitting a memorial. It was proposed to establish an observatory in the vicinity of Berlin well-equipped for the direct, spectroscopic, and photographic investigation of the surface of the sun, this observatory at the same time to be the central station for magnetic and meteorological observations.

The Academy of Sciences, at the request of the Government, on April 29, 1872, while recognizing the interest of the subject, objected to the execution of the proposition from the stand-point that the scientific requirements would demand the establishment of two institutes, of which one should be devoted to astro-physics and the other to tellurian physics; it opposed an organic combination of the two on the ground that its scope would be too large for a successful administration. In this case however the observations of the sun would have to form but a part of the labors of the astro-physical institute.

The Minister of Education then called together a special commission under the presidency of Privy Counsellor E. du Bois-Reymond, which, in accordance with the academic consideration, recommended at first the establishment of an astrophysical observatory, with the proviso, however, that, in the case of uncertainty of the early erection of a tellurian observatory, such magnetic observations should be provided for which are of essential interest in the study of the sun's activity. A plan was prepared with regard to the establishment, organization, and equipment of the observatory, which was accepted by the Royal Government and sanctioned by legislative action of 1873–'74.

The cost of the building for astrophysical observations was 874,000 mark ($218,500).

The budget for the Observatory is 71,600 mark ($17,900), of which 42,000 ($10,500) are paid out for salaries and compensation.

The astro-physical institute is not an establishment for teaching, but is intended exclusively for the scientific investigation of this new branch of astronomy. Since the short existence of the Observatory many scientists have taken part in its works, most especially as they relate to spectral analysis and photography.

The Observatory possesses the following larger telescopes:

One large refractor of 29.8 centimeters (11¾ inches) aperture and 5.4 meters focus. Objective by Schröder, mountings by A. Repsold, Hamburg. Placed in the central cupola. Refractor by Grubb of Dublin, aperture 20 centimeters (7⅞ inches), focus 3.2 meters; in place in the west cupola. A refractor by Steinheil, 13.5 centimeters aperture and 2.2 focus; placed in the east cupola. Photo-heliograph and mountings by Repsold, objective by Steinheil, aperture 16 centimeters and focus 4 meters; placed in the southern addition to the main building. Comet-seeker by Reinfelder and Hertel.

The observatory possesses a large number of spectral apparatus, among them one large by Schröder, for observations of the sun, and one spectroscope by the same. John Browning furnished two spectroscopes for observations of the stars and for solar protuberances, respectively. Other spectroscopes emanate from the shops of Hilger, London; Schmidt & Hænsch, Berlin; Repsold, Hamburg; Topper, Potsdam. In connection with the prism apparatus some finely graduated grates of glass and metal for the representation of grate spectra may be mentioned; among these are some excellent ones by Wanschaff, Berlin.

In addition to the larger apparatus for photographic work the equipment for laboratory work is excellent. For photometric representations of spectral analysis a large Zöllner photometer by Wanschaff and several smaller photometers are employed.

The Geodetic Institute.—The purpose of the Institute is the improvement of geodesy by scientific investigations. At the time of its establishment it was also intrusted with the Prussian portion of the European Geodetic Survey; in fact that undertaking was the cause of the establishment of the institute. The European Geodetic Survey developed itself out of the Middle European Survey. In 1862, at the suggestion of Lieutenant-General Baeyer—the collaborator of Bessels in the Eastern Prussian Geodetic Survey and chief of the trigonometrical division of the general staff of the Army—a convention took place of delegates of most of the German States, Austria, Switzerland, Italy, Denmark, Sweden, Norway, etc., for the Geodetic Survey of the represented States.

The Institute is divided into four sections, each of which has a chief, one regular and one temporary assistant. The field work of the summer is computed during the winter and published.

During Baeyer's administration, from 1869 to his death, September 10, 1885, the Institute measured the longitude of sixty-three places, obtained longitudinal differences of twenty-one places, and completed the astronomical work of twenty-seven stations of the trigonometrical system. The triangles were completed by elaborate work along the Rhine from Belgium to Switzerland and from the Middelrhein through Thuringia to Berlin, etc., and provided with two new base lines, obtained by means of measurements with a platin-iridium apparatus by Brunner at Paris. Pendulum observations were made at thirteen places, older ones corrected, new levels obtained from Swinemünde to Constanz and to the frontier of the Netherlands and of the mean water height of the Baltic Sea. The greater portion of the work has already been published.

The Museum of Ethnography.—Since the middle of the present century ethnology and anthropology have begun to take definite shape as a science with well-pronounced objects and purposes.

Of the exotic material which reached Europe as a result of the discoveries of, and scientific journeys in foreign continents the objects which could be incorporated in natural history collections found their proper places in the zoological or botanical museums, while the objects relating to man did not have any relation to the special studies of the times, and they therefore became more the subject of curiosity and wonder than of earnest consideration.

These objects generally received a place in the section of foreign curiosities in most the cabinets enjoying a princely protection, and these sections followed the movement of their respective museums.

Thus in Berlin, where from the days of the colonial policy of the Great Elector an interest had continued in that direction, and where the "Silver Chamber" of the Royal Castle contained many ethnological articles, which, in the union of the old and the new museum were transferred under the designation of "Ethnographic division."

In the new Museum one hall contained the pre-historic collections of Prussia, while three other halls contained everything of an ethnological character which had found its way thither by donation or purchase. The same arrangement existed in London, Copenhagen, Lyons, Munich, Göttingen, Wien, etc.

In the middle of the present century however, when anthropological and ethnological studies experienced that powerful impulse which has produced their rapid development and when the movement originated in England and France was transferred to Germany, it soon became apparent that the space devoted to these collections demanded an immediate enlargement.

Deliberations to that effect commenced in 1873 and continued during the years 1873–1876, and finally, in 1880 resulted in the accomplishment of the desire for an independent ethnographical museum.

The building was finished in the spring of 1886, at a cost of 2,000,000 mark ($500,000).

The Standard Measures Commission.—Article 18 of the law of August 17, 1868, relating to weights and measures within the North German Confederation provides for the establishment of a standard measures commission, with its seat at Berlin and duties specified as follows:

"A standard measures commission is to be appointed by the Confederation. The same is to be located at Berlin. It is the duty of the commission to see that all the measures within the Confederation are conducted on a uniform system. It has to prepare the standard and to communicate the same to all measure bureaus throughout the Confederation, and for that reason it is to be equipped with all necessary apparatus and instruments. The standard measures commission has to issue orders and prescriptions with regard to material, form, designation, and other conditions of weights and measures, and to determine the limit of errors. It decides on the kind of scales to be used in public and for special industrial purposes, and determines their accuracy. It is to issue all necessary prescriptions and formulas for the manufacture of weights and measures and to test the accuracy of any articles which may be offered for the purpose. The standard measures commission has to regulate the fees to be exacted by the measuring establishments, and in fact the entire technics of weights and measures.

"All bureaus of measures within the North German Confederation, in addition to the local stamp, have to use the mark of the standard measures commission which will be furnished for the purpose.

"The standard weights and measures are to be preserved after true and attested copies have been made of them."

Article 22 of the law providing for the introduction of the new weights and measures, on and after January 1, 1870, called for the immediate formation of the conference in order to enable the bureau of measures to have the necessary facilities and standards in time for the testing and approving of any new weights and measures brought before them.

As a result of the conference metrical regulations were established on July 16, 1869; and on July 21, 1869, the business instructions and the composition and organization of the standard measures commission were completed.

According to the rules adopted, the standard measures commission was to consist of the director, assisted by the regular force required for the business affairs of the commission, and of attached members to be called in for conference; their number is to correspond to the demand. They are recommended for appointment of five years by the director and confirmed by the chancellor of the Empire. Their office is an honorary one, but members, non-residents of Berlin, are refunded expenses incurred on occasion of official business conferences at Berlin. All projects relating to the entire system of measures are to be considered in full session.

The business instructions provide that the director be assisted by two experienced technicians, well versed with gauging; further, some person or persons skilled in making mathematical computations or physical examinations, a secretary, a messenger, and the required number of clerks and copyists.

The scientific publications of the commission relate to measurements, weighings, barometric, thermometric, and areometric examinations, to which are added experiments on alloys, inflammability of petroleum, etc.

The commission employs the following apparatus: universal comparator, by A. Repsold Sons, at Hamburg; longitudinal comparator, by A. Repsold Sons, at Hamburg; universal cathetometer (vertical, transversal, and longitudinal comparator), by C. Bamberg, at Berlin; kilogram scale, within hermetical glass bell, by P. Stückrath, at Berlin; scale of 100 grams by P. Stückrath; scales of 500 milligrams, by P. Stückrath; standard barometer and manometer, with vertical comparator; thermometers, and apparatus for the testing of air thermometers, by Fuess.

In addition to the above the commission possesses a number of apparatus and instruments, consisting of comparators, scales, barometers, barographs, thermometers, alcoholometers, areometers, hollow measures, gas-meters, cubic apparatus, petroleum tester, crucibles and furnace for alloy, measuring-scales of platinum, brass, crystal, iron, and aluminium.

The more delicate apparatus are mounted in rooms having double walls of zinc, the intervening space of which is tested by gas for the preservation of a constant temperature. They are lighted by Siemens's regenerative shallow burners, the radiation of which is prevented by cloaks of water, while the light is carried through a hollow lens filled with a solution of alum and is reflected by mirrors upon the scales to be read. The instruments have been isolated by placing them upon pillars extending in wells to a depth of 8 meters.

The Hydrographic Office of the Imperial Admiralty.—On December 18, 1853, Professor Berghaus suggested to Prince Adalbert, of Prussia, the establishment of a hydrographic office. The plan was accepted, but its execution delayed on account of insufficient funds. The desirability and necessity of such an establishment becoming more urgent, a Direction of Navigation was established on June 28, 1854, to form a technical division of the naval office at Danzig. On account of insufficient force the institute had to limit its operations to the needs of the war navy.

The Direction of Navigation at Danzig was dissolved on September 25, 1861, and in its place a "Hydrographic Bureau" was established to form a section of Division X of the Ministry of Navy. Its functions remained unchanged, and in addition it was commissioned with the current work of sea charts and with the collecting and tabulating of nautical informations, which, between the years 1863 and 1868, were furnished to the vessels of the war navy. In order to extend its usefulness to the mercantile marine these informations were published, since 1869, under the title of "Nachrichten für Seefahrer," at first as additions to the "Preussisches Handelsblatt" and since 1870 as additions to the "Marine Verordnungs-Blatt."

The demands on the Hydrographic Office increased with the rapid increase of the war navy, until it became unable to do all the work expected of it. In January, 1874, therefore, it was enlarged and appointed a separate division of the Admiralty, with the following personnel:

One full captain (or admiral), in charge; two chiefs of division, five section chiefs (including the chief of the Wilhelmshaven Observatory), five assistants, one librarian, and a number of draughtsmen, engravers, and mechanics.

For the business administration the following clerical force was appointed: Two chiefs of bureaus, two registrars, two secretaries of chancelry, and the required subordinates.

The Hydrographic Office in first line serves the interests of the imperial navy, but its advantages are extended to the mercantile navy as far as possible.

A survey of the entire territory of the North and Baltic Seas was begun in 1867 and completed in 1879; from 1880 to 1884 test measurements were made, scientific experiments instituted along the German coasts, and the knowledge of the physical conditions of the native seas improved.

The Central Telegraph Bureau and the Telephone Service.—With regard to its importance in the telegraphic intercourse the Bureau at Berlin occupies the first place among the telegraphic stations of the Empire. It is attached to the second division of the Imperial Post-office Department and serves as center of the telegraphic and pneumatic intercourse for Berlin.

A few figures will exhibit the importance of the establishment and afford an illustration of its operations.

The service of the Central Bureau gives employment to four hundred and ninety-two officials and one hundred and twenty-eight subordinates.

The telegraphing business requires the application of fifty-four type apparatus, Hughes's construction, one hundred and seventy-eight Morse apparatus, and fifty-one apparatus of a peculiar construction. Two hundred and eighty-two wires center at Berlin; of these fifty-six are underground and serve the larger circuit, thirty-seven, seventy, and twenty-eight overhead wires are used for foreign, the larger and smaller domestic circuits, respectively, while fifty-six underground wires accommodate the city trade. Within the city limits all wires, with the exception of those used in telephoning, are placed underground.

One hundred and twenty-four batteries, representing 7,350 elements, together with eight batteries for special purposes representing 290 elements, are employed in the central office.

The Berlin service comprises 351.8 kilometers (218.6 miles) of lines, with 2,428.5 kilometers (1,509 miles) of wire; the pneumatic service comprises 40.5 kilometers (25 miles) of lines. with 46.3 kilometers (29 miles) of tubes.

The pneumatic line commences at the Central Telegraph Bureau, radiates in six principal directions, and together with the branch lines forms connection with thirty-three pneumatic offices. Each line runs one train every fifteen minutes in either direction, with a velocity of 1,000 meters (3281 feet) per minute. The pressure and vacuum are produced at eight stations, each of which is provided with a double set of engines of a total force of 133 horse-power. Forty-four compartments of a total capacity of 772 cubic meters serve to hold the compressed air and the air ejected by the tubes. Each line controls a special series of signals.

The telephone service was established in 1881. At the beginning two central offices were established. Each of these received two switchboards for fifty plugs each. The great advantages of immediate personal communication and the extraordinary simplicity of the arrangement became so apparent that this mode of communication soon came into general use. The increase is best illustrated by stating that from the end of November, 1881, to June, 1886, the number of participants had increased from 442 to 5,194, while the line had increased from 1,319 to 10,477 kilometers (819 to 6,510 miles.)

In order to enable the subscribers to communicate with their own homes or places of business from more distant parts and also to throw the service open to general utility, public stations were established which could be used by any one so desirous upon the payment of a small fee. At present there are twelve such stations.

The constant increase in the number of telephones rendered it desirable to extend the telephonic service beyond the city limits. The progress made in this field of technic, especially in the construction of long-distance microphones, rendered it practicable to establish, in November, 1882, connection between Berlin and Charlottenburg; in May, 1883, it was extended to Berlin and Potsdam; in December 1883 connection was made between the exchanges of Berlin and Magdeburg; in the years 1884 and 1885 with the surburban places: Westend, Köpenick, Steglitz, Rixdorf, Gross Lichterfelde, Weissensee, Pankow, Rummelsburg, Friedenau, and Grünau.

The greatest distance of 347 kilometers (215 miles) was accomplished between Berlin and Hanover, and a large number is projected between Berlin and Halle, Breslau, Leipzig, Hamburg, etc.

The total cost of construction of the telephone service in Berlin to and including the year 1884–'85 has been about 2,000,000 mark ($500,-000). Large amounts are required for the support and changes of lines and for their maintenance and operation.

Until the beginning of the year 1886 the conduct and supervision of the telephone service was inthe hands of the Central Telegraph Bureau; the extent and growing demand however made it desirable to establish the service on an independent basis with a rank of an office of the first class. The service, at present employs two hundred and seventy-five regular officials with a corresponding number of subordinates.

The principal supervision of the erection of buildings for the telephonic service and of its administration, belongs to the Imperial Post-Office Department at Berlin, which has established a special division for the purpose. This is in charge of a councilor of post assisted by a telegraph inspector and ten officials for the business affairs of the bureau. To these, ten officials have been added who are employed in technical affairs, that is, in the preparation of plans and execution of the necessary building and changes in the lines and who have charge of a large number of laborers. And all decisions in telephonic affairs are rendered by this second division of the imperial post-office department.

The Telegraph Workshop, etc.—The duties of the telegraph apparatus workshop comprise: (1) The manufacture of apparatus and parts of such, as well as of the materials and tools required; (2) the changes and repairs to existing apparatus; (3) the making of contracts for the manufacture of apparatus and their parts, tools, materials, etc.; (4) the testing of apparatus, tools, materials, or changes or repairs made at private shops; (5) the examination of the bills; (6) the care and storage of apparatus and their parts; (7) the transmission of apparatus and their parts; (8) the sale of condemned material; (9) the keeping of accounts with regard to tools, apparatus, and their parts, materials and equipment; (10) the keeping of a list of applicants for mechanical positions; (11) the employment of assistant mechanics appointed by the post-office department.

The telegraph workshop is therefore divided into three branches, viz: (*a*) the bureau, (*b*) the mechanical work shop, (*c*) the carpenter shop and shipping office.

The bureau gives employment to one official of the first class, and three of the second, while the mechanical workshop employs one official of the first class who supervises the force, consisting of twenty mechanics.

Each mechanic has a separate and completely equipped place assigned within the shop.

The cable examination-room serves in the first line for continued measurements of the underground lines of Berlin, which are made once a week; it is furthermore used for the accurate measurements and experiments with new batteries, apparatus, and switches, etc. The currents of the two underground cables, Berlin and Thorn and Berlin and Dresden, are measured by the aid of two self-registering apparatus located in the testing-room.

The use of underground cables made it desirable to test their electrical properties at regular, short intervals by measuring the resistance of the copper wire, the isolating resistance, etc., of the insulating material, etc. This had the effect of giving full information of the state of the cable at all times; any mechanical injury was at once indicated, and could be repaired without delay. These regular measurements offered an opportunity for the instruction to a large number of officials located throughout the Empire, thus enabling them to be of service in case of emergency.

The Postal Museum.—The collections of the postal museum are located on the ground floor of the monumental building of the central Post-Office Department of the German Empire.

The Postmaster-General, soon after entering upon his duties, endeavored to interest the official bureaus, private individuals, artists, scientists, etc., in his ideas, and succeeded therein in such a measure that at the beginning of the present decade already a pretty complete representation from the beginning of communication to the present days, was presented in the collections of the postal museum.

In the mean time, by the incorporation, in 1876, of the telegraphy with the post, all the apparatus, models, materials, etc., collected by the former general director of telegraphy were transferred to the older sister establishment, and this collection being very rich and complete, the telegraph division of the postal bureau offers to the technician and to the physical science a rich source for earnest study, and especially a true historical picture of the development of the telegraph.

The postal division, of course, is still more extended, for the history of communication is as old as man himself. In the museums are represented the Egyptians, Assyrians, Persians, Hebrews, and other people of antiquity; Egyptian hieroglyphs, papyrus with hieratic writing, and Niniveh writing upon terra-cotta plates are the proofs which those people offer. The little plates of the Greeks and Romans which were laid before the oracle of Dodons, the *skytale* written upon parchment, the well executed imitations of the rare "*tabellæ duplices et triplices*" and the "*Diptyches*," distributed by the Roman consuls upon the commencement of their terms of office, conclude the antiquity. The gradual development of more regulated forms of later periods, including the Middle Ages, are illustrated by precious samples of writings emanating from the contemplative quiet of the monasteries, representations of boats, wagons and teams, streets, ships, etc., etc., while the modern cosmopolitan character of the "post" is represented by natural models of all peoples and countries. Everything is represented, from the most primitive row-boat to the highly elegant steamer, from the "dog post" to the six-horse postal carriage, railway post, pigeon post, field post, etc. Numerous illustrations of the homes of the post in all zones, and models of the stately buildings of modern times, complete the panorama.

IX.—THE ROYAL LIBRARY.

The establishment of a public library dates back to the year 1661, and is owing to the Grand Elector who ordered the collection of the fragments of the monasterial libraries and had them combined with the library of the castle, forming a collection comprising 1,618 European and Oriental manuscripts and 20,600 printed works, representing about 90,000 volumes. Frederick I added to it the purchased Spanheim collection of books, and Frederick II the library and collection of charts of Colonel Quintus Icilius and other valuable purchases. The present building was erected in the years 1774–1780 by order of Frederick the Great; the books were transferred to it in 1782, and the reading-room was opened in 1784.

According to the report of the director, in 1836, the library contained about 200,000 printed volumes, and 4,611 manuscripts. The growth of the library was such, that toward the end of the reign of Frederick William III, the lower floor of the building—used for the storage of books, had to be applied to the use of exposition. It was re-modelled during the years 1840 to 1842, and divided into two stories. The burning in 1843, of the Royal Opera House, immediately opposite the library, called attention to the importance of fire-proofing the building, the provision of iron stairways, doors, etc., and of suitable water reservoirs.

The extraordinary increase of the collections—amounting at present (1886) to 800,000 volumes, and 20,000 manuscripts, rendered it necessary to divide the elevation of the stories by means of iron ceilings, and to use a portion of the attic for the storage of books.

The budget for 1886–'87 allowed 96,000 mark ($24,000) for the purchase of books, manuscripts, journals, music, charts, and illustrations, and for the necessary expense for the binding of books.

X.—THE ROYAL BUREAU OF ENGRAVING AND PRINTING.

In 1850, when the Finance Department had under advisement the best method for preventing the manufacture of counterfeit money, a proposition was made to have all paper money and securities made at some central establishment. On April 30, 1851, a royal decree authorized the establishment of a bureau for the manufacture of paper money, bonds, and other securities, and a building was purchased for the purpose at a cost of 5,380 thaler ($4,135).

In 1852, the establishment commenced operations with a personnel consisting of four officials, two messengers, and fifteen laborers. At present nine hundred persons are employed.

On August 1, 1852, the manufacture of postage stamps, stamped envelopes, newspaper wrappers, which until then had been made by private contract, was given to the bureau together with all the necessary machines and implements for the manufacture of the same, which had been the property of the Post-Office Department.

At the close of the year 1860, when the Royal Lithographing Institute became combined with the Bureau of Engraving and Printing, it was found necessary to employ copper engraving with photographic and galvanoplastic processes in engraving of charts instead of lithography, and this change again was productive of an enlargement of the office together with a corresponding increase in machinery.

A great source of revenue and profit was offered in the enormous supply of postage stamps and cards required by the totally unexpected development of the postal service. This kind of work had formerly been performed by the printing office of Decker, which by decree of May 23, 1877, had been purchased by the German Government for the sum of 6,780,000 mark ($1,695,000) and had been placed under the jurisdiction of the Postmaster-General.

By law of May 15, 1879, the Prussian Bureau of Engraving was purchased by the Imperial Government for a consideration of 3,573,000 mark ($893,250) and consolidated with the printing establishment under the name, "Royal Bureau of Engraving and Printing."

The amalgamation took place at once from a business point of view, the general supervision remaining in the hands of the chief of the German post and telegraph administration, in whose bureau a separate division was established under the name "Director of the Royal Bureau of Engraving and Printing."

In order to accommodate the increased force of the combined offices, the adjacent buildings were purchased in May, 1879, for the sum of 517,500 mark ($129,375); the tearing down of the old buildings began at once and in the autumn of 1881 the new building was ready for occupation.

The bureau at present employs ninety-five artists and regular mechanics, and seven hundred and seventy laborers (male and female), apprentices, and porters.

The work of the bureau increases from year to year although a great deal of it, not involving money or bonds, is now turned over to private industries.

At present the ordinary work required by the Government and bureaus represents about 120,000,000 sheets, of which the Imperial Post and Telegraph Administration uses about 13,000,000 sheets and about 60,000,000 cards, independent of the large amount of work ordered from private firms.

HERTZ'S RESEARCHES ON ELECTRICAL OSCILLATIONS.[*]

BY G. W. DE TUNZELMANN, B. SC.

H. Hertz has been engaged for some time past in a series of researches on electrical oscillations, which have led to results of very exceptional importance, and as these results throw considerable light on the nature of electrical action, it will be of interest to have a connected account of the investigations, to which I therefore propose to devote a short series of papers.

In Hertz's first paper on the subject, viz, "On Very Rapid Electrical Oscillations" (Wiedemann's *Annalen*, 1887, vol. XXXI, page 421), he refers to a paper by Colley, "On Some New Methods for Observing Electrical Oscillations, with Applications" (*ibid.*, vol. XXVI, page 432), who calls attention to the fact that Sir William Thompson in 1853, showed the possibility of producing electrical oscillations by the discharge of a charged conductor, and gives references to all the investigations in the same direction which were known to him.[†]

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[*] From *The Electrician* (published in London), Sept. 14 to Nov. 16, 1888, vol. XXI, pp. 587, 625, 663, 696, 725, 757, 788; vol. XXII, pp. 16, 41.

[†] For the benefit of readers who may wish to pursue the subject further the list is reproduced below:—

[Joseph Henry was the first to experimentally demonstrate the oscillation of electrical discharges, in June, 1842. *Proceedings American Philosoph. Soc.*, vol. II, pages 193-196. Also, "Scientific Writings of Joseph Henry," published by the Smithsonian Institution; vol. I, page 200.]

Von Helmholtz, "Erhaltung der Kraft." Berlin, 1847: Translated and published in Tyndall's "Scientific Memoirs," London, 1853, vol. I, page 143. Also, "Gesammelte Abhandlungen," vol. I, page 531.

Sir William Thomson, *L. E. D. Phil. Mag.* 1853, vol. V, page 400. Also, "Mathematical and Physical Papers," vol. I, page 540.

Feddersen, Poggendorff's *Annalen*, 1858, vol. CIII, page 69; 1859, vol. CVIII, page 497; 1861, vol. CXII, page 452; 1861, vol. CXIII, page 437; 1862, vol. CXV, page 336; 1862, vol. CXVI, page 132.

Kirchhoff, "Gesammelte Abhandlungen," page 168, containing remarks on and corrections of some of Feddersen's results.

Von Oettingen, Poggendorff's *Annalen*, 1862, vol. CXV, page 513; and Jubelband of same, 1874, page 269.

Bernstein, Poggendorff's *Annalen*, 1871, vol. CXLII, pages 54–88.

Schiller, Poggendorff's *Annalen*, 1872, vol. CLII, page 535.

Mouton, Thèse, Paris, 1876. *Journal des Physique*, 1876, vol. VI, pages 5 and 46.

According to these investigations, the electrical oscillations produced in an open circuit by means of an induction coil are measured by ten thousandths of a second, while in the case of the oscillatory discharge of a Leyden jar they are about a hundred times as rapid, as was shown by Feddersen.

According to theory, still more rapid oscillations should be possible in an open circuit of wire of good conducting material, provided its ends are not connected with conductors of any considerable capacity; but it is not possible to determine from theory whether measurable oscillations are actually produced. Some observations of Hertz's led him to believe that under certain circumstances oscillations of this kind were produced, and his researches show that this is so, and that the oscillations are about a hundred times as rapid as those observed by Feddersen; so that their periods are measured by hundred millionths of a second, and they therefore occupy a position intermediate between acoustic and luminous vibrations.

Preliminary Experiments.—It is known that if in the secondary circuit of an induction coil there be inserted, in addition to the ordinary air space, across which sparks pass, a Riess spark micrometer, with its poles joined by a long wire, the discharge will pass across the air space of the micrometer in preference to following the path of least resistance through the wire, provided this air space does not exceed a certain limit, and it is upon this principle that lightning protectors for telegraph lines are constructed. It might be expected that the sparks could be made to disappear by diminishing the length and resistance of the connecting wire; but Hertz finds that though the length of the sparks can be diminished in this way, it is almost impossible to get rid of them entirely, and they can still be observed when the balls of the micrometer are connected by a thick copper wire only a few centimeters in length.

This shows that there must be variations in the potential measureable in hundredths of a volt in a portion of the circuit only a few centimeters in length, and it also gives an indirect proof of the enormous rapidity of the discharge, for the difference of potential between the micrometer knobs can only be due to self-induction in the connecting wire. Now the time occupied by variations in the potential of one of the knobs must be of the same order as that in which these variations can be transmitted through a short length of a good conductor to the second knob. The resistance of the wire connecting the knobs is found to be without sensible effect on the results.

L. Lorenz, Wiedemann's *Annalen*, 1879, vol. VII, page 161.

Olearsky, Verhandlungen der Academie von Krakau, 1882, vol. VII, page 141.

Kolacek, Beiblätter zu Wiedemann's *Annalen*, 1883, vol. VII, page 541 (abstract of a paper published in the reports of the Bohemian Scientific Society in 1882).

Bichat et Blondlot, *Comptes Rendus*, 1882, vol. XCIV, page 1590.

Oberbeck, Wiedemann's *Annalen*, 1882, vol. XVII, pages 816 and 1040; 1883, vol. XIX, pages 213 and 265.

In Fig. 1, *A* is an induction coil and *B* a discharge. The wire connecting the knobs 1 and 2 of the spark micrometer *M*, consists of a rectangle, half a meter in length, of copper wire 2 millimeters in diameter. This rectangle is connected with the secondary circuit of the coil in the manner shown in the diagram; and when the coil is in action, sparks—sometimes several millimeters in length—are seen to pass between the knobs 1 and 2, showing that there are violent electrical oscillations, not only in the secondary circuit itself, but in any conductor in contact with it. This experiment shows even more clearly than the previous one that the rapidity of the oscillations is comparable with the velocity of transmission of electrical disturbances through the copper wire, which, according to all the evidence at our disposal, is nearly equal to the velocity of light.

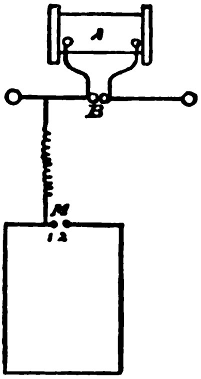

FIG. 1.

In order to obtain micrometer sparks some millimeters in length, a powerful induction coil is required, and the one used by Hertz was 52 centimeters in length and 20 centimeters in diameter, provided with a mercury contact breaker, and excited by six large Bunsen cells. The discharger terminals consisted of brass knobs 3 centimeters in diameter. The experiments showed that the phenomenon depends to a very great extent on the nature of the sparks at the discharger, the micrometer sparks being found to be much weaker when the discharge in the secondary circuit took place between two points, or between a point and a plate, than when knobs were used. The micrometer sparks were also found to be greatly enfeebled when the secondary discharge took place in a rarified gas, and also when the sparks in the secondary were less than half a centimeter in length, while on the other hand, if they exceeded 1½ centimeters the sparks could no longer be observed between the micrometer knobs. The length of secondary spark which was found to give the best results, and which was therefore employed in the further observations, was about three-quarters of a centimeter.

Very slight differences in the nature of the secondary sparks were found to have great effect on those at the micrometer, and Hertz states that after some practice he was able to determine at once from the sound and appearance of the secondary spark whether it was of a kind to give the most powerful effects at the micrometer. The sparks which gave the best results were of a brilliant white color, only slightly jagged, and accompanied by a sharp crack.

The influence of the spark is readily shown by increasing the distance between the discharger knobs beyond the striking distance, when the micrometer sparks disappear entirely, although the variations of potential are now greater than before. The length of the micrometer circuit has naturally an important influence on the length of the spark, as the greater its length the greater will be the retardation of the electrical wave in its passage through it from one knob of the micrometer to the other.

The material, the resistance, and the diameter, of the wire of which the micrometer circuit is formed, have very little influence on the spark. The potential variations can not therefore be due to the resistance, and this was to be expected, for the rate of propagation of an electrical disturbance along a conductor depends mainly on its capacity and co-efficient of self-induction, and only to a very small extent on its resistance. The length of the wire connecting the micrometer circuit with the secondary circuit of the coil is also found to have very little influence, provided it does not exceed a few meters in length. The electrical disturbances must therefore traverse it without undergoing any appreciable change. The position of the point of the micrometer circuit which is joined to the secondary circuit, is on the other hand of the greatest importance, as would be expected, for if the point is placed symmetrically with respect to the two micrometer knobs the variations of potential will reach the latter in the same phase, and there will be no effect, as is verified by observation. If the two branches of the micrometer circuit on each side of the point of contact of the connection with the secondary are not symmetrical, the spark can not be made to disappear entirely; but a minimum effect is obtained when the point of contact is about half-way between the micrometer knobs. This point may be called the null point.

Fig. 2 shows the arrangement employed, *e* being the null point of the rectangular circuit, which is 125 centimeters long by 80 centimeters broad. When the point of contact is at *a* or *b*, sparks of from 3 to 4 millimeters in length are observed, when it is at *e* no sparks are seen, but they can be made to re-appear by shifting the point of contact a few centimeters to the right or left of the null point. It should be noted that sparks only a few hundredths of a millimeter in length can be observed. If when the point of contact is at *e* another conductor is placed in contact with one of the micrometer knobs the sparks re-appear.

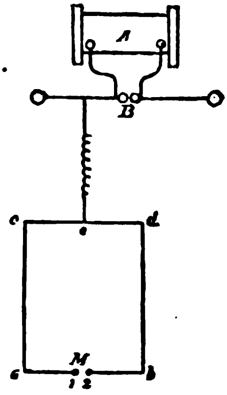

FIG. 2.

Now the addition of this conductor can not produce any alteration in the time taken by the disturbances proceeding from *e* to reach the knobs, and therefore the phenomenon can not be due simply to single waves in the directions *c a* and *d b* respectively, but must be due to repeated reflection of the waves until a condition of stationary vibration is attained, and the addition of the conductor to one of the knobs must diminish or prevent the reflection of the waves from that terminal. It must be assumed then, that definite oscillations are set up in the micrometer circuit just as an elastic bar is thrown into definite vibrations by blows from a hammer. If this assumtion is correct, the condition for the disappearance of the sparks at *M* will be that the vibration periods of the two branches *e* 1 and *e* 2 shall be equal. These periods are determined by the products of the coefficients of self-induction of these conductors into the capacity of their terminals, and are practically independent of their resistances.

In confirmation of this, it is found that if when the point of contact is at *e* and the sparks have been made to re-appear by connecting a conductor with one of the knobs, this conductor is replaced by one of greater capacity, sparking is greatly increased. If a conductor of equal capacity is connected with the other micrometer knob the sparks disappear again; the effect of the first conductor can also be counteracted by shifting the point of contact towards it, thereby diminishing the self-induction in that branch. The conclusions were further confirmed by the results obtained when coils of copper wire were inserted into one or other and then into both of the branches of the micrometer circuit.

Hertz supposed that as the self-induction of iron wires is, for slow alternations, from eight to ten times that of copper wires, therefore a short iron wire would balance a long copper one; but this was not found to be the case, and he concludes that, owing to the great rapidity of the alternations, the magnetism of the iron is unable to follow them and therefore has no effect on the self-induction.*

Induction phenomena in open circuits.—In order to test more fully his conclusion that the sparks obtained in the experiments described in the previous section, were due to self-induction, Dr. Hertz placed a rectangle of copper wire with sides 10 and 20 centimeters in length, respectively, broken by a short air space, with one of its sides parallel and close to various portions of the secondary circuit of the coil, and of the micrometer circuit, with solid di-electrics interposed, to obviate the possibility of sparking across, and he found that sparking in this rectangle invariably accompanied the discharges of the induction coil, the longest sparks being obtained when a side of the rectangle was close to the discharger.

A copper wire, *i g h* (Fig. 3), was next attached to the discharger, and a side of the micrometer circuit, which was supported on an insulating stand, was placed parallel to a portion of this wire, as shown in the diagram. The sparks at *M* were then found to be extremely feeble

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* In a note in Wiedemann's *Annalen*, vol. XXXI, page 543, Dr. Hertz statest hat since the publication of his paper in the same volume, he had found that Von Bezold had published a paper in 1870 (Poggendorff's *Annalen*, vol. CXL, page 541), in which he had arrived by a different method of experimenting at similar results and conclusions as those given by him under the head of Preliminary Experiments.

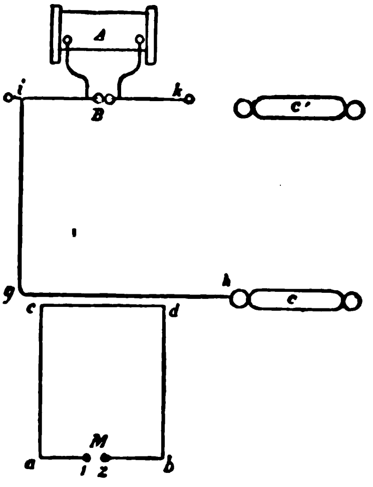

Fig. 3.

until a conductor, *C*, was attached to the free end, *h*, of the copper wire, when they increased to 1 or 2 millimeters in length. That the action of *C* was not an electrostatic one was shown by its producing no effect when attached at *g* instead of at *h*. When the knobs of the discharger *B* were so far separated that no sparking took place there, the sparks at *M* were also found to disappear, showing that these were due to the sudden discharges and not to the charging current. The sparks at the discharger which produced the most effect at the micrometer were of the same character as those described in my last paper. Sparks were also found to occur between the micrometer circuit and insulated conductors in its vicinity. The sparks became much shorter when conductors of larger capacity were attached to the micrometer knobs, or when these were touched by the hand, showing that the quantity of electricity in motion was too small to charge these conductors to a similarly high potential. Joining the micrometer knobs by a wet thread did not perceptibly diminish the strength of the sparks. The effects in the micrometer circuit were not of sufficient strength to produce any sensation when it was touched or the circuit completed through the body.

In order to obtain further confirmation of the oscillatory nature of the current in the circuit *k i h g* (Fig. 3), the conductor *C* was again attached to *h*, and the micrometer knobs drawn apart until sparks only passed singly. A second conductor, *C'*, as nearly as possible similar to *C*, was then attached to *k*, when a stream of sparks was immediately observed, and it continued when the knobs were drawn still further apart. This effect could not be ascribed to a direct action of the portion of circuit *i k*, for in this case the action of the portion of circuit *g h* would be weakened, and it must therefore have consisted in *C'* acting on the discharging current of *C*, a result which would be quite incomprehensible unless the current in *g h* were of an oscillatory character.

Since an oscillatory motion between *C* and *C'* is essential for the production of powerful inductive effects, it will not be sufficient for the spark to occur in an exceedingly short time, but the resistance must at the same time not exceed certain limits. The inductive effects will therefore be excessively small if the induction coil included in the circuit *C C'* is replaced by an electrical machine alternately charging and discharging itself, or if too small an induction coil is used; or again if the air space between the discharger knobs is too great, as in all these cases the motion ceases to be oscillatory.

The reason that the discharges of a powerful induction coil gives rise to oscillatory motion is that firstly, it charges the terminals C and C' to a high potential; secondly, it produces a sudden spark in the intervening circuit; and thirdly, as soon as the discharge begins the resistance of the air space is so much reduced as to allow of oscillatory motion being set up. If the terminal conductors are of very large capacity, for example, if the terminals are in connection with a battery, the current of discharge may indefinitely reduce the resistance of the air space, but when the terminal conductors are of small capacity this must be done by a separate discharge, and therefore under the conditions of the author's experiments, an induction coil was absolutely essential for the production of the oscillations.

As the induced sparks in the experiment last described were several millimeters in length, the author modified it by using the arrangement shown in Fig. 4, and greatly increasing the distance between the micrometer circuit and the secondary circuit of the induction coil. The terminal conductors C and C' were 3 meters apart, and the wire between them was of copper, 2 millimeters in diameter, with the discharger B at its center.

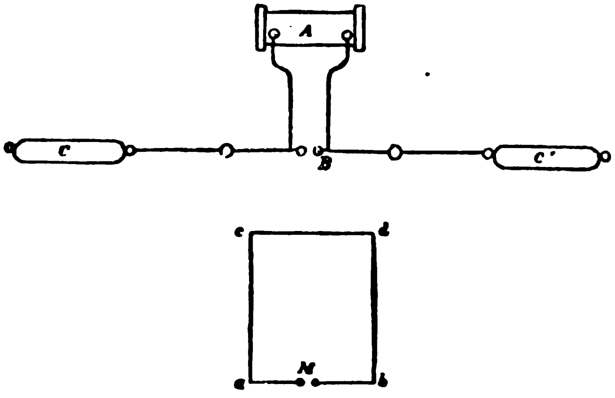

FIG. 4.

The micrometer circuit consisted, as in the preceding experiments, of a rectangle 80 centimeters broad by 120 centimeters long. With the nearest side of the micrometer circuit at a distance of half a millimeter from $C\ B\ C'$ sparks 2 millimeters in length were obtained at M, and, though the length of the sparks decreased rapidly as the distance of the micrometer circuit was increased, a continuous stream of sparks was still obtained at a distance of 1½ meters. The intervention of the observer's body between the micrometer circuit and the wire $C\ B\ C'$ produced no visible effect on the stream of sparks at M. That the effect was really due to the rectilinear conductor $C\ B\ C'$ was proved by the fact that when one or other, or both, halves of this conductor were removed the sparks at M ceased. The same effect was produced by drawing the knobs of the discharger *B* apart until sparks ceased to pass showing that the effect was not due to the electro-static potential difference of *C C′*, as this would be increased by separating the discharger knobs beyond sparking distance.

The closed micrometer circuit was then replaced by a straight copper wire, slightly shorter than the distance *C C′*, placed parallel to *C B C′*, and at a distance of 60 centimeters from it. This wire terminated in knobs, 10 centimeters in diameter, attached to insulating supports, and the spark micrometer divided it into two equal parts. Under these circumstances sparks were obtained at the micrometer as before.

With the rectilinear open micrometer circuit sparks were still observed at the micrometer when the discharger knobs of the secondary coil circuit were separated beyond sparking distance. This was of course due simply to electro-static induction, and shows that the oscillatory current in *C C′* was superposed upon the ordinary discharges. The electro-static action could be got rid of by joining the micrometer knobs by means of a damp thread. The conductivity of this thread was therefore sufficient to afford a passage to the comparatively slow alternations of the coil discharge, but was not sufficient to provide a passage for the immeasureably more rapid alternations of the oscillatory current. Considerable sparking took place at the micrometer when its distance from *C B C′* was 1 or 2 meters, and faint sparks were distinguishable up to 3 meters. At these distances it was not necessary to use the damp thread to get rid of the electro-static action, as owing to its diminishing more rapidly with increase of distance than the effect of the current induction, it was no longer able to produce sparks in the micrometer, as was proved by separating the discharger knobs beyond speaking distance, when sparks could no longer be perceived at the micrometer.

Resonance phenomena.—In order to determine whether, as some minor phenomena had led the author to suppose, the oscillations were of the nature of a regular vibration, he availed himself of the principle of resonance. According to this principle, an oscillatory current of definite period would, other conditions being the same, exert a much greater inductive effect upon one of equal period than upon one differing even slightly from it.*

If then two circuits are taken, having as nearly as possible equal vibration periods, the effect of one upon the other will be diminished by altering either the capacity or the co-efficient of self-induction of one of them, as a change in either of them would alter the period of vibration of the circuit.

This was carried out by means of an arrangement very similar to that of Fig. 4. The conductor *C C′* was replaced by a straight copper wire 2.6 meters in length and 5 millimeters in diameter, divided into two equal parts, as before, by a discharger. The discharger knobs were attached directly to the secondary terminals of the induction coil. Two hollow zinc spheres, 30 centimeters in diameter, were made to slide on the wire, one on each side of the discharger, and since, electrically speaking, these formed the terminals of the conductor, its length could be varied by altering their position. The micrometer circuit was chosen of such dimensions as to have, if the author's hypothesis were correct, a slightly shorter vibration period than that of $C\ C'$. It was formed of a square, with sides 75 centimeters in length, of copper wire 2 millimeters in diameter, and it was placed with its nearest side parallel to $C\ B\ C'$, and at a distance of 30 centimeters from it. The sparking distance at the micrometer was then found to be 0.9 millimeter. When the terminals of the micrometer circuit were placed in contact with two metal spheres, 8 centimeters in diameter, supported on insulating stands, the sparking distance could be increased up to 2.5 millimeters. When these were replaced by much larger spheres the sparking distance was diminished to a small fraction of a millimeter. Similar results were obtained on connecting the micrometer terminals with the plates of a Kohlrausch condenser. When the plates were far apart the increase of capacity increased the sparking distance, but when the plates were brought close together the sparking distances again fell to a very small value.

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* See Oberbeck, Wiedemann's *Annalen*, 1885, vol. XXVI, p. 245.

The simplest method of adjusting the capacity of the micrometer circuit is to suspend to its ends two parallel wires, the distance and lengths of which are capable of variation. By this means the author succeeded in increasing the sparking distance up to 3 millimeters, after which it diminished when the wires were either lengthened or shortened. The decrease of the sparking distance on increasing the capacity was naturally to be expected; but it would be difficult to understand, except on the principle of resonance, why a decrease of the capacity should have the same effect.

The experiments were then varied by diminishing the capacity of the circuit $C\ B\ C'$ so as to shorten its period of oscillation, and the results confirmed those previously obtained, and a series of experiments in which the lengths and capacities of the circuits were varied in different ways showed conclusively that the maximum effect does not depend on the conditions of either one of the two circuits, but on the existence of the proper relation between them.

When the two circuits were brought very close together, and the discharger knobs separated by an interval of 7 millimeters, sparks were obtained at the micrometer, which were also 7 millimeters in length, when the two circuits had been carefully adjusted to have the same period. The induced *electro-motive forces* must in this case have attained nearly as high a value as the inducing ones.

To show the effect of varying the co-efficient of self-induction, a series of rectangles $a\ b\ c\ d$ (Fig. 4), were taken, having a constant breadth $a\ b$, but a length $a\ c$ continually increasing from 10 centimeters up to 250 centimeters; it was found that the maximum effect was obtained with a length of 1.8 meters. The quantitative results of these experiments are shown in Fig. 5, in which the abscissæ of the curve are the double lengths of the rectangles, and the ordinates represent the corresponding maximum sparking distances. The sparking distances could not be determined with great exactness, but the errors were not sufficient to mask the general nature of the result.

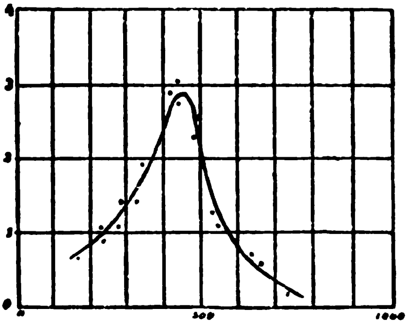

Fig. 5.

Curve showing relation between length of side of rectangle (taken as abscissa) and maximum sparking distance (taken as ordinate), the sides consisting of straight wires of varying lengths,

In a second series of experiments the sides *a c* and *b d* were formed of loose coils of wire which were gradually pulled out, and the result is shown in Fig. 6. It will be seen that the maximum sparking distance was attained for a somewhat greater length of side, which is explained by the fact that in the latter experiments the self-induction only was increased by increase of length, while in the former series the capacity was increased as well. Varying the resistance of the micrometer circuit by using copper and German silver wires of various diameters was found to have no effect on the period of oscillation, and extremely little on the sparking distance.

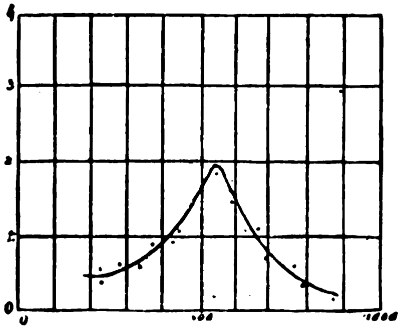

Fig. 6.

Curve showing relation between length of side of rectangle (taken as abscissa) and maximum sparking distance (taken as ordinate), the sides consisting of spirals gradually drawn out.

When the wire *c d* was surrounded by an iron tube, or when it was replaced by an iron wire, no perceptible effect was obtained, confirming the conclusion previously arrived at that the magnetism of the iron is unable to follow such rapid oscillations, and therefore exerts no appreciable effect.

Nodes.—The vibrations in the micrometer circuit which have been considered are the simplest ones possible, but not the only ones. While the potential at the ends alternates between two fixed limits, that at the central portion of the circuit retains a constant mean value. The electrical vibration therefore has a node at the center, and this will be the only nodal point. Its existence may be proved by placing a small insulated sphere close to various portions of the micrometer circuit while sparks are passing at the discharger of the coil, when it will be found that if the sphere is placed close to the center of the circuit the sparking will be very slight, increasing as the sphere is moved farther away. The sparking cannot however be entirely got rid of, and there is a better way of determining the existence and position of the node. After adjusting the two circuits to unison, and drawing the micrometer terminals so far apart that sparks can only be made to pass by means of resonant action, let different parts of the circuit be touched by a conductor of some capacity, when it will be found that the sparks disappear, owing to interference with the resonant action, except when the point of contact is at the center of the circuit. The author then endeavored to produce a vibration with two nodes, and for this purpose he modified the apparatus previously used by adding to the micrometer circuit a second rectangle *e f g h* exactly similar to the first (as shown in Fig. 7), and joining the points of the circuit near the terminals by wires 13 and 24, as shown in the diagram.

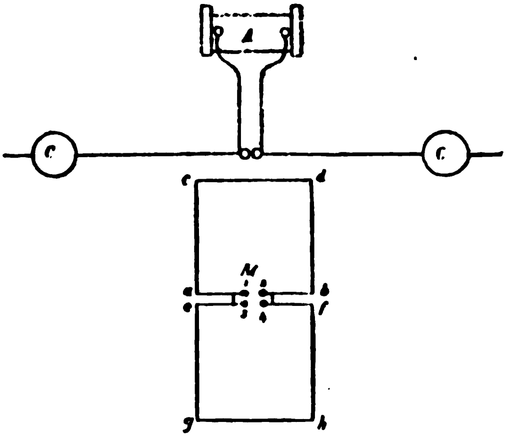

FIG. 7.

The whole system then formed a closed metallic circuit, the fundamental vibration of which would have two nodes. Since the period of this vibration would necessarily agree closely with that of each half of the circuit, and therefore with that of the circuit $C\ C'$, it was to be expected that the vibration would have a pair of loops at the junctions 1 and 3, and 2 and 4, and a pair of nodes at the middle points of $c\ d$ and $g\ h$. The vibrations were determined by measuring the sparking distance between the micrometer terminals 1 and 2. It was found that—contrary to what was expected—the addition of the second rectangle diminished this sparking distance from about 3 millimeters to about 1 millimeter. The existence of resonant action between the circuit $C\ C'$ and the micrometer circuit was however fully demonstrated, for any alteration in the circuit $e\ f\ g\ h$, whether it consisted in increasing or in decreasing its length, diminished the sparking distance. It was also found that much weaker sparking took place between $c\ d$ or $g\ h$ and an insulated sphere, than between $a\ e$ or $b\ f$ and the same sphere, showing that the nodes were in $c\ d$ and $g\ h$, as expected. Further, when the sphere was made to touch $c\ d$ or $g\ h$ it had no effect on the sparking distance of 1 and 2; but when the point of contact was at any other portion of the circuit the sparking distance was diminished, showing that these nodes did really belong to the vibration, the resonant action of which increased this sparking distance.

The wire joining the points 2 and 4 was then removed. As the strength of the induced oscillatory current should be zero at these points, the removal ought not to disturb the vibrations, and this was shown experimentally to be the case, the resonant effects and the position of the nodes remaining unchanged. The vibration with two nodal points was of course not the fundamental vibration of the circuit, which consisted of a vibration with a node between a and e, and for which the highest values of the potential were at the points 2 and 4.

When the spheres forming the terminals at these points were brought close together, slight sparking was found to take place between them, which was attributed to the excitation, though only to a small extent, of the fundamental vibration. This explanation was confirmed in the following manner: The sparks between 1 and 2 were broken off, leaving only the sparks between 2 and 4, which measured the intensity of the fundamental vibration. The period of vibration of the circuit $C\ C'$ was then increased by drawing it out to its full length, and thereby increasing its capacity, when it was observed that the sparking gradually increased to a maximum, and then began to diminish again. The maximum value must evidently occur when the period of vibration of the circuit $C\ C'$ is the same as that of the fundamental vibration of the micrometer circuit, and it was shown that when the sparking distance between 2 and 4 had its maximum value, the sparks corresponded to a vibration with only one nodal point, for the sparks ceased when the previously existing nodes were touched by a conductor, and the only point where contact could take place without effect on the sparking was between *a* and *e*. These results show that it is possible to excite at will in the same conductor either the fundamental vibration or its first overtone, to use the language of acoustics.

Hertz appears to consider it very doubtful whether it will be possible to get higher overtones of electrical vibration, the difficulty of obtaining such lying not only in the method of observation, but also in the nature of the oscillations themselves. The intensity of these is found to vary considerably during a series of discharges from the coil even when all the circumstances are maintained as constant as possible, and the comparative feebleness of the resonant effects shows that there must be a considerable amount of damping. There are moreover many secondary phenomena which seem to indicate that irregular vibrations are superposed upon the regular ones, as would be expected in complex systems of conductors. If therefore we wish to compare electrical oscillations (from a mathematical point of view) with those of acoustics, we must seek our analogy in the high notes intermixed with irregular vibrations, obtained, say, by striking a wooden rod with a hammer, rather than in the comparatively slow harmonic vibration of tuning-forks or strings; and in the case of vibrations of the former class we have to be contented, even in the study of acoustics, with little more than indications of such phenomena as resonance and nodal points.

Referring to the conditions to be fulfilled in order to obtain the best results, should other physicists desire to repeat the experiments, Dr. Hertz notes a fact of very considerable interest and novelty, namely, that the spark from the discharger should always be visible from the micrometer, as when this was not the case, though the phenomena observed were of the same character, the sparking distance was invaribly diminished.

Theory of the experiments.—The theories of electrical oscillations which have been developed by Sir William Thomson, von Helmholtz, and Kirchhoff, have been shown* to hold good for the open-circuit oscillations of induction apparatus, as well as for the oscillatory Leyden-jar discharge; and although Dr. Hertz has not succeeded in obtaining definite quantitative results to compare with theory, it is of interest to inquire whether the observed results are of the same order as those indicated by theory.

Hertz considers, in the first place, the vibration period. Let T be the period of a single or half vibration proper to the conductor exciting the micrometer circuit; P its co-efficient of self-induction in absolute electro-magnetic measure expressed therefore in centimeters; C the capacity of one of its terminals in electro-static measure, and therefore also expressed in centimeters; and v the velocity of light in centimeter-seconds.

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* Lorentz, Wiedemann's *Annalen*, 1879, vol. VII, p. 161.

Then, if the resistance of the conductor is small,

$$T=\frac{\pi\sqrt{P\ C}}{v}$$

In the case of the resonance experiments, the capacity C was approximately the radius of the sphere forming the terminal, so that $C=15$ centimeters. The co-efficient of self-induction was that of a wire of length $l=150$ centimeters, and diameter $d=\frac{1}{2}$ centimeter.

According to Neumann's formula,

$$P=\int\int\frac{\cos\varepsilon}{r}d\,s\,d\,s',$$

which gives in the case considered

$$P=2\,l\left(\log\frac{4\,l}{d}-0.75\right)=1902^{\text{cm}}.$$

As however it is not quite certain that Neumann's formula is applicable to an open circuit, it is better to use von Helmholtz's more general formula, containing an undetermined constant k, according to which

$$P=2l\left(\log\frac{4\,l}{d}-0.75+\frac{1-k}{2}\right).$$

Putting $k=1$ this reduces to Neumann's formula; for $k=0$ it reduces to that of Maxwell; and for $k=-1$ to Weber's. The greatest difference in the values of P obtained by giving these different values to k would not exceed a sixth of its mean value, and therefore for the purposes of the present approximation it is enough to assume that k is not a large positive or negative number; for if the number 1902 does not give the correct value of the co-efficient for the wire 150 cm. in length, it will give the value corresponding to a conductor not differing greatly from it in length.

Taking $P=1902^{\text{cm}}$., we have $\pi\ \sqrt{CP}=531^{\text{cm}}$., which represents the distance traversed by light during the oscillation, or, according to Maxwell's theory, the length of an electro-magnetic æther wave. The value of T is then found to be $\left(\frac{1.77}{100,000,000}\right)$ 1.77 hundred millionths of a second, which is of the same order as the observed results.

The ratio of damping is then considered. In order that oscillations may be possible the resistance of the open circuit must be less than $2\,v\sqrt{\frac{P}{C}}$. For the exciting circuit used this gives 676 ohms as the upper limit of resistance. If the actual resistance r is sensibly below this limit, the ratio of damping will be $e^{\frac{rT}{2P}}$. The amplitude will therefore be reduced in the ratio 1:2.71 in

$$\frac{2\,P}{r\,T}=\frac{2\,v}{\pi\,r}\sqrt{\frac{P}{C}}=\frac{676}{\pi\,r}=\frac{215}{r}$$

oscillations. Unfortunately we have no means of determining the resistance of the air space traversed by the spark, but as the resistance of a strong electric arc is never less than a few ohms we shall be justified in assuming this as the minimum limit. From this it would follow that the number of oscillations due to a single impulse must be reckoned in tens, and not in hundreds or thousands, which is in accordance with the character of the experimental results, and agrees with the results observed in the case of the oscillatory Leyden-jar discharge. In the case of closed metallic circuits, on the other hand, theory indicates that the number of oscillations before equilibrium is attained must be reckoned by thousands.

Hertz compares lastly the order of the inductive actions of these oscillations, according to theory, with that of the effects actually observed. To do this it must be noted that the maximum *electro-motive force* induced by the oscillation in its own circuit is approximately equal to the maximum potential difference at its extremities; for if there were no damping, these quanities would be identical, since at any moment the potential difference at the extremities and the E. M. F. of induction would be in equilibrium. In the experiments under consideration the potential difference at the extremities was such as to give a spark 7 to 8^{mm}. in length, which must therefore represent the maximum inductive action excited in its own circuit by the oscillation. Again, at any instant the induced E. M. F. in the micrometer circuit must be to that in the exciting conductor in the same ratio as that of the co-efficient of mutual induction p of the two circuits to the co-efficient of self-induction P of the exciting circuit. The value of p for the case considered is easily calculated from the ordinary formulæ, and it is found to lie between one-ninth and one-twelfth of P. This would only give sparks of from $\frac{1}{2}$ to $\frac{2}{3}^{mm}$. in length, so that according to theory visible sparks ought in any case to be obtained; but, on the other hand, sparks several millimeters in length, as were obtained in the experiments previously described, can only be explained on the assumption that the successive inductive actions produce an accumulative effect; so that theory indicates the necessity of the existence of the resonant effects actually observed.

Dr. Hertz was at first inclined to suppose that as the micrometer circuit was only broken by the extremely short air space limited by the maximum sparking distance under the conditions of the experiment, it might therefore be treated as a closed circuit, and only the total induction considered. The ordinary methods of electro-dynamics give the means of completely determining the total inductive effect of a current element on a closed circuit, and would therefore in this case have sufficed for the investigation of the phenomena observed. He found however that the treatment of the micrometer circuit as a closed circuit led to incorrect results, so that it, as well as the primary, had to be treated as an open circuit, and therefore a knowledge of the total induction was insufficient, and it became necessary to consider the value both of the E. M. F. induction and of the electro-static E. M. F. due to the charged extremeties of the exciting circuit at each point of the micrometer circuit.

The investigations to which these considerations led are described by Dr. Hertz in a paper "On the Action of a Rectilinear Electrical Oscillation upon a Circuit in its Vicinity," published in Wiedemann's *Annalen*, 1888, vol. XXXIV, page 155.

In what follows, the exciting circuit will be spoken of as the primary, and the micrometer circuit as the secondary. Hertz points out that the reason that the electro-static effect can not be neglected is to be found in the extreme rapidity with which the electro-static forces change their sign. If the electro-static alternations in the primary were comparatively slow they might attain a very high intensity without giving rise to a spark in the secondary, since the electro-static distribution on the secondary would vary so as to remain in equilibrium with the external E. M. F. This however is impossible, because the variations in direction follow each other too rapidly for the distribution to follow them.

In the present investigations the primary circuit consisted of a straight copper wire 5 millimeters in diameter, carrying at its extremeties hollow zinc spheres 30 centimeters in diameter. The centers of the spheres were 1 meter apart, and at the middle of the wire was an air space three-fourths centimeter in length. The wire was placed in a horizontal position, and the observations were all made at points near to the horizontal plane through it, which however did not of course affect their generality, as the same effects would necessarily be produced in any plane through the horizontal wire. The secondary circuit consisted of a circle of 35 centimeters radius, of copper wire 2 millimeters in diameter, the circle being broken by an air space capable of variation by means of a micrometer screw.

The circular form was selected for the secondary circuit because the former investigations had shown that the sparking distance was not the same at all points of the secondary, even when the conductor as a whole remained unchanged in position, and with a circular circuit it was easier to bring the air space to any part than if any other form had been used. To attain this object the circle was made movable about an axis passing through its center perpendicular to its plane.

The circuits of the dimensions stated were very nearly in unison, and they were further adjusted by means of little strips of metal soldered to the extremities, and varied in length until the maximum sparking distance was obtained.

We shall follow Dr. Hertz in first considering the subject theoretically, and then examining how far the experimental results are in accordance with the theoretical conclusions. It will be assumed that the E. M. F. at every point is a simple harmonic function of the time, but that it does not undergo reversal in direction, and it will further be assumed that the oscillations are at any given moment everywhere in the same phase. This will certainly be the case in the immediate neighborhood of the primary, and for the present we shall confine our attention to such points. Let s be the distance of a point, measured along the circuit from the air space of the secondary, and F the component E. M. F. at that point along the circular arc $d\,s$. Then F is a function of s, which assumes its original value after passing once round the circle of circumference S. It may therefore be expanded in the form

$$F = A + B \cos\frac{2\pi s}{S} + \ldots\ldots + B' \sin\frac{2\pi s}{S} + \ldots\ldots$$

The higher terms of the series may be neglected, as the only result of so doing will be that the approximate theory will give an absolute disappearance of sparks where really the disappearance is not quite complete, and indeed the experiments are not delicate enough to enable us to compare their results with theory beyond a first approximation.

The force A acts in the same direction, and is of constant amount at all points of the circle, and therefore it must be independent of the electro-static E. M. F., as the integral of the latter round the circle is zero. A, then, represents the total E. M. F. of induction, which is measured by the rate of variation of the number of magnetic lines of force which pass through the circle. If the electro-magnetic field containing the circle is assumed to be uniform, A will therefore be proportional to the component of the magnetic induction perpendicular to the plane of the secondary. It will therefore vanish when the direction of the magnetic induction lies in the plane of the secondary. A will consist of an oscillation, the intensity of which is independent of the position of the air space in the circle, and the corresponding sparking distance will be called a.

The term $B' \sin\frac{2\pi s}{S}$ can have no effect in exciting the fundamental vibration of the secondary, since it is symmetrical on opposite sides of the air space.

The term $B \cos\frac{2\pi s}{S}$ will give force acting in the same direction in the two quadrants opposed to the air space, and will excite the fundamental vibration. In the two quadrants adjacent to the air space it will give a force in the opposite direction, but its effect will be less than that of the former one. For the current is zero at the extremities of the circuit, and therefore the electricity can not move so freely as near the center. This corresponds to the fact, that if a string fastened at each end has its central portion and ends acted on respectively by oppositely directed forces, its motion will be that due to the force at the central portion, which will excite the fundamental vibration if its oscillations are in unison with the latter. The intensity of the vibration will be proportional to B. Let E be the total E. M. F. in the uniform field of the secondary, φ the angle between its direction and the plane of the latter, and θ the angle which its projection on this plane makes with the radius drawn to the air space. Then we shall have, approximately,

$$F = E \cos\varphi \sin\left(\frac{2\pi s}{S} - \theta\right)$$

and therefore $$B = -E \cos\varphi \sin\theta$$

B, therefore, is a function simply of the total E. M. F. due both to the electro-static and electro-dynamic actions. It will vanish when $\varphi = 90^\circ$—that is to say, when the total E. M. F. is perpendicular to the plane of the circle, whatever be the position of the air space on the circle. B will also vanish when $\theta = 0$, — that is to say, when the projection of the E. M. F. on the plane of the circle coincides with the radius through the air space. If the position of the air space on the circle is varied, the angle θ will vary, and therefore also the intensity of the vibration and the sparking distance. The sparking distance corresponding to the second term of the expansion for F can therefore be represented approximately by a formula of the form $\beta \sin\theta$.

Now the oscillations giving rise to sparks of lengths a and $\beta \sin\theta$ respectively are in the same phase. The resulting oscillations will therefore be in the same phase, and their amplitudes must be added together. The sparking distance being approximately proportional to the maximum total amplitude, may therefore also be obtained by adding the sparking distances due to the two oscillations respectively. The sparking distance will therefore be given as a function of the position of the air space on the secondary circuit by the expression $a+\beta \sin\theta$. Since the direction of the oscillation in the air space does not come into consideration we are concerned only with the absolute value of this expression, and not with it sign. The determination of the absolute values of the quantities a and β would involve elaborate theoretical investigations, and is moreover unnecessary for the explanation of the experimental results.

Experiments with the secondary circuit in a vertical plane.—When the circle forming the secondary circuit was placed with its plane vertical, anywhere in the neighborhood of the primary, the following results were obtained:

The sparks disappeared for two positions of the air space, separated by 180°, namely, those in which it lay in the horizontal plane through the primary; but in every other position sparks of greater or less length were observed.

From this it followed that the value of a must have been constantly zero, and that θ was zero when the air space was in the horizontal plane through the primary.

The electro-magnetic lines of force must therefore have been perpendicular to this horizontal plane, and therefore consisted of circles with their centers on the primary, while the electro-static lines of force must have been entirely in the horizontal plane, and therefore this system of lines of force consisted of curves lying in planes passing through the primary. Both of these results are in agreement with theory.

When the air space was at its greatest distance from the plane the sparking distance attained a maximum value of from 2 to 3 millimeters. The sparks were shown to be due to the fundamental vibration, by slightly varying the secondary, so as to throw it out of unison with the primary, when the sparking distance was diminished, which would not have been the case if the sparks had been due to overtones. Moreover, the sparks disappeared when the secondary was cut at its points of intersection with the horizontal plane through the primary, though these would be nodal points for the first overtone.

When the air space was kept at its greatest possible distance from the horizontal plane through the primary, and turned about a vertical axis, the sparking distance attained two maxima at the points for which $\varphi=0$, and almost disappeared at the points for which $\varphi=90°$.

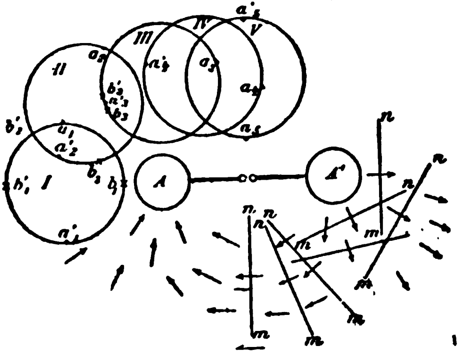

FIG. 8.

The lower half of Fig. 8 shows the different positions of minimum sparking. *A A'* is the primary conductor, and the lines *m n* represent the projections of the secondary circuit on the horizontal plane. The arrows perpendicular to these give the direction of the resultant lines of force. As this did not anywhere vanish in passing from the sphere *A* to the sphere *A'*, it could not change its sign.

The diagram brings out the two following points:

(1) The distribution of the resultant E. M. F. in the vicinity of the rectilinear vibration is very similar to that of the electro-static E. M. F. due to the action of its two extremities. It should be specially noted that near the center of the primary the direction is that of the electro-static E. M. F., showing that it is more powerful than the electro-dynamic, as required by theory.

(2) The lines of force deviate more rapidly from the line *A A'* than the electro-static lines, though this is not so evident on the reduced scale of the diagram as in the author's original drawings on a much larger scale.

It is due to the components of the electro-static E. M. F. parallel to *A A'* being weakened by the E. M. F. of induction, while the perpendicular components remained unaffected.

Experiments with the secondary circuit in a horizontal plane.—The results obtained when the plane of the secondary was horizontal can best be explained by reference to the upper half of the diagram in Fig. 8.

In the position *I*, with the center of the circle in the line *A A′* produced, the sparks disappeared when the air space occupied either of the b_1 or b'_1, while two equal maxima of the sparking distance were obtained at a_1 and a'_1, the length of the spark in these positions being 2.5 millimeters. Both these results are in accordance with theory.

In position *II* the circle is cut by the electro-magnetic lines of force, and therefore a does not vanish. It will however be small, and we should expect that the expression $a+\beta \sin \theta$ would have two unequal maxima $\beta+a$ and $\beta-a$, both for $\theta=90°$, and having the line joining them perpendicular to the resultant E. M. F., and between these two maxima we should expect two points of no sparking near to the smaller maximum. This was confirmed by the observations.

The maximum sparking distances were 3.5 millimeters at a_2 and 2 millimeters at a'_2. Now with the air space at a_2,—the sphere *A* being positive,—the resultant E. M. F. in the opposite portion of the circle will repel positive electricity from *A*, and therefore tend to make it flow round the circle clockwise. Between the two spheres the electro-static E. M. F. acts from *A* towards *A′*, and the opposite E. M. F. of induction in the neighborhood of the primary acts from *A′* to *A*, parallel to to the former, and acting more strongly on the nearer than on the further portion of the secondary, tends to cause a current in the same direction as that due to the former, namely, in a clockwise direction. Thus the resultant E. M. F. is the sum of the two as required by theory, and in the same way it is easily seen that when the air space is at a'_2, the resultant E. M. F. is equal to their difference.

As the position *III* is gradually approached, the maximum disappears, and the single maximum sparking distance a_3 was found to be 4 millimeters in length, having opposite to it a point of disappearance a'_3. In this case clearly $a=\beta$, and the sparking distance is given by the expression $a\ (1+\sin \theta)$. The line $a_3 a'_3$ is again perpendicular to the resultant E. M. F.

As the circle approaches further towards the center of *A A′*, a will become greater than β, and the expression $a+\beta$ sin θ will not vanish for any value of θ, but will have a maximum $a+\beta$ and a minimum $a-\beta$, and in the experiments it was found that the sparks never entirely disappeared, but varied between a maximum and a minimum, as indicated by theory.

In the position *IV* a maximum sparking distance of 5.5 millimeters was observed at a_4 and a minimum of 1.5 millimeter at a'_4.

In the position *V* there was a maximum sparking distance of 6 millimeters at a_5 and a minimum of 2.5 millimeters at a'_5. In these experiments the air space should be screened off from the primary in the latter positions as well as in the earlier ones, in which it is unavoidable, as otherwise the results would not be comparable.

In passing from the position *III* to the position *V* the line $a\,a'$ rapidly turned from its position of parallelism to the primary circuit into a position perpendicular to it. In the latter positions the sparking was essentially due to the inductive action, and therefore the author was justified, in his former experiments, in assuming the effect in these positions to be due to induction.

Even in these positions however, the sparking is not totally independent of electro-static action, except when the air space is half way between the maximum and minimum positions, and therefore $\beta \sin \theta = 0$.

Other positions of the secondary circuit.—Dr. Hertz made numerous observations with the secondary circuit in other positions, but in no case were any phenomena observed which were not completely in accordance with theory. As an example of these consider the following experiment:

The secondary was first placed in the horizontal plane in the position *V* (Fig. 8), and the air space was in the position a_5 relatively to the primary. The circle was then turned about a horizontal axis through its center and parallel to the primary, so as to raise the air space above the horizontal plane. During this rotation θ remained equal to 90°, and the value of β remained nearly constant, but a varied approximately in the same ratio as $\cos \Psi$, Ψ being the angle between the plane of the circle and the horizontal, for a is proportional to the number of magnetic lines of force passing through the circle. Let a_0 be the value of a in the initial position, then in the other positions its value would be $a_0 \cos \Psi$, and therefore the sparking distance should be given by the expression $a_0 \cos \Psi + \beta$, in which a_0 was known to be greater than β. This was confirmed by observation, for it was found that as the air space increased its height above the horizontal plane the sparking distance diminished from 6 millimeters down to 2 millimeters, its value when the air space was at its greatest distance above the horizontal plane. During the rotation through the next quadrant the sparking distance diminished almost to zero, and then increased to the smaller maximum of 2.5 millimeters, which it attained when the circle had turned though 180°, and was therefore again horizontal. Similar results were obtained in the opposite order, as the circle was rotated from 180° to 360°. When the circle was kept with the air space at its maximum height above the horizontal plane, and then raised or lowered bodily without rotation, the sparking distance was found to diminish in the former case and to increase in the latter, results completely in accordance with theory.

Forces at greater distances.—Experiments with the secondary at greater distances from the primary are of great importance, as the distribution of E. M. F. in the field of an open circuit is very different according to different theories of electro-dynamic action, and the results may therefore serve to eliminate some of them as untenable. In making these experiments however, an unexpected difficulty was encountered, as it was found that at distances of from 1 to 1.5 meters from the primary the maximum and minimum, except in certain positions, became indistinctly defined; but when the distance was increased to upwards of 2 meters, though the sparks were then very small, the maximum and minimum were found to be very sharply marked when the sparks were observed in the dark. The positions of maximum and minimum were found to occur with the circle in planes at right angles to each other. At considerable distances the sparking diminished very slowly as the distance was increased. Dr. Hertz was not able to determine an upper limit to the distance at which sensible effects took place, but in a room 14 meters by 12, sparks were distinctly observed when the primary was placed in one corner of the room, wherever the secondary was placed. When however the primary was slightly displaced, no effects could be observed, even when the secondary was brought considerably nearer. The interposition of solid screens between the two circuits greatly diminished the effect.

Dr. Hertz mapped out the distribution of force throughout the room by means of chalk lines on the floor, putting stars at the points where the direction of the E. M. F. became indeterminate. A portion of the diagram obtained in this manner is shown on a reduced scale in Fig. 9,

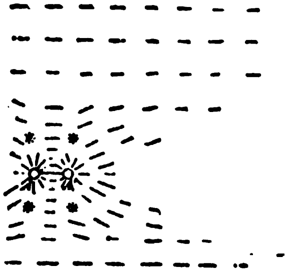

FIG. 9.

with respect to which the following points are note-worthy:

1. At distances beyond 3 meters the E. M. F. is everywhere parallel to the primary oscillation. Within this region, therefore, the electrostatic E. M. F. is negligible in comparison with the E. M. F. of induction. Now all the theories of the mutual action of current elements agree in giving an E. M. F. of induction inversely proportional to the distance, while the electro-static E. M. F., being due to the differential action of the two extremities of the primary, is approximately inversely proportional to the cube of the distance. Some of these theories however are not in accordance with the experimental result that the effect diminishes much more rapidly in the direction of the primary oscillation than in a direction at right angles to it, induced sparks being observed at a distance exceeding 12 meters in the latter direction, while they disappeared at a distance of about 4 meters in the former direction.

2. For distances less than 1 meter (as already proved), the distribution of E. M. F. is practically that of the electro-static E. M. F.

3. There are two straight lines, at all points of which the direction of the E. M. F. is determinate, namely, the line in which the primary oscillation takes place, and the perpendicular to the primary through its middle point. Along the latter the E. M. F. does not vanish at any point, the sparking diminishes gradually as the distance is increased. This again is inconsistent with some of the theories of mutual action of current elements, according to which it should vanish at a certain definite distance. A very important result of the investigation is the demonstration of the existence of regions within which the direction of the E. M. F. becomes indeterminate. These regions form two rings encircling the primary circuit. Since the E. M. F. within them acts very nearly equally in every direction, it must assume different directions in succession, for of course it can not act in different directions simultaneously.

The observations therefore lead to the conclusion that within these regions the magnitude of the E. M. F. remains very nearly constant, while its direction varies through all the points of the compass at each oscillation. Dr. Hertz states that he has been unable to explain this result, as also the existence of overtones, by means of the simplified theory in which the higher terms of the expansion of F are neglected, and he considers that no theory of simple action at a distance is capable of explaining it. If however the electro-static E. M. F. and the E. M. F. of induction are propagated through space with unequal velocities it admits of very simple explanation; for within these annular regions the two E. M. F.'s are at right angles and of the same order of magnitude; they will therefore in consequence of the distance traversed, differ in phase, and the direction of the resultant will turn through all the points of the compass at each oscillation.

This phenomenon appears to him to be the first indication which has been observed of a finite rate of propagation through space of electrical actions, for if there is a difference in the rate of propagation of the electro-static and electro-dynamic E. M. F. one at least of them must be finite.

At the end of the paper in which the preceding experiments are described Dr. Hertz describes some observations which he has made on the conditions at the primary sparking point which affect the production of sparks in the secondary circuit. He finds that illuminating the primary spark diminishes its power of exciting rapid oscillations, the sparks in the secondary being observed to cease when a piece of magnesium wire was burnt, or an arc lamp lighted, near the primary sparking point. The observed effect on the primary sparks is that they are no longer accompanied by a sharp crackling sound as before. The effect of a second discharge is especially noteworthy, and it was found that the secondary sparks could be made to disappear by bringing an insulated conductor close to the opposed surfaces of the spheres forming the terminals at the primary air space, even when no visible sparking took place between the latter and the insulated conductor. The secondary sparking could also be stopped by placing a fine point close to the primary air space, or by touching one of the opposed surfaces of the terminals with a piece of sealing-wax, glass, or mica. Dr. Hertz states that further experiments have led him to conclude that even in these cases the effect is due to light too feeble to be perceived by the eye, arising from a side discharge. He points out that these effects afford another example of the effects of light on electric discharges, which have been observed by E. Wiedemann, H. Ebert, and W. Hallwachs.

Dr. Hertz's next paper in order of publication in Wiedemann's *Annalen*, "On Some Induction Phenomena Arising from Electrical Actions in Dielectrics" (Wiedemann's *Annalen*, 1888, vol. XXXIV., p. 273), contains an account of some researches undertaken with a view of obtaining direct experimental confirmation of the assumption involved in the most suggestive theory of electrical actions, viz, that of Faraday and Maxwell, that the well-known electro-static phenomena observed in dielectrics are accompanied by corresponding electro-dynamic actions. The method of observation consisted in placing a secondary conductor adjusted to unison, as regards electrical oscillations, with the primary, as near as possible to the former, and in such a relative position that the sparks in the primary produced no sparking in the secondary. As the equilibrium could be disturbed and sparking induced in the secondary by the approach of conductors, it formed a kind of induction balance; but the point of special interest in connection with it was that a similar effect was produced when the conductors were replaced by insulators, provided the latter were of comparatively large size. The observed rapidity of the oscillations induced in the di-electrics showed that the quantities of electricity in motion under the influence of di-electric polarization were of the same order of magnitude as in the case of metallic conductors.

The apparatus employed is shown diagrammatically in Fig. 10, and was supported on a light wooden framework, not shown in the illustration. The primary conductor consisted of two brass plates, *AA'*, with sides 40 centimeters in length, joined by a copper wire 70 centimeters long and half a centimeter in diameter, containing an air space of three-quarters of a centimeter, with terminals formed of polished brass spheres. When placed in connection with a powerful induction coil, oscillations are set up, the period of which, determined by the dimensions of the primary, can be determined to a hundred millionth of a second. The secondary conductor consisted of a circle, 35 centimeters in radius, of copper wire 2 millimeters in diameter, containing an air space, the length of which could be varied by means of a screw from a few hundredths of a millimeter up to several millimeters. The dimensions stated were such as to bring the two conductors into unison, and secondary sparks up to six or seven millimeters in length could be obtained.

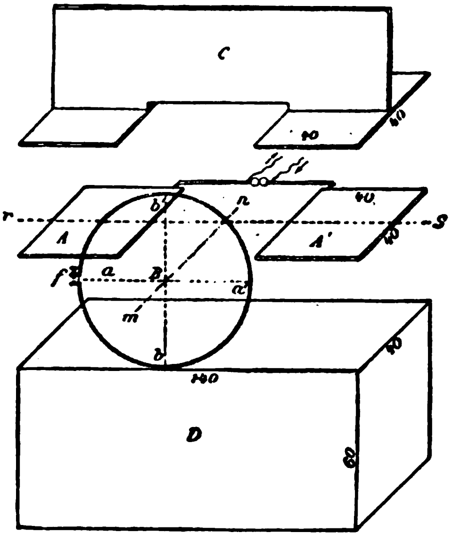

FIG. 10.

The circle was movable about an axis through its center perpendicular to its plane, to enable the position of the air space to be varied. The axis was fixed in the position *m n* in the plane of *A* and *A'*, and half way between them. The center of the circle was at a distance of 12 centimeters from the nearest points of *A* and *A'*.

When *f* was in either of the positions *a* or *a'* lying in the plane of *A A'* no sparking occurred in the secondary, while maximum sparking took place at *b* and *b'* 90° from the former positions. The E. M. F. giving rise to the secondary sparks is, as in previous experiments, partly electrostatic and partly electro-magnetic, and the former being the greater will determine the sign of the resultant E. M. F. The oscillations must, for the reason previously explained, be considered as produced in the part of the secondary most remote from the air space. Assuming the E. M. F. and the amplitude of the resulting oscillation to be positive when *f* is in the position *b'*, they will both be negative when *f* is at *b*.

When the circle was slightly lowered in its own plane the sparking distance was increased at *b'* and diminished at *b*, and the null points lay at a certain distance below *a* and *a'*. The electro-static E. M. F. is scarcely affected by such a displacement, but the integral of the E. M. F. of induction taken round the circle is no longer zero, and therefore gives rise to an oscillation which will be of positive sign whatever be the position of *f*; for the direction of the resultant E. M. F. of induction is opposite to that of the electrostatic E. M. F. in the upper half of the circle, and coincides with it in the lower half where the electrostatic E. M. F. has been assumed to be positive. Since the new oscillation so produced is in the phase as the previously existing one, their amplitudes must be added to give the resultant amplitude, which explains the phenomena.

Effects of the Approach of Conductors.—In making these observations it was found necessary to remove all conductors to a considerable distance from the apparatus, in order to obtain a complete disappearance of sparking at the points *a* and *a'*. Even the neighborhood of the observer was sufficient to set up sparking when the air space *f* was in either of these positions, and the sparks had therefore to be observed from a distance. The conductor used for the experiments was of the form shown at *C* (Fig. 10), and consisted of thin metal foil. The objects kept in view in selecting the material and dimensions were to obtain a conductor which would give a moderately large effect, and having an oscillation period less than that of the primary.

When the conductor *C* was brought near to *A A'*, it was found that the sparking distance decreased at *b* and increased at *b'*, and the null points were displaced upwards,—that is, in the direction of *C*.

From the results of experiments already described it is evident that the effect of displacing *A A'* upwards would be the same, qualitatively, as that of a current in the same direction as that in *A A'* directly above it. The effect produced by the approach of *C* was the reverse of this, and could be explained by an inductive action, supposing there were a current in *C* in the opposite direction to that in *A A'*, which is exactly what must occur; for the electro-static E. M. F. would give rise to such a current, and since the oscillations in *C* are more rapid than those of this E. M. F. the current must be in the same phase as the inducing E. M. F. The truth of this explanation was confirmed by the following experiments. The horizontal plates of the conductor *C* being left in the same position as before, the vertical plate was removed, and successively replaced by wires of increasing length and fineness, in order to lengthen the oscillation period of *C*. The effect of this was to displace the null points more and more in an upward direction, while at the same time they became less sharply defined, a minimum sparking taking the place of the previous absolute disappearance. The sparking distance at the highest point had previously been much less than at the lowest point, but after the disappearance of the null points it began to increase. At a certain stage the sparking distance at the two positions became equal, and then no definite minimum points could be found, but sparking took place freely at all positions of *f*. Beyond this stage the sparking distance at the lowest point diminished and very soon two minimum points made their appearance close to it, not clearly defined at first, but gradually becoming more distinct, and at the same time approaching the points *a a'*, with which they ultimately coincided, when the minimum points again became absolute null points. These results are in agreement with the conclusion drawn from the former observations, for as the oscillation period of *C* approaches that of *A A'*, the intensity of the current in the former increases, but a difference of phase arises between it and the exciting E. M. F. When the two are in unison the current in *C* attains its maximum, and, as in other cases of resonance, the difference of phase gives rise to a slightly damped oscillation, having a period of about a quarter that of the original one, which makes any interference between the oscillations excited in the circle *B* by *A A'* and *C* respectively impossible. These conditions clearly correspond to the stage at which the sparking distances at *b* and *b'* were equal. When the oscillation period of *C* becomes decidedly greater than that of *A A'*, the amplitude of the oscillation in the former will again diminish, so that the difference in phase between it and the exciting E.M.F. will approach half of the original period. The current in *C* will therefore always be in the same direction as that in *A A'*, so that interference between the two oscillations excited in *B* will again become possible, and the effect of *C* will then be opposite to its original effect. When the conductor *C* was made to approach *A A'* the sparks in *B* became much smaller, which is explained by the fact that its effect will be to increase the oscillation period of *A A'*, and therefore to throw it out of unison with *B*.

Effects of the approach of dielectrics.—A very rough estimate shows that when a di-electric of large mass is brought near to the apparatus, the quantities of electricity set in motion by di-electric polarization are at least as large as in metallic wires or thin rods. If therefore the action of the apparatus were unaffected by the approach of such masses it would show that in contradiction to the theories of Faraday and Maxwell, no electro-dynamic actions are called into play by means of di-electric polarization, or as Maxwell calls it, electric displacement. The experiments however showed an effect similar to that which would be produced if the di-electric were replaced by a conductor with a very small oscillation period. In the first experiment made, the mass of di-electric consisted of a pile of books, 1.5 meter long, 0.5 meter broad, and 1 meter high, placed under the plates *A A'*. Its effect was to displace the null points through about 10° towards the pile. A block of asphalt (*D*, in Fig. 10), weighing 800 kilograms, and measuring 1.4 meter in length, 0.4 meter in breadth, and 0.6 meter in height, was then used in place of the books, the plates being allowed to rest upon it.

The following results were then obtained:

(1) The spark at the highest point of the circle was now decidedly stronger than that at the lowest point, which was nearer to the asphalt.

(2) The null points were displaced through about 23° downwards, that is, in the direction of the block, and at the same time were transformed into mere points of minimum sparking, a complete disappearance being no longer obtainable.

(3) When the plates *A A′* rested on the asphalt block the oscillation period of the primary was increased, as shown by the fact that the period of *B* had to be slightly increased in order to obtain the maximum sparking distance.

(4) When the apparatus was moved gradually away from the block its action steadily diminished without changing its character.

(5) The action of the block could be compensated by bringing the conductor *C* over the plates *A A′*, while they rested on the block, the null points being brought back to *a* and *a′* when *C* was at a height of 11 centimeters above the plates. When the upper surface of the asphalt was 5 centimeters below the plates, compensation was obtained when *C* was placed at a height of 17 centimeters above them, showing that the action of the di-electric was of the order of magnitude which had been anticipated.

The asphalt contained about 5 per cent. of aluminium and iron compounds, 40 per cent. of calcium compounds, and 17 per cent. of quartz sand. In order to make sure that the observed effects were not due to the conductivity of some of these substances, a number of further experiments were made.

In the first place the asphalt was replaced by a mass of the same dimensions of the so-called artificial pitch prepared from coal, and effects of a similar kind were observed, but slightly weaker, the greatest displacement of the null points amounting to 19°. Unfortunately this pitch contains free carbon, the amount of which it is difficult to determine, and this would have some conductivity.

The experiments were then repeated with a conductor, *C*, of half the linear dimensions of the former one, and smaller blocks of various substances, on account of the great cost of obtaining large blocks of pure materials. The substances used were asphalt, coal-pitch, paper, wood, sandstone, sulphur, paraffine, and also a fluid di-electric, namely petroleum. With the smaller apparatus it was not possible to obtain quantitative results of the same accuracy as before, but the effects were of an exactly similar character, and left little room for doubt of the reality of the action of the di-electric.

The results might possibly be supposed to be due to a change in the distribution of the electro-static E. M. F. in the neighborhood of the di-electric, but in the first place Dr. Hertz states that he has been unable to explain the details of the observations on this hypothesis, and in the second place it is disproved by the following experiment:

The smaller apparatus was placed with the line *r s* on the upper near corner of one of the large blocks, in which position the di-electric was bounded by the plane of the plates *A A′* and the perpendicular plane through *r s*, both of which are equipotential surfaces, so that if the action were electro-static no effect should be produced by the di-electric. It was found however to produce the same effect as in other positions. It might also be supposed that the effects were due to a slight conductivity, but this could hardly be the case with such good insulators as sulphur and paraffine. Suppose moreover that the conductivity of the di-electric is sufficient to discharge the plate *A* in the ten-thousandth of a second, but not much more rapidly. Then, during one oscillation, the plates would lose only the ten-thousandth part of their charge, and the conduction current in the substance experimented on would not exceed the ten-thousandth part of the primary current in *A A'*, so that the effect would be quite insensible.

It was shown in the experiments described in the last section, that when variable electrical forces act in the interior of di-electrics of specific inductive capacity not equal to unity, the corresponding electric displacements produce electro-dynamic effects. In a paper "On the Velocity of Propagation of Electro-Dynamic Actions," in Wiedemann's *Annalen*, 1888, vol. XXXIV, p. 551, Dr. Hertz shows that similar actions take place in the air, which proves, as was previously pointed out, that electro-dynamic action must be propagated with a finite velocity.

The method of investigation was to excite electrical oscillations in a rectilinear conductor in the same manner as in former experiments, and then to produce effects in a secondary conductor by exciting electrical oscillations in it by means of those in the rectilinear conductor, and at the same time by the primary conductor acting through the intervening space. This distance was gradually increased, when it was found that the phase of the vibrations at a distance from the primary lagged behind those in its immediate neighborhood, showing that the action is propagated with a finite velocity, which was found to be greater than the velocity of propagation of electrical waves in wires in the ratio of about 45 to 28, so that the former is of the same order as the velocity of light. Dr. Hertz was unable to obtain any evidence with respect to the velocity of propagation of electro-static actions.

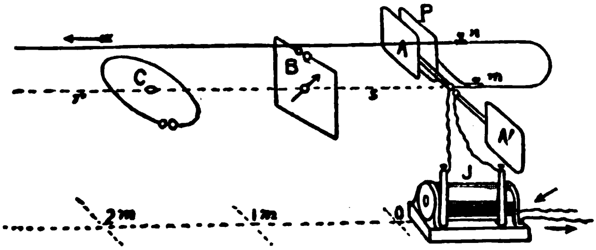

FIG. 11.

The primary conductor *A A'* (Fig. 11) consisted of a pair of square brass plates with sides 40 centimeters in length, connected by a copper wire 60 centimeters in length, at the middle point of which was an air space, across which sparks were made to pass by means of powerful discharges from the induction coil *J*. The conductor was fixed at a height of 1.5 meter above the base-plate of the coil, with its plates vertical, and the connecting wire horizontal. A straight line *r s*, drawn horizontally through the air space of the primary and perpendicular to the direction of the primary oscillation, will be called the base-line, and a point in this situated at a distance of 45 centimeters from the air space will be referred to as the null point.

The experiments were made in a large lecture room, with nothing near the base-line for a distance of 12 meters from the primary conductor. The room was darkened during the experiments.

The secondary conductor consisted either of a circular wire *C*, of 35 centimeters radius, or of a square of wire *B*, with sides 60 centimeters long. The primary and secondary air spaces were both capable of adjustment by means of micrometer screws. Both the secondary conductors were in unison with the primary, the (half) vibration period of each being $\frac{1.4}{100,000,000}$ (1.4/hundred-millionths) of a second, as calculated from the capacity and coefficient of self-induction. It is doubtful whether the ordinary theory of electrical oscillations would lead to accurate results under the conditions of these experiments; but as it gives correct numerical results in the case of Leyden-jar discharges, it may be expected to be correct as far as the order of the results is concerned. When the center of the secondary lies in the base-line, and its plane coincides with the vertical plane through the base-line, no sparks are observed in the secondary, the E. M. F. being everywhere perpendicular to the direction of the secondary. This will be referred to as "the first principal position" of the secondary. When the plane of the secondary is vertical and perpendicular to the base-line, the center still lying in the base-line, the secondary will be said to be in its "second principal position." Sparking then occurs in the secondary when its air space is either above or below the horizontal plane through the base-line, but not when it is in this plane. As the distance from the primary was increased the sparking distance was observed to decrease, rapidly at first, but ultimately very slowly. Sparks were observed throughout the whole distance of 12 meters available for the experiments. The sparking in this position is due essentially to the E. M. F. produced in the portion of the secondary remote from the air space. The total E. M. F. is partly electro-static and partly electro-dynamic, and the experiments show beyond the possibility of doubt that the former is greater, and therefore determines the direction of the total E. M. F. close to the primary, while at greater distances it is the electro-dynamic E. M. F. which is the greater.

The plane of the secondary was then turned into the horizontal, its center still lying in the base-line. This may be called "the third principal position." When the center of the circular secondary conductor was kept fixed at the null point, and the air space was made to travel round the circle, vigorous sparking was observed in all positions. The sparking distance attained its maximum length of about six millimeters when its air space was nearest to that of the primary, and its minimum length of about three millimeters when the distance between the two air spaces was greatest. If the secondary had been influenced by the electrostatic force, sparking would only be expected when the air space was close to the base-line, and a cessation of sparks in the intermediate positions. The direction of the oscillation would, moreover, be determined by the direction of the E. M. F. in the portion of the secondary farthest from the air space. There is however superposed upon the electro-statically-excited oscillation a second oscillation due to the E. M. F. of induction, which produces a considerable effect since its integral round the circle (considered as a closed circuit) does not vanish; and the direction of this integral E. M. F. is independent of the position of the air space, opposing the electro-static E. M. F. in the portion of the secondary next to *A A'*, and assisting it in the portion furthest from *A A'*, as explained in a previous paper.

The electro-static and electro-dynamic E. M. F.s therefore act in the same direction when the air space is turned towards the primary conductors, and in opposite direction when the air space is turned away from the primary. In the latter position it is the E. M. F. of induction which is the more powerful, as is shown by the fact that there is no disappearance of sparking in any position of the air space, for when this is 90 degrees to the right or left of the base line it coincides with a node with respect to the electrostatic E. M. F. In these positions the inductive action in the neighborhood of the primary can be observed, independently of the electrostatic action.

Waves in Rectilinear Wires.—In order to produce in a wire by means of the primary oscillations a series of advancing waves of the character required for these experiments, the following arrangements were made: Behind the plate *A* was placed a plate *P* of equal size. A copper wire one millimeter in diameter connected *P* to the point *m* of the base-line. From *m* the wire was continued in a curve about a meter in length to the point *n*, situated about 30 centimeters above the air space, and was then further continued in a straight line parallel to the base-line for such a distance as to obviate all danger of disturbance from reflected waves. In the present series of experiments the wire passed through a window, and after being carried to a distance of about 60 meters was put to earth, and a special series of experiments showed that this length was sufficient. When a wire, bent so as to form a nearly closed circuit with a small air space, was brought near to this straight wire, a series of fine sparks was seen to accompany the discharges of the induction coil. Their intensity could be varied by varying the distance between the plates *P* and *A*. The waves in the rectilinear wire were of the same period as that of the primary oscillations, as was proved by their being shown to be in unison with each of the two secondary conductors previously described. The existence of stationary waves showed that the waves in the rectilinear wire were of a steady character in space as well as in time. The nodal points were determined in the following manner: The further end of the wire was left free, and the secondary conductor was brought near to it, in such a position that the wire lay in its plane, and had the air space turned towards it. As the secondary was moved along the wire, points of no sparking were observed to recur periodically. The distance from the point *n* to the first of these was measured, and the length of the wire made equal to a multiple of this distance. The experiments were then repeated and it was found that the nodal points occurred at approximately equal intervals along the wire.

The nodes could also be distinguished from the loops in other ways. The secondary conductor was brought near to the wire, with its plane perpendicular to it, and with its air space neither directed completely towards the wire nor completely away from it, but in an intermediate position, so as to produce E. M. F.'s perpendicular to the wire. Sparks were then observed at the nodes, while they disappeared at the loops. When sparks were taken from the rectilinear wire by means of an insulated conductor, they were found to be stronger at the nodes than at the loops; the difference however was small, and was indeed scarcely distinguishable unless the position of the nodes and loops was previously known. The reason that this and other similar methods do not give a well-defined result lies in the fact that irregular oscillations are superposed upon the waves considered; the regular waves however can be picked out by means of the secondary, just as definite notes are picked out by means of a Helmholtz resonator. If the wire is severed at a node, no effect is produced upon the waves in the portion of wire next to the origin; but if the severed portion of wire is left in its place, the waves continue to be propagated through it, though with somewhat diminished strength.

The possibility of measuring the wave-lengths leads to various applications. If the copper wire hitherto used is replaced by one of different diameter, or by a wire of some other metal, the nodal points retain their position unchanged. It follows from this that the velocity of propagation in a wire has a definite value independent of its dimensions and material. Even iron wires offer no exception to this, showing that the magnetic susceptibility of iron does not play any part in the case of such rapid motions. It would be interesting to investigate the behavior of electrolytes in this respect. In their case we should expect a smaller velocity of propagation, because the electrical motions are accompanied by motions of the molecules carrying the electric charges. It was found that no propagation of the waves took place through a tube 10 millimeters in diameter, filled with a solution of sulphate of copper; but this may have been due to the resistance being too high. By the measurement of wave-lengths the relative vibration periods of different primary conductors can be determined, and it therefore becomes possible to compare in this manner the vibration periods of plates, spheres, ellipsoids, &c.

In the experiments made by Dr. Hertz, nodes were very distinctly produced when the wire was severed at a distance of either 8 meters or 5.5 meters from the null point of the base line. In the first case the nodes occurred at distances from the null point of -0.2 meter, 2.3 meters, 5.1 meters, and 8 meters, and in the latter case at distances of -0.1 meter, 2.8 meters, and 5.5 meters. It appears therefore that the (half) wave-length in a free wire cannot differ much from 2.8 meters The fact that the wave-lengths nearest to *P* were somewhat smaller was to be expected from the influence of the plates and of the curvature of the wire. This wave-length, with a period of 1.4 / hundred-millionths of a second, gives, 200,000 kilometers per second for the velocity of propagation of electrical waves in wires. Fizeau and Gounelle (Poggendorff's *Annalen*, vol. LXXX, p. 158, 1850) obtained for the velocity in iron wires 100,000 kilometers per second, and 180,000 in copper wires. W. Siemens (Poggendorff's *Annalen*, vol. CLVII, p. 309, 1876), by the aid of Leyden-jar discharges, obtained a velocity of from 200,000 to 260,000 kilometers per second in iron wires. Dr. Hertz's result is very nearly the mean of these, from which we may conclude that the order, at any rate, of the vibration period as calculated by him is correct. The value obtained cannot be regarded, independently of its agreement with experimental results otherwise obtained, as a fresh determination of the velocity, since it rests upon a theory which is open to doubt.

Interference of the direct actions with those transmitted through the wire.—If the square circuit *B* is placed at the null point in the second principal position, with the air space at its highest point, it will be unaffected by the waves in the wire, but the direct action when in this position was found to produce sparks 2 millimeters in length. *B* was then turned about a vertical axis into the first principal position in which there would be no direct action of the primary oscillation, but the waves in the wire gave rise to sparks, and by bringing *P* near enough to *A*, a sparking distance of 2 millimeters could be obtained. In the intermediate positions sparks were produced in both these ways, and it would therefore be possible to get a difference of phase, such that one should either increase or diminish the effect of the other. Phenomena of this nature were, indeed, observed. When the plane of *B* was in such a position that the normal drawn towards *A A′* was directed away from that side of the primary conductor on which *P* was placed, there was more sparking than even in the principal position; but if the normal were directed towards *P* the sparks disappeared, and only re-appeared when the air space was made smaller. When the air space was at the lowest point of *B*, the other conditions remaining the same, the sparks disappeared when the normal was turned away from *P*. Further variations of the experiment gave results in accordance with these.

It is easily seen that these phenomena were exactly what were to be expected. To fix the ideas, suppose the air space to be at the highest point and the normal directed towards *P*, as in Fig. 11. Consider what happens at the moment that the plate *A* has its greatest positive charge. The electro-static, and therefore the total E. M. F., is directed from *A* towards *A'*. The oscillation to which this gives rise in *B* is determined by the direction of the E. M. F. in the lower portion of *B*. Therefore positive electricity will flow towards *A'* in the lower portion, and away from *A'* in the upper portion.

Consider next the action of the waves. As long as *A* is positively charged, positive electricity will flow from the plate *P*. This current is, at the moment considered, at its maximum value at the middle point of the first half wave-length. A quarter of a wave-length further from the origin—that is to say, in the neighborhood of the null point—it first changes its direction. The E. M. F. of induction will here therefore impel positive electricity towards the origin. A current will therefore flow round *B* towards *A'* in the upper portion and away from *A'* in the lower portion. The electro-static and electro-dynamic E. M. F.'s are therefore in opposite phases and oppose each other's action. If the secondary circuit is rotated through 90 deg., through the first principal position, the direct action changes its sign, but not so the action of the waves, so that they now tend to strengthen each other. The same reasoning holds when the air space is at the lowest point of *B*.

Greater lengths of wire were then included between *m* and *n*, and it was found that the interference became gradually less marked, until with a length of 2.5 meters it disappeared entirely, the sparks being of equal length whether the normal were directed towards or away from *P*. When the length of wire between *m* and *n* was further increased, the distinction between the different quadrants re-appeared, and with a length of 4 meters the disappearance of the sparks was fairly sharp. The disappearance however then took place (with the air space at the highest point) when the normal was directed away from *P*, the opposite direction to that in which the disappearance previously took place. With a still further increase in the length of the wire the interference re-appeared and returned to its original direction with a length of 6 meters. These phenomena are clearly to be explained by the retardation of the waves in the wire, and show that here again the direction of motion in the advancing waves changes its sign at intervals of about 2.8 meters.

To obtain interference phenomena with the secondary circuit *C* in the third principal position, the rectilinear wire must be removed from its original position, and placed in the horizontal plane through *C* either on the side of the plate *A* or of the plate *A'*. Practically it is sufficient to stretch the wire loosely, and to fix it by means of an insulated clamp on each side of *C* alternately. It was found that when the wire was on the same side as the plate *P* the waves in it diminished the previous sparking, and when on the opposite side the sparking was increased, both results being unaffected by the position of the air space in the secondary circuit. Now it has been already pointed out that at the moment when the plate *A* has its maximum positive charge, and at which therefore the primary current begins to flow from *A*, the current at the first node of the rectilinear wire begins to flow away from the origin. The two currents therefore flow around *C* in the same direction when *C* lies between the rectilinear wire and *A* and in opposite directions when the wire and *A* are on the same side of *C*. The fact that the position of the air space is indifferent confirms the conclusion formerly arrived at, that the direction of oscillation is that due to the electro-dynamic E. M. F. These interferences are also changed in direction when the wire *m n*, 1 meter in length, is replaced by a wire 4 meters in length.

Dr. Hertz also succeeded in obtaining interference phenomena when the center of the secondary circuit was not in the base-line, but these results were of no special importance, except that they confirmed the previous conclusions.

Interference phenomena at various distances.—Interference may be produced with the secondary at greater distances than that of the null point, but care must then be taken that the action of the waves in the wire is of about the same magnitude as the direct action of the primary circuit through the air. This can be effected by increasing the distance between *P* and *A*.

Now if the velocity of propagation of the electro-dynamic disturbances through the air is infinite, the interference will change its sign at every half-wave length in the wire—that is to say, at intervals of about 2.8 meters. If the velocities of propagation through the air and through the wire are equal, the interference will be in the same direction at all distances. Finally, if the velocity of propagation through the air is finite, but different from the velocity in the wire, the interference will change in sign at intervals greater than 2.8 meters.

The interferences first investigated were those which occurred when the secondary circuit was rotated from the first into the second principal position, the air space being at the highest point. The distance of the secondary from the null point was increased by half-meter stages from 0 up to 8 meters, and at each of these positions an observation was made of the effects of directing the normal towards and away from *P* respectively. The points at which no difference in the sparking was observed in the two positions of the normal are marked 0 in the table below. Those in which the sparking was least, showing the existence of interference, when the normal was directed towards P are marked +, and those in which the sparking was least when the normal was directed away from *P* are marked —. The experiments were repeated with different lengths of wire *m n*, varying by steps of half a meter from 1 meter up to 6 meters. The first horizontal line in the table gives the distances in meters of the center of the secondary circuit from the null point, while the first vertical line gives the lengths of the wire *m n*, also in meters:

TABLE I.

	0		1		2		3		4		5		6		7		8
100	+	+	0	—	—	—	—	—	0	0	0	0	0	+	+	+	+
150	+	0	—	—	—	—	0	0	0	0	0	+	+	+	+	+	0
200	0	—	—	—	—	—	0	+	+	+	+	+	0	0	0	0	0
250	0	—	—	—	—	0	0	+	+	+	+	0	0	0	0	0	0
300	—	—	—	—	0	+	+	+	+	+	0	0	0	0	—	—	—
350	—	—	0	+	+	+	+	+	+	0	0	0	—	—	—	—	—
400	—	—	0	+	+	+	+	0	0	0	0	—	—	—	—	—	—
450	—	0	+	+	+	+	+	0	0	0	—	—	—	—	—	—	0
500	—	0	+	+	+	+	0	—	—	—	—	—	0	0	0	0	+
550	0	+	+	+	+	0	0	—	—	—	—	—	0	0	0	0	+
600	+	+	+	+	0	0	—	—	—	—	—	0	0	+	+	+	+

An inspection of this table shows, in the first place, that the changes of sign take place at longer intervals than 2.8 meters; and in the second place that the change of phase is more rapid in the neighborhood of the origin than at a distance from it. As a variation in the velocity of propagation is very unlikely, this is probably due to the fact indicated by theory that the electro-static E. M. F., which is more powerful than the electro-dynamic E. M. F. in the neighborhood of the primary oscillation, has a greater velocity of propagation than the latter.

In order to obtain a definite proof of the existence of similar phenomena at greater distances, Dr. Hertz continued the observations in the case of three of the lengths *m n* up to a distance of 12 meters, and the result is given in the table below:

TABLE II.

	0	1	2	3	4	5	6	7	8	9	10	11	12
100	+	0	—	—	0	0	0	+	+	+	+	+	0
250	0	—	—	0	+	+	0	0	0	0	—	—	—
400	—	0	+	+	0	0	—	—	—	—	0	0	0

If we make the assumption that at the greater distance it is only the E. M. F. of induction which produces any effect, the experiments would show that the interference of the waves excited by the E. M. F. of induction with the original waves in the wire changes its sign only at intervals of about 7 meters.

In order to investigate the E. M. F. of induction close to the primary oscillation, where the results are of special importance, Dr. Hertz made use of the interferences which were obtained when the secondary circuit was in the third principal position, and the air space was rotated through

90 degrees from the base-line. The direction of the interference at the null point, which has already been considered, was taken as negative, the interference being considered positive when it was produced by the passage of waves on the side of *C* remote from *P*, which makes the signs correspond with those of the previous experiments. It must be borne in mind that the direction of the resultant E. M. F. at the null point is opposed to that of the E. M. F. of induction, and therefore the first table would have begun with a negative sign if the electro-static E. M. F. could have been eliminated. The present experiments showed that up to a distance of 3 meters interference continued to occur, and always of the same sign as at the null point. It was unfortunately impossible to extend these observations to a greater distance than 4 meters, on account of the feebleness of the sparks, but the results obtained were sufficient to give distinct evidence of a finite velocity of propagation of the E. M. F. of induction. These observations, like the former ones, were repeated with various lengths of the wire *m n* in order to exhibit the variation in phase, and the results obtained are given in the table below:

TABLE III.

	0	1	2	3	4
100	—	—	—	—	0
150	—	—	0	0	0
200	0	0	0	+	+
250	0	+	+	+	+
300	+	+	+	+	+
350	+	+	+	+	0
400	+	+	+	+	0
450	+	+	+	0	0
500	+	+	0	0	0
550	+	0	0	0	–
600	0	—	—	—	—

which shows that as the distance increases the phase of the interference changes in such a manner that a reversal of sign takes place at intervals of from 7 to 8 meters. This result is further confirmed by comparing the results of Table III with the results for greater distances given in Table II, for in the former series, the effect of the electro-static E. M. F. is eliminated, owing to the special position of the secondary circuit, while in the former it becomes insensible at the greater distances, owing to its rapid decrease with increasing distance. We should therefore expect the results given in the first table for distances beyond 4 meters to follow without a break the results given in Table III for distances up to 4 meters. This was found to be the case, as is evident from inspection of Tables II and III.

To show this more clearly the signs of the interference of the waves, due to the electro-dynamic E.M.F., with the waves in the wire, are collected together in Table IV, the first four columns of which are taken from Table III, and the remaining columns from Table II.

TABLE IV.

	0	1	2	3	4	5	6	7	8	9	10	11	12
100	—	–	–	—	0	0	0	+	+	+	+	+	0
250	0	+	+	+	+	+	0	0	0	0	—	—	—
400	+	+	+	+	0	0	—	—	—	—	0	0	0

From the results given in this table, the author draws the following conclusions:

(1) The interference does not change its sign at intervals of 2.8 meters. The electro-dynamic actions are therefore not propagated with an infinite velocity.

(2) The interference is not in the same phase at all points, therefore the electro-dynamic actions are not propagated through air with the same velocity as electric waves in wires.

(3) A gradual retardation of the waves in the wire has the effect of displacing a given phase of the interference towards the origin of the waves. The velocity of propagation through the air is therefore greater than through a wire.

(4) The sign of the interference is reversed at intervals of 7.5 meters, and therefore in traversing this distance an electro-dynamic wave gains one length of the waves in the wire.

Thus, while the former travels 7.5 meters, the latter travels 7.5—2.8=4.7 meters, and therefore the ratio of the velocities is 75: 47, which gives for the half-wave length of the electro-dynamic action 2.8+75/47=4.5 meters. Since this distance is traversed in 1.4/ hundred millionths of a second, the absolute velocity of propagation through the air must be 320,000 kilometers per second. This result can only be considered reliable as far as its order is concerned; but its true value can hardly exceed half as much again, or be less than two-thirds of this amount. In order to obtain a more accurate determination of the true value it will be necessary to determine the velocity of electric waves in wires with greater exactness.

It does not necessarily follow from the fact that in the immediate neighborhood of the primary oscillation the interference changes its sign after an interval of 2.8 meters that the velocity of propagation of the electro-static action is infinite, for such a conclusion would rest upon a single change of sign, which might moreover be explained independently of any change of phase, by a change in the sign of the amplitude of the resultant force at a certain distance from the primary oscillation. Quite independently however of any knowledge of the velocity of propagation of electrostatic actions, there exist definite proofs that the rates of propagation of electro-static and electro-dynamic E. M. F.'s are unequal.

In the first place the total force does not vanish at any point on the base-line. Now, near the primary, the electro-static E. M. F. is the greater, while the electro-dynamic E. M. F. is the greater at greater distances. There must therefore be some point at which they are equal, and since they do not balance, they must take different times to reach this point.

In the second place, the existence of points at which the direction of the resultant E. M. F. becomes indeterminate does not seem capable of explanation, except on the supposition that the electrostatic and electro-dynamic components perpendicular to each other are in appreciably different phases, and therefore do not compound into a rectilinear oscillation in a fixed direction. The fact that the two components of the resultant are propagated with different velocities is of considerable importance, in that it gives an independent proof that one of them at any rate must have a finite velocity of propagation.

The latest researches of Dr. Hertz on electrical oscillations of which accounts have been published at present, are described in a paper "On Electro-Dynamic Waves in Air, and their Reflections," in Wiedemann's *Annalen*, 1888, vol. XXXIV, p. 609. The author had been endeavoring to find a more striking and direct proof of the finite velocity of propagation of electro-dynamic waves than those which he had hitherto given, for though these are quite sufficient to establish the fact, they can only be properly appreciated by one who has obtained a grasp of the results of the entire series of researches.

In many of the experiments which have been described, Dr. Hertz had noticed the appearance of sparks at points in the secondary conductor, where it was clear from geometrical considerations that they could not be due to direct action, and it was observed that this occurred chiefly in the neighborhood of solid obstacles. It was found moreover, that in most positions of the secondary conductor the feeble sparks produced at a great distance from the primary became considerably stronger in the vicinity of a solid wall, but disappeared with considerable suddenness quite close to the wall. The most obvious explanation of these experiments was that the waves of inductive action were reflected from the wall and interfered with the direct waves, especially as it was found that the phenomena became more distinct when the circumstances were such as to favor reflection to the greatest possible extent. Dr. Hertz therefore determined upon a thorough investigation of the phenomena.

The experiments were made in the Physical Lecture Theatre, which is 15 meters in length, 14 meters in width, and 6 meters in height. Two rows of iron columns, running parallel to the sides of the room, would collectively act almost like a solid wall towards electro-dynamic action, so that the available width of the room was only 8.5 meters. All pendent gas-fittings were removed, and the room left empty, with the exception of wooden tables and forms, which would not exert any appreciable disturbing effect. The end wall, from which the waves were to be reflected, was of solid sandstone, with two doors in it, and the numerous gas pipes attached to it gave it, to a certain extent, the character of a conducting surface, and this was increased by fastening to it a sheet of zinc 4 meters high and 2 meters broad, connected by wires to the gas-pipes and a neighboring water-pipe. Special care was taken to provide an escape for the electricity at the upper and lower extremities of the zinc plate, where a certain accumulation of electricity was to be expected.

The primary conductor was the same that was employed in the experiments last described, and was placed at a distance of 13 meters from the zinc plate, and therefore two meters from the wall at the other end of the room. The conducting wire was placed vertically, so that the E. M. F.'s to be considered increased and diminished in a vertical direction. The center of the primary conductor was 2.5 meters above the floor of the room, which left a clear space for the observations above the tables and benches. The point of intersection of the reflecting surface with the perpendicular from the center of the primary conductor will be called the point of incidence, and the experiments were limited to the neighborhood of this point, as the investigation of waves striking the wall at a considerable angle would be complicated by the differences in their polarization. The plane of vibration was therefore parallel to the reflecting surface, and the plane of the waves was perpendicular to it, and passed through the point of incidence.

The secondary conductor consisted of the circle of 35 centimeters radius, which has been already described. It was movable about an axis through its center perpendicular to its plane, and the axis itself was movable in a horizontal plane about a vertical axis. In most of the experiments the secondary conductor was held in the hand by its insulating wooden support, as this was the most convenient way of bringing it into the various positions required. The results of these experiments however had to be checked by observations made with the observer at a greater distance from the secondary, as the neighborhood of his body exerted a slight influence upon the phenomena. The sparks were distinct enough to be observed at a distance of several meters when the room was darkened, but when the room remained light they were practically invisible even when the observer was quite close to the secondary.

When the center of the secondary was placed in the line of incidence and with its plane in the plane of vibration, and the air space was turned first towards the reflecting wall and then away from it, a considerable difference was generally observed in the strength of the sparks in the two positions. At a distance of about 0.8 meter from the wall the sparks were much stronger when the air space was directed towards the wall, and its length could be adjusted so that while there was a steady stream of sparks when in this position, they disappeared entirely when the air space was directly away from the wall. These phenomena were reversed at a distance of 3 meters, and recurred, as in the first case, at a distance of 5.5 meters. At a distance of 8 meters the sparks were stronger when the air space was turned away from the wall, as at the distance of 3 meters, but the difference was not so well marked. When the distance was increased beyond 8 meters no further reversal took place, owing to the increase in the direct effect of the primary oscillation and the complicated distribution of the E. M. F. in its neighborhood.

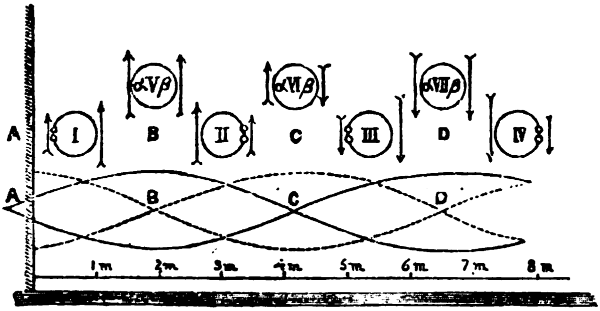

FIG. 12

The positions I, II, and IV, (Fig. 12) of the secondary circle are those in which the sparks were strongest, the distance from the wall being shown by the horizontal scale at the foot. When the secondary circle was in positions V, VI, and VII, the sparks were equally strong in both positions of the air space, and quite close to the wall the difference between the sparking in the two positions again diminished. Therefore the points A, B, C, D in the diagram may in a certain sense be regarded as nodes. The distance between two of these points must not however be taken as the wave half-length, for if all the electrical motions changed their directions on passing through one of these points the phenomena observed in the secondary circuit would be repeated without variation, since the direction of oscillation in the air space is indifferent.

The conclusion to be drawn from the experiments is that in passing any one of these points part of the action is reversed, while another part is not. The experimental results however warrant the assumption that twice the distance between two of these points is equal to the half wave-length, and when this assumption is made the phenomena can be fully explained.

For suppose a wave of E. M. F., with oscillations in a vertical direction, to impinge upon the wall, and to be reflected with only slightly diminished intensity, thus giving rise to stationary waves. If the wall were a perfect conductor, a node would necessarily be formed in its surface, for at the boundary and in the interior of a perfect conductor the E. M. F. must be infinitely small. The wall cannot however be considered as a perfect conductor, for it was not metallic throughout, and the portion which was metallic was not of any great extent. The E. M. F. would therefore have a finite value at its surface, and would be in the direction of the impinging waves. The node, which in the case of perfect conductivity would occur at the surface of the wall, would therefore actually be situated a little behind it, as shown at A in the diagram. If then twice the distance A B—that is to say, the distance A C—is half the wave-length the steady waves will be as represented by the continuous lines in Fig. 12. The E. M. F.'s acting on each side of the circles, in the positions I, II, III, and IV, will therefore at a given moment be represented in magnitude and direction by the arrows on each side of them in the diagram. If therefore in the neighborhood of a node the air space is turned towards the node, the strongest E. M. F. in the circle will act under more favorable conditions against a weaker one under less favorable conditions. If however the air space is turned away from the node, the stronger E. M. F. acts under less favorable conditions against a weaker one under more favorable conditions. In the latter case the resultant action must be less than in the former, whichever of the two E. M. F.'s has the greater effect, which explains the change of sign of the phenomenon at each quarter wave-length.

This explanation is further confirmed by the consideration that if it is the true one, the change of sign at the points B and D must take place in quite a different manner from that of the point C. The E. M. F.'s, acting on the secondary circle, in the positions V, VI, and VII, are shown by the corresponding arrows, and it is clear that in the positions B and D, if the air space is turned from one side to the other, the vibration will change its direction round the circle, and therefore the sparking must during the rotation vanish either once or an uneven number of times. In the position C, however, the direction of vibration remains unaltered, and therefore the sparks must disappear an even number of times, or not at all.

The experiments showed that at B and D the sparking diminished as the air space receded from α, vanished at the highest point, and again attained its original value at the point β. At C, on the other hand, the sparking continued throughout the rotation, being a little stronger at the highest and lowest points. If then there is any change of sign in the position C, it must occur with very much smaller displacements than in other positions, so that in any case there is a distinction such as required between this and the other two cases.

Another very direct proof of the truth of Dr. Hertz's presentation of the nature of the waves was obtained. If the secondary circle lies in the plane of the waves instead of in the plane of vibration, the E. M. F. must be equal at all points of the circle, and for a given position of the air space, the sparking must be directly proportional to its intensity. When the experiment was made it was found, as expected, that at all distances the sparking vanished at the highest and lowest points of the circle, and attained a maximum value at the points in the horizontal plane through the point of incidence.

The air space was then placed at such a point and close to the wall and was then moved slowly away from the wall, when it was found that while there was no sparking quite close to the metal plate, it began at a very small distance from it, rapidly increased, reached a maximum at the point B, and then diminished again. At C the sparking again became excessively feeble, and increased as the circle was moved still further away. The sparking continued steadily to increase after this, as the motion of the circle was continued in the same direction, owing, as before, to the direct action of the primary oscillation.

The curves shown by the continuous lines in Fig. 12 were obtained from the results of these experiments, the ordinates representing the intensity of the sparks at the distances represented by the corresponding abscissæ.

The existence in the electrical waves of nodes at A and C, and of loops at B and D, is fully established by the experiments which have been described; but in another sense the points B and D may be regarded as nodes, for they are the nodal points of a stationary wave of magnetic induction which, according to theory, accompanies the electrical wave and lags a quarter wave-length behind it.

This can easily be shown to follow from the experiments, for when the secondary circle is placed in the plane of vibration with the air space at its highest point, there will be no sparking if the E. M. F. is uniform throughout the space occupied by the secondary. This can only take place if the E. M. F. varies from point to point of the circle, and if its integal round the circle differs from zero. This integral is proportional to the number of magnetic lines of force passing backwards and forwards across the circle, and the intensity of the sparks may be considered as giving a measure of the magnetic induction, which is perpendicular to the plane of the circle. Now in this position vigorous sparking was observed close to the wall, diminishing rapidly to zero as the point B was approached, then increasing to a maximum at C, falling to a well-marked minimum at D, and finally increasing continuously as the secondary approached still nearer to the primary. If the intensities of these sparks are taken as ordinates, positive and negative, and the distances from the wall as abscissæ, the curve shown by the dotted lines in Fig. 12 is obtained, which therefore represents the magnetic waves.

The phenomena observed in the first series of experiments described in this paper may therefore be regarded as due to the resultant electric and magnetic actions. The former changes sign at A and C, the latter at B and D, so that at each of these points one part of the action changes sign, while the other does not, and therefore the resultant action, which is their product must change sign at each of these points, as was found to be the case.

When the secondary circle was in the plane of vibration the sparking in the vicinity of the wall was observed to be a maximum on the side towards the wall, and a minimum at the opposite side, and as the circle was turned from one position to the other there was found to be no point at which the sparks disappeared. As the distance from the wall was increased, the sparks on the remote side gradually became weaker, and vanished at a distance of 1.08 meters from the wall. When the circle was carried further in the same direction, the sparks appeared again on the side remote from the wall, but were always weaker than on the side next to it; the sparking however no longer passed from a maximum to a minimum merely, but vanished during the rotation once in the upper and once in the lower half of the circle. The two null points gradually receded from their original coincident positions, until at the point B they occurred at the highest and lowest points of the circle. As the circle was moved further in the same direction, the null points passed over to the side next to the wall, and approached each other again, until when the center was at a distance of 2.35 meters from the wall the two null points were again coincident. B must be exactly half-way between this point and the similar point previously observed, which gives 1.72 meters as the distance of B from the wall, a result which agrees, within a few centimeters with that obtained by direct observation. Moving further in the direction of C, the sparking at different points of the circle became more nearly equal, until at C it was exactly so. In this position there was no null point, and as the distance was further increased the phenomena recurred in the same order as before.

Dr. Hertz found that the position of C could be determined within a few centimeters, the determinations of its distance from the wall varying from 4.10 to 4.15 meters; he gives its most probable value as 4.12 meters. The point B could not be observed with any exactness, the direct determinations varying from 6 to 7.5 meters as its distance from the wall. It could however be determined indirectly, for the distance between B and C being found to be 2.4 meters, taking this as the true value, A must have been 0.68 meter behind the surface of the wall, and 6.52 meters in front of it. The half-wave length would be 4.8 meters, and by an indirect method it was found to be 4.5 meters, so that the two results agree fairly well. Taking the mean of these as the true value, and the velocity of light as the velocity of propagation, gives as the vibration period of the apparatus 1.55/ hundred-millionths of a second, instead of 1.4/ hundred-millionths, which was the theoretically calculated value.

A second series of experiments were made with a smaller apparatus, and though the measurements could not be made with as much exactness as those already described, the results showed clearly that the position of the nodes depends only on the dimensions of the conductors and not on the material of the wall.

Dr. Hertz states that after some practice he succeeded in obtaining indications of reflection from each of the walls. He was also able to obtain distinct evidence of reflection from one of the iron columns in the room, and of the existence of electro dynamic shadows on the side of the column remote from the primary.

In the preceding experiments the secondary conductor was always placed between the wall and the primary conductor;—that is to say, in a space in which the direct and reflected rays were travelling in opposite directions, and gave rise to stationary waves by their interference.

He next placed the primary conductor between the wall and the secondary, so that the latter was in a space in which the direct and reflected waves were traveling in the same direction. This would necessarily give rise to a resultant wave, the intensity of which would depend on the difference in phase of the two interfering waves. In order to obtain distinct results it was necessary that the two waves should be of approximately equal intensities, and therefore the distance of the primary from the wall had to be small in comparison with the extent of the latter, and also in comparison with its distance from the secondary.

To fulfill these conditions the secondary was placed at a disadvantage of 14 meters from the reflecting wall, and therefore about 1 meter from the opposite one, with its plane in the plane of vibration, and its air space directed towards the nearest wall, in order to make the conditions as favorable as possible for the production of sparks. The primary was placed parallel to its former position, and at a perpendicular distance of about 30 centimeters from the center of the reflecting metallic plate. The sparks observed in the secondary were then very feeble, and the air space was increased until they disappeared. The primary conductor was then gradually moved away from the wall, when isolated sparks were soon observed in the secondary, passing into a continuous stream when the primary was between 1.5 and 2 meters from the wall;—that is, at the point B. This might have been supposed to be due to the decrease in the distance between the two conductors, except that as the primary conductor was moved still further from the wall the sparking again diminished, and disappeared when the primary was at the point C. After passing this point the sparking continually increased as the primary approached nearer to the secondary. These experiments were found to be easy to repeat with smaller apparatus, and the results obtained confirmed the former conclusion, that the position of the nodes depends only on the dimensions of the conductor, and not on the material of the reflecting wall.

Dr. Hertz points out that these phenomena, which are exactly analogous to the acoustical experiment of approaching a vibrating tuning-fork to a wall, when the sound is weakened in certain positions and strengthened in others, and also to the optical phenomena illustrated in Lloyd's form of Fresnel's mirror experiments; and as these are accepted as arguments tending to prove that sound and light are due to vibration, his investigations give a strong support to the theory that the propagation of electro-magnetic induction also takes place by means of waves. They therefore afford a confirmation of the Faraday-Maxwell theory of electrical action. He points out however that Maxwell's, in common with other electrical theories, leads to the conclusion that electricity travels through wires with the velocity of light, a conclusion which his experiments show to be untrue. He states that he intends to make this contradiction between theory and experiment the subject of further investigation.

REPETITION OF HERTZ'S EXPERIMENTS,

AND DETERMINATION OF THE DIRECTION OF THE VIBRATION OF LIGHT.*

By **Frederick T. Trouton.**

Since last October (1888), Professor Fitzgerald and I have been repeating some of Professor Hertz's experiments, as occasion allowed, and it may not be without interest at the present time to give a short account of our work.

The first experiment tried was the interference of direct electro-magnetic radiation with that reflected from a metallic sheet. This experiment is analogous to that known in optics as "Lloyd's experiment."

The radiation was produced by disturbances caused in the surrounding space by electrical oscillations in a conductor. It was arranged in this wise. Two thin brass plates, about 40 centimeters square, were suspended by silk threads at about 60 centimeters apart, so as to be in the same plane. Each plate carried a stiff wire furnished at the end with a brass knob. The knobs were about 3 millimeters apart, so that on electrifying one plate a spark could easily pass to the other. This spark, as is well known, consists not simply of a transference of half the electricity of the first plate to the second—though this, which is the final state, is all that is observable by ordinary experimental methods—but the whole charge passes across to the second plate, then returns, and so on, pendulum-fashion, the moving part of the charge becoming less each time, till finally brought to rest, the energy set free at sparking being converted partly into heat in the wire and air break, partly into radiation into space, or in terms of action at a distance in inducing currents in other bodies.

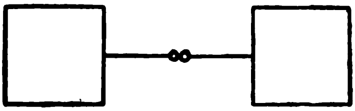

Fig. 1.

The time taken by the charge to pass over to the second plate and to return, is a definite thing for a given sized arrangement, and depends on the connection between them. If C be the capacity of the plates, and I the self-induction of the connection, the time of each complete oscillation equals $2\pi\sqrt{(CI)}$. The time in the case of the particular arrangement used is (speaking roughly) about the $\frac{1}{30,000,000}$ (one/thirty-millionth) of a second.

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* From *Nature*, Feb. 21, 1889, vol. xxxix, pp. 391-393.

If there be conductors in the neighborhood of this "vibrator," currents will as usual be induced in each on every passage of the charge between the plates, each passage serving simply as a primary current.

Now, speaking briefly, the whole object of the experiment is to find out if these induced currents take place simultaneously in conductors situated at various distances from the primary current, and if not, to determine the delay. In order to do this we must, in the first place, be possessed of some means of even ascertaining that these currents occur, all ordinary methods being inadequate for detecting currents lasting only for such exceedingly short periods as these do. By devising how to determine the existence of these currents, Hertz made the experiment possible.

His method depends on the principle of resonance, previously suggested by Fitzgerald, and his current-observing apparatus is simply a conductor, generally a wire bent into an unclosed circle, which is of such a length that if a current be induced in it by a passage of a charge across the "vibrator" the return current or rush back of the electricity thus produced in the ends of the wire occurs simultaneously with the next impulse, due to the passage back across the "vibrator."

In this way the current in the "resonator" increases every time, so that at last the end charges, which are always of opposite sign, grow to be so great that sparks will actually occur if the ends of the wire are brought near together. Thus Hertz surmounted the difficulty previously experienced by Fitzgerald when proposing electro-magnetic interference experiments.

The time of vibration in this circle is, as before, $2\pi\sqrt{(CI)}$, but on account of difficulties in calculating these quantities themselves, the length of the wire is most readily found by trial. To suit the "vibrator" we used, it was about 210 centimeters of wire No. 17. The ends of the wire were furnished with small brass knobs, which could be adjusted as to distance between them, by a screw arrangement, the whole being mounted on a cross of wood for convenience in carrying about.

At first sight the simplest "resonator" to adopt would seem to be two more plates arranged similarly to the "vibrator," but it will be seen on consideration that it would not do, because no break for seeing the sparking could be put between the plates, for if it were, the first induced current would be too feeble to jump the break, so that the re-enforcement stage could never begin.*

The charging of the "vibrator" was effected by connecting the terminals of an induction-coil with the plates. In this way a continuous shower of sparks could be obtained in the resonating circle.

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* However, two pairs arranged in line, the pairs connected by a wire, could probably be got to spark between the center plates.

The circle in the interference experiment was held in the horizontal plane containing the axis of the "vibrator," the ends of the circle of wire being in such a position that a line joining the knobs was at right angles to the "vibrator." In this position only the magnetic part of the disturbance could affect the circle, the "magnetic lines of force," which are concentric circles about the axis of the "vibrator," passing through the "resonator" circles.

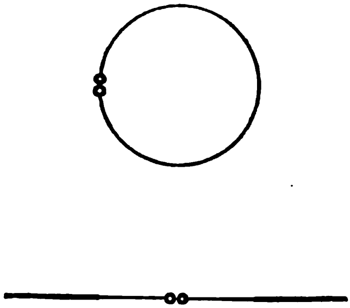


FIG. 2.

When the knobs of the circle are brought round through 90°, so as to be parallel to the "vibrator," the electric part of the disturbance comes into play, the electric lines of force being, on the whole, parallel to the axis of the "vibrator." The electric action alone can cause a forced vibration in the knobs, even when the connecting wire is removed, if placed fairly close to the "vibrator."

Again, if the knobs be kept in this position, but the circle be turned through 90°, so that its plane is vertical, only the electric part can act, the magnetic lines of force just grazing the circle. In this way the disturbance can be analyzed into its magnetic and electric constituents.

Lastly, if the knobs be in the first position, while the circle is vertical, there will be no action.

To exhibit these alone forms an interesting set of experiments. It also makes a very simple and beautiful experiment to take a wire twice as long and fix it instead of the first, but with two turns instead of one; no sparking is then found to occur. This is of course quite opposed to all ordinary notions, double the number of turns being always expected to give double the electro-motive force. In this way the reality of the resonance is easily shown.

Interference experiment.—The sparking of course becomes less intense as the resonator is carried away from the "vibrator," but by screwing the knobs nearer together it was possible to get sparks at 6 and 7 meters away. On bringing a large sheet of metal (3 meters square, consisting of sheet zinc) immediately behind the "resonator," when in sparking position, the sparking increased in brightness, and allowed the knobs to be taken further apart without the sparking ceasing; but when the sheet was placed at about 2.5 meters further back, the sparking ceased, and could not be obtained again by screwing up the knobs. On the other hand, when the sheet was placed at double this distance (about 5 meters), the sparking was slightly greater than without the sheet.

Now these *three* observations can only be explained by the interference and re-enforcement of a direct action of the "vibrator" with one reflected from the metallic sheet, and in addition by the supposition that the action spreads out from the vibrator at a finite velocity. According to this explanation, in the first position the reflected part combines with the direct and reinforces its effects. In the second position (that of no sparking), the reflected effect in going to the sheet and returning has taken half the time of a complete vibration of the "vibrator," and so is in the phase opposite to the incident wave, and consequently interferes with it.

If it were possible to tell the direction of the current in a "resonator" at any moment, then, by employing two of them, and placing one just so much beyond the other that the currents induced in them were always in opposite directions, we would obtain directly the half-way length. Now by reflection, we virtually are put in possession of two "resonators," which we are enabled to place at this distance apart, although unable to tell more than whether there be a current or not.

The distance from the position of interference to the sheet is a quarter of the wave-length, being half the distance between these simultaneous positions of opposite effects.

In the third position, the reflected wave meets the effect of the next current but one, in the "vibrator," after the current it itself emanated from, and since these two currents are in the same direction, their effects re-enforce each other in the "resonator." This occurs at half the wave-length from the sheet.

The first two observations alone could be explained by action at a distance, by supposing the currents induced in the metallic sheet to oppose the direct action in the "resonator" everywhere, and by also supposing that in the immediate neighborhood of the sheet, the direct action is overmastered by that from the sheet, while at 2.5 meters away the two just neutralize each other.

On this explanation, at all distances further the direct action should be opposed by that from the sheet, so that the fact of being increased at 5 meters upsets this explanation. Again, behind the sheet, evidently on this supposition, the two actions should combine so as to increase the sparking, but instead of this the sparking was found to cease on placing the sheet in front of the "resonator."

In performing these experiments, the "resonator" circle was always placed in the position in which only the magnetic part of the disturbance had effect. Hertz has also used the other positions of the resonating circle, whereby he has observed the existence of an electric disturbance coincident with the magnetic one, the two together forming the complete electro-magnetic wave.

Ordinary masonry walls were found to be transparent to radiation of this wave-length (that is, of about 10 meters), and some visitors to the opening meeting of the Dublin University Experimental Society, last November, were much astonished by seeing the sparking of the resonating circle out in the College Park, while the vibrator was in the laboratory.

Attempts were first made last December to obtain reflection from the surface of a non-conductor, with the hope of deciding by direct experiment whether the magnetic or electric disturbance was in the plane of polarization; that is, to find out whether the "axis of the vibrator" should be at right angles to the plane of reflection or in it, when at the polarizing angle, for obtaining a reflected radiation. It is to be observed that in these radiations the electric vibration is parallel to the "axis of the vibrator" while the magnetic is perpendicular to it, and that they are consequently polarized in the same sense as light is said to be polarized.

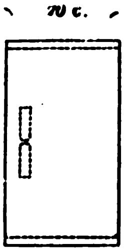
Two large glass doors were taken down and used for this purpose, but without success; and until lately, when reflection from a wall was tried, the experiment seemed unlikely to be successful.

In working with the glass plate, the resonator circle was first placed so that the "vibrator" had no effect on it. Then the glass plate was carried into position for reflection, but without result, though even the reflection from the attendants moving it was amply sufficient to be easily detected.

To obviate a difficulty arising from the fact that the wave was divergent, we decided to try Hertz's cylindrical parabolic mirrors, for concentrating the radiation. Two of these were made with sheets of zinc nailed to wooden frames, cut to the parabolic shape required.

In the "focal line" (which was made 12.5 centimeters from the vertex) of one of these, a "vibrator" was placed, consisting of two brass cylinders in line, each about 12 centimeters long and 3 centimeters in diameter, rounded at the sparking ends.

In order that the "resonator" wire may lie in the "focal line" of the receiving mirror, it has to be straight; this necessitates having two of them. They each consist of a thick wire 50 centimeters long, lying in the "focal line," and of a thin wire, 15 centimeters long, attached to one end at right angles, and which passes out to the back of the mirror through a hole in the zinc, where the sparking can be viewed, without obstructing the radiation in front. The total length of each "resonator" is about two wave-lengths, the wave-length being about 33 centimeters, so that it may be that there are two vibrating segments in each of these "resonators."



Side elevation.

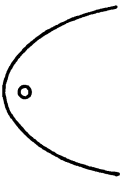


FIG. 3.—Plan.

With this apparatus it is possible to deal with definite angles of incidence. No effect was obtained with glass plates using these mirrors, whether the "vibrator" was perpendicular to the plane of reflection or in it. But with a wall 3 feet thick reflection was obtained, when the "vibrator" was perpendicular to the plane of reflection; but none, at least at the polarizing angle,* when turned through 90° so as to be in it.

This decides the point in question, the magnetic disturbance being found to be in the plane of polarization, the electric at right angles. Why the glass did not reflect was probably due to its thinness, the reflection from the front interfering with that from the back, this latter losing half a wave-length in reflection at a surface between a dense and a rare medium; and, as Mr. Joly pointed out, is in that case like the black spot in Newton's rings, or more exactly so, the black seen in very thin soap-bubbles.

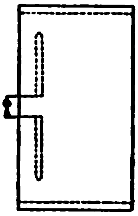


FIG. 4.

Hertz has pointed out several important things to be guarded against in making these experiments. Ultra-violet light, for example, falling on the "vibrator," prevents it working properly, the sparking in the resonator ceasing or becoming poor. Also the knobs of the "vibrator" must be cleaned of burnt metal, and polished every quarter of an hour at least, to prevent a like result.

Both these effects probably arise, as suggested by Mr. Fitzgerald, from a sort of initial brush discharging (either ultra-violet light or points being capable of doing this), which prevents the discharging impulse being sufficiently sudden to start the oscillation in the "vibrator." For to start a vibration, the time of impulse must be short compared with the time of oscillation. These precautions therefore become especially needful when working with small-sized "vibrators." Possibly charging the "vibrator" very suddenly, after the manner of one of Dr. Lodge's anti-lightning-rod experiments, would save the irksome necessity of repeatedly cleaning the knobs of the "vibrator."

Several important problems seem to be quite within reach of solution by means of these Hertzian waves, such for instance as dispersion. Thus it could be tried whether placing between the reflector and the "resonator" conducting bodies of nearly the same period of vibration as the waves used would necessitate the position of the "resonator" being changed so as to retain complete interference. Or again, whether interspersing throughout the mass of a large Hertzian pitch-prism, conductors with nearly the same period would alter the angle of refraction. In some such way as this, anomalous dispersion, with its particular case of ordinary dispersion, may yet be successfully imitated.

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* Slight reflection was obtained at an incidence of 70°.

The determining the rate of propagation through a large tile, or sheet of sandstone, could be easily made by means of the interference experiment, by placing it between the screen and the "resonator."

EXPERIMENTS ON ELECTRO-MAGNETIC RADIATION,

INCLUDING SOME OF THE PHASE OF SECONDARY WAVES.*

In continuation of some experiments which were described in *Nature*, vol. XXXIX, p. 391 ("Repetition of Hertz's Experiments and Determination of the Direction of the Vibration of Light"), attempts were made to obtain periodic reflection of electric radiation from plates of different thicknesses, analogous to Newton's rings, with the view of further identifying these radiations with "light."

It was there described how a sheet of window-glass refused to reflect the Hertzian waves, but how a masonry wall reflected them readily. The non-reflection from the thin sheet is due to the interference of the reflected waves from each side which takes place owing to a change of phase of half a period on reflection at the second surface, as in the black spot of Newton's rings.

By making the reflection plate such a thickness that the reflection from the back has to travel half a wave-length farther than that from the front, the two reflections ought to be in accordance, for they differ by a whole period, half arising from difference in path, and half from change of phase on reflection; but if the difference in paths were made a whole wave-length by doubling the thickness of the plate, there ought again to be interference, and so on.

The first plan tried with this end in view, was to fill a large wooden tank to different depths with water or other liquids. On gradually filling the tank reflection should be obtained, and at a certain depth equal to $\frac{1}{4}(\lambda \sec r) \div \mu$, reach a maximum; further addition of the liquid then should diminish the reflection, and at double the above depth the reflection should reach a minimum, the two waves interfering.

The mirrors for concentrating the radiation had for this purpose to be suspended over the tank as shown in the figure. The tank was first tried empty, but unfortunately the wooden bottom was found to reflect,

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* From *Nature*, August 22, 1889, vol. XL, pp. 398–400.

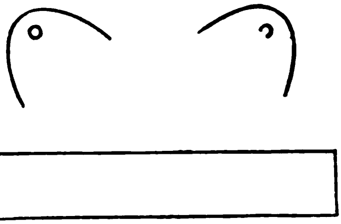

FIG. 5.

thus it was useless for the purpose intended. I then tried what ought to have been tried before constructing the tank, namely—whether ordinary boards, such as flooring, reflected. The floor was found to reflect readily. This was attributed to moisture in the wood causing it to conduct, specially as wood was found not to polarize by reflection. Experiments were then undertaken to determine if water reflected, even though in thin sheets. A large glass window was placed beneath the mirrors and flooded with water; this was found to reflect well, both when the mirrors were in the position shown and when rotated to the position "at right angles." Thus water also acts like a metal, reflecting the radiation however polarized. The glass had to be hardly more than damp to get some reflection.

The wooden tank being unsuitable, a glass tank was thought of, but was given up for solid paraffine, which, being in slabs, could be easily built up into a vertical wall of any desired thickness. Through the kindness of Mr. Rathborne a large quantity of this was lent for the purpose.

A thin sheet of paraffine about 2 centimeters thick was found not to reflect, as was expected. Next a wall 13 centimeters thick (180 centimeters long, 120 centimeters high) was tried, and found to reflect, this being the thickness required in order to add another half period to the retardation of the wave reflected from the back at an incident angle of 55°, the wave-length being taken as 66 centimeters, and the index of refraction being taken as 1.51, the square root of 2.29, the value taken as the specific inductive capacity of paraffine.

Then a wall twice the thickness was tried, but it also reflected, contrary to expectation. While in doubt as to the cause of this, it was decided to make a determination by direct experiment of the index of refraction of paraffine for these waves, by a method suggested in *Nature* (vol. XXXIX, p. 393), which consists in interposing a sheet or wall of paraffine between the resonator and the metallic reflection in the Hertzian experiment of loops and nodes which are formed by the interference of the reflected wave with the direct radiation; the ratio of the velocity in the wall to that in the air being easily found from the observed shifting of the loops and nodes towards the screen.

In this way the index of refraction for the radiation of the period employed was found to be about 1.8, so that the paraffine walls which had been used were too thick, the proper thickness being about 10 and 20 centimeters—exactly so for an incident angle of 51°. On making this alteration I fancied I could detect a slight difference between the reflections from the thick and thinner walls; still the difference was not sufficient to be at all satisfactory. The nature of the observing apparatus makes it almost impossible to say if the reflection on one occasion is more intense or less so than on another so long as sparks can be obtained. This is due to the sparking-point in the receiving apparatus continually requiring re-adjustment when working with small sparks, as the distance between them changes either from shaking or from the points getting burnt up.* Dust, and moisture from the observer's breath, are also troublesome. Thus it might be quite possible that the points had always to be much closer with the 20-centimeter wall than with the 10-centimeter wall in order to get sparks, and yet the difference escape detection; the thing observed being whether sparks can be obtained or not, the eye being incapable of comparing with any degree of accuracy the intensity of light on one occasion with that on another.

However, if it had been possible to suddenly change the wall, while viewing the sparking, from being 10 to 20 centimeters, it would have been easy to detect any difference which might have existed, but unfortunately it took some little time to alter the wall.

In order to obviate this difficulty the following device was resorted to with the object of showing that there was a difference in the behavior of the wall when 10 centimeters thick to its behavior when 20 centimeters thick. (For at the time I did not see that the experiment was inconclusive, the effects observed being the same whether the back reflected at all or not.) A *small* sheet of zinc was placed at the back of the wall, and the effect on the sparking observed while an attendant suddenly removed or again replaced the zinc. It was supposed that when the wall was 20 centimeters thick, and there was sparking, that on suddenly placing the zinc on the back the sparking would increase, owing to the phase of the reflection from the back being half a period different from that of the reflection from the zinc; but when the wall was 10 centimeters thick that the presence of the zinc would diminish the sparking.

It was with no little surprise that the reverse was observed. That is to say, placing a sheet of zinc about 30 centimeters square on the back of the wall actually aided the reflection from the back so as to diminish the sparking with the 20-centimeter wall, but increasing it with the 10-centimeter wall. This observation made it look as if it must be on the first reflection from the paraffine (that is to say, on passing from a rare to a dense medium) that the "change of phase" occurs, and not at the back (at a reflection from a dense to a rare medium), as is ordinarily supposed. For Hertz's experiment of loops and nodes showed that there was no change of phase on metallic reflection, that is, of the *magnetic* displacement; there is a change of phase of the *electric* displacement. It is important to bear in mind that the electric loop and the magnetic node occurred at the same place, and of course so too the electric node and the magnetic loop.

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* With *very small* sparks the thermal expansion must be counteracted by unscrewing.

In order to investigate this, attempts were made to obtain Hertz's loops and nodes off a paraffine wall as reflector, but no reflection could be discovered, the intensity of the vertically reflected rays being insufficient. However, by inclining the incident radiation to an angle of 57°, the intensity of the reflection was found to be amply sufficient.

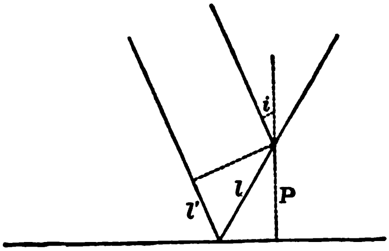

FIG. 6.

With a circular resonator, which is for these waves about 10 centimetres in diameter, sparks were obtained close to the reflector, the circle being held at right angles to the wall so as to be equally inclined to both direct and reflected radiation, and this was confirmed by a straight resonator giving none there. At 30 centimeters from the wall* there was interference with the circle, and vigorous sparking with the straight resonator. This being about the right distance for the loop to be from the reflector at an incident angle of 57°,

$$\tfrac{1}{2}\lambda = l + l' = p \sec i(1 + \cos 2i) = 2p \cos i.$$

Thus there is no doubt that it is on the second reflection that the change of phase occurs.

Here then was a difficulty; the small sheet of zinc at the back of the paraffin undoubtedly reflected with a change of phase, while, according to the Hertzian experiment, metallic reflection is unaccompanied by change of phase. On mentioning this to Professor Fitzgerald, he pointed out to me its complete agreement with wave theory. For by considering the secondary waves produced by dividing up a primary wave with reference to any point into half-period zones, it can be seen that the effect of the primary is equivalent to half of that arising from the central circle, and in consequence is half a period behind the phase which would be at the point if an infinitesimal portion of the center alone acted. For the effect of each ring can be considered as destroyed by half the effect of its two neighbors, and thus half the effect of the central circle is left uncompensated. But the distance of the edge of this circle is half a wave-length farther from the point than its center is, so that the resultant phase at the point will be behind that due to the center, but in front of that due to the edge, which effect would be half a period behind that arising from the center. Taking the mean between them, the resultant phase then at the point is a *quarter* of a period* behind what it would be if the center alone acted. Thus it was that the reflection from the *small* sheet of zinc differed from what I had expected it to be.

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* It would occur at about 17 centimeters on vertical reflection. This experiment was also tried with a metallic reflector.

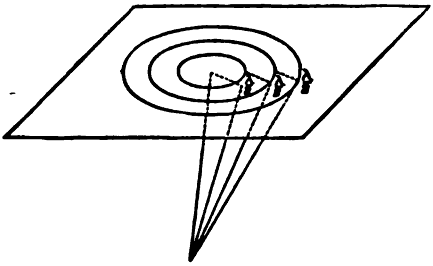

FIG. 7.

Experiment showing phase of secondary waves.—To experimentally test this, the small sheet of zinc was used as reflector in the Hertzian experiment of loops and nodes. Employing the circular resonator, the position of interference was found to have shifted out from 17 to over 24 centimeters, which nearly corresponds to an acceleration of phase of a quarter of a period, the wave going in all nearly a quarter of a wave-length farther, and nevertheless being still only half a period behind the phase on starting. The farthest out the loop could be is 25.5 centimeters: to obtain this would require an indefinitely small reflector. Of course, when the resonator was close in to the sheet, no change of phase was found to occur, the sheet being then practically infinite.

Another interesting observation was made. A *long* sheet of zinc, 30 centimeters wide, was found to act similarly to the sheet 30 centimeters square, provided it was placed with its breadth parallel to the electric displacement. When thus placed at 24 centimeters from the circular "resonator," there was interference, but on rotating the reflector so as its length was parallel to the electric displacement, sparking occurred, and now the "resonator" had to be brought back to 17 centimeters in order to again obtain interference. This experiment is interesting in connection with the electro-magnetic way of looking at the acceleration of phase as being due to the accumulations of electricity on the edges of the reflector, which is the same as the reason why it is necessary to use *long* cylindrical mirrors, as was pointed out by Professor Hertz in a letter last February to Professor Fitzgerald. This experiment is really the same as Stokes's *experimentum crucis*, as Professor Fitzgerald points out.

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* That it aided the back rather than the front was probably due to their phase not being an exact period or half period different from each other.

If instead of using the whole primary wave in the former experiment, it be passed through a screen with a hole in it (either square or a long slit at right angles to the electric displacement), the position of interference, as might be anticipated, was not shifted out as much as before. In the rough experiment made, it was found to occur at about 19 centimeters from the screen.

It was now thought well to repeat the determination of the index of refraction with a larger wall and metallic reflector than had been used before, as this change of phase might have affected the former results. But it was found that it had not done so to a sensible extent. However, the result of these new experiments was finally to give for paraffin, $\mu = 1.75$, and at the same time it was found that the wave-length given by the "vibrator" was 68 and not 66 centimeters, as had been assumed.

Two new knobs for the "vibrator" had been made, and the fact had been overlooked that they were slightly larger than the old ones, which gave a wave-length of 66 centimeters. These new knobs were electroplated with gold, and were a great saving of trouble, as they could be cleaned by merely rubbing with paper; apparently, the gold carried across by the sparking (in the form of a black powder) coming off,—but some may have re-burnished on. It was a curious thing that if the knobs were left uncleaned over night, the next morning it was very hard to get the black off,—some molecular change probably occurring.

If the value of μ thus found be not in some way due to the paraffin being in separate blocks, it would show a remarkable anomalous dispersion for paraffin near these curiously slow vibrations, and as suggested by Professor Fitzgerald, may be connected with the vibration periods of atoms in the molecule, as it can hardly be connected with the vibrations in the atoms themselves. It might be interesting to investigate whether these slow vibrations could cause dissociation, and thus lead to a photographic method of observing them. It may also be allied with ordinary electrolysis by very long period currents, as is also suggested by Professor Fitzgerald.

Assuming $\mu = 1.75$,* and $\lambda = 68$ centimeters, the thicknesses of the walls in the "Newton's ring" experiment, as above described, were wrong. However, it was found more convenient to alter the angle of incidence to suit the walls than to change the thickness of the walls. Thus, the mirrors were put at 25°, which is the proper angle with the above data for 10 centimeter and 20 centimeter walls. On now repeating the experiment, better results were obtained than I should have anticipated. When the wall was 10 centimeters thick, continuous sparking was easily obtained, but when 20 centimeters thick, it was only after much adjustment and patience that perhaps one slight spark could be obtained. This was quite sufficient, considering the nature of the wall, for it was only built up of plates, which afforded internal reflections, weakening the transmitted rays, and also since it requires the sum of the effect arising from the multiple reflections back and forward inside the wall to completely interfere with the front, and some of these are lost at the edge of the beam.

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*This value agrees with polarization experiments. No reflection was obtained at its corresponding angle, while at $\tan^{-1} 1.51$ some sparks were occasionally seen.

PROGRESS OF METEOROLOGY IN 1889.

By George E. Curtis.

I.—INSTITUTIONS; INTERNATIONAL POLAR WORK; NECROLOGY.

U. S. Signal Office.—The work of the year has been prosecuted with no important change in the personnel of the office.

Professor Abbe has completed a report entitled "Preparatory Studies for Deductive Methods in Storm and Weather Predictions" which appears as appendix 15, annual report for 1889.

A limited number of lithographic copies of that portion of the bibliography of meteorology covering temperature and moisture have been issued under the editorship of Mr. O. L. Fassig, librarian and bibliographer.

New life has been infused into the river and flood service by Professor Russell, to whom the work has been intrusted, and it is now for the first time being conducted from the stand-point of scientific hydrology. The inter-relation of rainfall, evaporation, and discharge has been investigated, and the results, which are of as great value as the data now at hand admit, are published in appendix No. 14 of the annual report.

The instrument division, under the direction of Professor Marvin, has not only raised the standard of the instrumental work of the service by greater perfection in the details of operation, but has accomplished much valuable work, both theoretical and practical, in the perfecting of new instruments, and in the development of improved methods for the reduction and treatment of instrumental records.

Capt. H. H. C. Dunwoody has been in the charge of the weekly weather crop bulletin, and of the work co-operative with the State weather services, of which latter the field of operation and usefulness have been largely extended during the past few years.

A valuable compilation of the rainfall statistics of the Pacific slope has been made, largely by Lieut. W. A. Glassford, and is published as a Congressional document. (1888).

The weather forecasts during the year have apparently not increased in accuracy.

Blue Hill Meteorological Observatory.—The observations for 1887 and 1888 have been published *in extenso* in quarto form as parts I and II, vol. XX, of the Annals of the Astronomical Observatory of Harvard

College. The director, Mr. A. L. Rotch, by his own private munificence in the establishment and equipment of this observatory, has made it a model of its kind, comparable with the best observatories of foreign meteorological institutions. The personal inspection of European observatories made by Mr. Rotch has enabled him to incorporate their best features in his own methods and equipment, and in the form of publication of results. The staff of the observatory has remained unchanged, with Mr. H. Helm Clayton as observer, and Mr. Fergusson as assistant.

The present volumes contain, besides the more usual observations and their summaries, hourly precipitation; hourly wind azimuths and movements; number of hours of prevalence of each wind direction; days of visibility of western mountains; hourly sunshine; hourly cloud observations from 8 A. M. to 11 P. M.; appendices in the volume for 1887, containing comparisons of thermometer shelters; investigations of normal and abnormal temperature differences between the base and summit; and meteorograms illustrating special phenomena. Of the observations above enumerated, the hourly cloud observations deserve special mention because of the indefatigable industry and enthusiasm necessary to their prosecution, and because of the interesting and important results that promise to be developed from their discussion.

Indian meteorological service.—The Report on the Administration of the Meteorological Department of the Government of India in 1887–'88 describes the actual working of the department and the condition of the observatories, and contains extracts from the reports of the inspection of the stations. Mr. Eliot has discontinued solar and terrestrial radiation observations except at a few selected stations. The calculation of daily averages, and the extension and improvement of the methods of collecting rainfall data have been undertaken. An observatory has been opened at Bagdad, and the question of establishing one at Perim, at the entrance of the Red Sea, has been suggested by the English Meteorological Council.

The International Meteorological Committee held a meeting at Zurich, September 3–5, 1888, at which the following resolution was adopted:

"The committee, in view of the circumstance that the assembling of an international meeting of the same character as the congress at Vienna and Rome presents great difficulties, considers that the commission it received at Rome is exhausted and that it ought to dissolve itself.

"At the same time, in order to continue the relations between the different meteorological organizations that have been productive of such good results during a series of years, the committee appoints a small bureau with the duty of using its best endeavors to bring about, at some convenient time, an international meeting of representatives of the different meteorological services."

By a subsequent resolution the bureau was made to consist of the president and secretary of the committee, Professor Wild and Mr. Scott. (*Nature*, XXXVIII, p. 491.)

The third general assembly of the Italian Meteorological Society, which is held every three years, met in Venice from 14th to 21st September, 1888.

The subjects of the programme were divided into four classes:

(1) General meteorology; (2) agricultural meteorology and phenology; (3) medical meteorology and hydrology; (4) geodynamics.

Among others, papers were presented upon the following topics:

General meteorology and climatology.—New studies and experiments of Prof. L. Palmieri on the origin of atmospheric electricity reported by Prof. Del Gaizo, of Naples. Results of the magnetic observations conducted at one hundred and sixty-three stations by P. Denza. Results of the meteorological observations made at the suggestion of the Society upon two Italian steamers, the *Generale* and the *Veloce*, extending through forty-three voyages in 1887. The helio-photometric observations of Prof. Friedrich Craveri, at the observatory of Bra, conducted since 1874 with a helio-photometer of his own construction. Two papers by Professor Busin, of Rome, upon the distribution of temperature in Italy, and upon the high and low pressures of the northern hemisphere. Notes by Professor Galli and by Professor Golfarelli upon the hourly velocity of the wind and upon lightning conductors. Professor Roberto described a new hygrometer.

Agricultural meteorology.—P. Ferrari, of Rome, gave an exposition of the present applications of meteorology to the interests of agriculture.

Medical meteorology and hydrology.—Discussion arose upon the disposition and classification of climatic stations. P. Siciliani, of Bologna, presented a paper on the relation between the height of water in wells and the air pressure. P. Bertelli took up the theories that assume electricity as the principal cause of earth tremors, and demonstrated their improbability. P. Denza had a paper on the more important earthquakes of 1887.

An Intercolonial Meteorological Congress was held at the Melbourne Observatory September 11–15, 1888, at which all the Australian colonies, New Zealand, and Tasmania were represented. The question of thermometer exposure was discussed at length. Mr. Todd considered it impossible for any one to say positively what is the best form of exposure, but had himself fully tested the Stevenson stand and should adopt it for his out-stations. Various other questions were discussed, including the relation of climatologic observations to hygiene, and the reduction of the barometer to sea-level.

International Meteorological Tables.—The International Meteorological Committee has published the collection of tables that have been in course of preparation for several years. They fill 400 quarto pages and the volume is sold for 35 francs. The tables include the reduction of both temperature and pressure to sea-level, conversion tables, units of measure, geodetic measures, hygrometric tables, and tables for the reduction of wind, rain, evaporation, magnetism, and electricity.

The sixth volume of the Reports of the International Bureau of Weights and Measures contains three papers of importance to instrumental meteorology in continuation of the valuable memoirs published in the preceding volumes, which include researches on the tensions of aqueous vapor, on the fixed points of thermometers, on the true weight of a litre of air, on the dilatation of mercury, on methods of verifying subdivided linear measures, on calibrating thermometers, and other thermometric studies. Of the papers in the new volume that are of meteorological interest, one (tome VI, pp. 620) is by Dr. René Benoît, on the measurement of dilatations by the method of M. Fizeau; one, on the comparison of mercurial thermometers with the air or hydrogen thermometer, by Dr. P. Chappuis; and a third paper, on practical formulæ for the transformation of thermometric co-efficients, by Dr. A. E. Guillaume.

Daily synoptic weather charts for the North Atlantic Ocean, Parts 1 to 5, September, 1883, to November, 1884, have been published conjointly by the Danish Meteorological Institute, and the Seewarte at Hamburg. Each volume contains a carefully prepared summary of the principal meteorological features. A novel point in the work is the introduction by Dr. Köppen of a new system of discussing the paths of storms in which their rate and direction of motion are shown to be dependent on surrounding conditions; so that the movement of cyclones is almost entirely dependent on their relation to existing anti-cyclones. Dr. Köppen finds one type of conditions to consist of an almost stationary anti-cyclone with a cyclone traveling along its borders. He then proceeds to represent all periods of this type on one chart, the number of such periods in the year ending with August, 1884, being fifty-seven, each ranging from three to eleven days each.

The movements of the cyclones during the type are represented by lines joining the ascertained positions each day, and by a simple arrangement the lowest pressure and the wind force are represented daily. The anti-cyclones are considered as stationary and the isobar of 30.12 inches has been adopted and plotted as the inclosing periphery of such areas; its position is the mean position of that isobar during the period; the maximum barometric readings are shown near the center, and the direction in which the highest pressures moved. By this method the representation of storm tracks has been greatly simplified. In some special cases one chart has been devoted to a single period showing conditions of marked interest. In the study of tropical cyclones the positions of neighboring anti-cyclonic areas is shown to govern the various paths pursued.

The publication of synoptic weather maps twice a day was begun by the Central Physical Observatory May 12, 1889. The map covers the whole of Europe, and a summary of the weather is given in Russian and French.

Lady Franklin Bay Expedition.—The official report of the Lady Franklin Bay Expedition, made by General A. W. Greely, has been published in two quarto volumes. The first volume contains the report of the commanding officer, and appendices containing detailed reports of special expeditions and the diaries of Lieutenant Lockwood and Sergeant Brainard; it is the history of the expedition. The second volume contains the main scientific results. The meteorological report and tables of observation occupy 360 pages. The annual mean temperature for three years was —3°.9 F., the lowest mean temperature known for any place on the globe. The precipitation was a little less than 4 inches a year, and evaporation in winter was found to be inappreciable. Auroras were neither frequent nor brilliant. Storms were not especially frequent or severe. In the winter, calms averaged seventeen hours daily, but during the continuous sunlight, only one hour daily. The diurnal oscillation of the barometer was less than 0.01 inch. These are a few striking results abstracted from this great store-house of arctic meteorology. Separate chapters are given to the astronomical observations for determining geodetic positions, and to the magnetic, tidal, and pendulum observations which are specially reported upon by the officers of the Coast and Geodetic Survey. A valuable bibliography of arctic literature is also given as an appendix.

M. **HERVÉ MANGON** died in Paris May 15, 1888. In his death agriculture and meteorology have lost a most active worker. In 1850 he published his "Études sur les Irrigations de la Campine Belge" and the "Travaux Analogues de la Sologne," which brought about important improvements in the French laws in relation to agriculture. Drainage was at that time scarcely known in France; in 1851 M. Mangon published a work on the subject which received from the Academy of Sciences the decennial prize for the most useful work on agriculture issued during the previous ten years. Irrigation and the fertilization of land were subjects to which he gave prolonged and careful study. These researches were followed by meteorological studies in which he took an active interest; he invented or improved many meteorological instruments and organized on his estate in Normandy a model meteorological station provided with the latest scientific improvements; he aided in the re-organization of the French Meteorological Service, and became the president of the meteorological council. He contributed also to the organization of the scientific mission to Cape Horn which obtained a large amount of valuable meteorological data. (*Nature*, XXXVIII, p. 111.)

Dr. O. J. **BROCH**, director of the International Bureau of Weights and Measures, died at Sevres, February 5, 1889, at the age of seventy-one. The memoirs on thermometry published by the bureau during his administration have greatly advanced the accurate measurement of temperature.

Prof. **ELIAS LOOMIS**, the American meteorologist, died at New Haven, August 15, 1889, at the age of seventy-eight years. For fifty-six years Professor Loomis had been engaged in collegiate work and in original research, having devoted the whole of his strength during the last ten years to the completion of his meteorological studies.

In a memoir by Prof. H. A. Newton (Am. Journ. Sci., June, 1890) a bibliography of Professor Loomis's writings is given containing 164 titles. These include contributions to astronomy, terrestrial magnetism, and meteorology, and a series of mathematical text-books which attained an extensive circulation.

His first work in terrestrial magnetism consisted in a series of hourly observations (seventeen hours each day) of the magnetic needle for a period of thirteen months in 1834 and 1835. With exception of a short series by Professor Bache this was the earliest series of hourly magnetic observations in America, and only one or two ante-dated it in Europe.

Professor Loomis's early work in astronomy was likewise at the time when that science was having its beginning in this country. Comets, shooting stars, and the determination of geodetic positions formed the subject of his observations and study. He was among the first to engage with Professor Bache in the telegraphic determination of longitudes, several years before the use of the method by European astronomers. In later years, additional important papers upon the aurora, terrestrial magnetism and astronomy followed these early labors. But meteorology has been the science to which Professor Loomis devoted his best work, and in which he made the most important advances in human knowledge. At the beginning of his work not a single law of storms was satisfactorily demonstrated. Franklin, in the last century, had discovered their progressive motion, and Brandes and Dove (1810–1830) had announced their essential character to be that of extended whirlwinds (*wirbelstürme.*) The rival theories of Redfield and Espy, the former claiming a circular movement of the winds around a storm center, and the latter, following Brandes, claiming a radial direction, each had its warm supporters, but no decisive victory had been gained by either. Professor Loomis's first meteorological investigations beginning in 1837, were directed towards the further study of these unsettled problems of storm movement, and his last work a half century later, centered in the statistical development of all the phenomena of cyclonic systems. This example of patiently sustained scientific labor, directed in certain well-marked lines for half a century, is a rich legacy of unselfish devotion and becomes itself a part of the history of science.

In his second meteorological paper published in 1840, Professor Loomis made a study of a storm occurring near the 20th of December, 1836, adopting graphical methods very similar to those principally used by Espy, but the results were not entirely satisfactory, and he waited for another opportunity for continuing the investigation. This was found in two storms of February, 1842. Instead of using the line of minimum depression of the barometer, as before, Professor Loomis now drew maps containing lines of equal barometric pressure (or more correctly of equal departures from the normal) together with wind directions, temperatures, and weather.

The suggestion of this method of representing barometric observations was made by Brandes in 1810 in recommending that observers should give in their record books the departure from the normal of every barometric observation, and that these departures should form the principal data in the study of the compiled data. Whether Professor Loomis was acquainted with this suggestion, does not appear. This graphical method is now the essence of the modern weather map, and the memoir of 1843 in which the method was presented created a profound impression. In his appreciative biographical sketch, Professor Newton expresses his opinion that the introduction of this single method of representing and discussing the phenomena of a storm was the greatest of the services which Professor Loomis rendered to science. Professor Loomis's own estimate of the method at the time of publication was expressed as follows: "It appears to me that if the course of investigation adopted with respect to the two storms of February, 1842, was systematically pursued we should soon have some settled principles in meteorology. If we could be furnished with two meteorological charts of the United States daily for one year it would settle forever the laws of storms. No false theory could stand against such an array of testimony. Such a set of maps would be worth more than all which has hitherto been done in meteorology. A well arranged system of observations spread over the country would accomplish more in one year than observations at a few insulated posts, however accurate and complete, continued to the end of time. Is not such an enterprise worthy of the American Philosophical Society? If private zeal could be more generally enlisted the war might soon be ended, and men would cease to ridicule the idea of our being able to predict an approaching storm."

Thirty years passed before the system of observations was inaugurated and the maps constructed for which Professor Loomis here appealed. In 1871 the signal service was established, and as soon as two years' maps had been issued, Professor Loomis turned again, with unabated interest and enthusiasm, to the work that he began with such scanty material in 1840. For the remaining fifteen years of his life he devoted nearly his whole strength to this work. Twenty-three papers, entitled Contributions to Meteorology, were published in the American Journal of Science, and in 1884 he began a revision of the whole series. This revision was arranged in three chapters, covering areas of low pressure, areas of high pressure, and rain-fall. Chapters I and II were presented to the National Academy in 1885 and 1887, respectively, and chapter III was completed and issued a few weeks before the author's death. In these three monographs is compiled a wealth of statistical data which will long afford the observational basis for the explanation of climate and for the theoretical study of the atmospheric circulation and the phenomena of storms.

II.—GENERAL TREATISES; CLIMATE: WEATHER PREDICTIONS AND VERIFICATIONS.

Contributions to Meteorology, by Elias Loomis. Chapters II and III of Professor Loomis's revision of his Contributions to Meteorology have been issued during 1889.—Chapter I, previously published in Vol. III of the Memoirs of the National Academy of Sciences, treats of areas of low pressure—their form, magnitude, direction, and velocity of movement. Chapter II treats, similarly, of areas of high pressure, and of the relation of areas of high pressure to areas of low pressure. Chapter III, which completes the revision, treats of rain-fall; the conditions favorable and the conditions unfavorable to rain-fall are enumerated, and their application is shown in a survey of the more striking features of the mean annual rain-fall of different countries. Special study is made of individual rain-falls in the United States, in Europe, and over the Atlantic Ocean, and fruitful suggestions are offered towards a physical explanation of the characteristic features of individual cases. In this revision the earlier series of papers are reduced to a more systematic form, new researches and results are introduced based on later and more extended data, and the text is accompanied by a large number of elegantly printed illustrative plates. Chapter III is accompanied by a rain-fall map of the world and by rain-fall maps of India and California, which present features of special interest. The whole forms a great compendium of meteorological results derived by statistical methods and inductive processes from the modern weather map.

Meteorologia Generale, Luigi de Marchi, Milano, 1888, 160 pages.—The author has brought together in this brief compendium, in a thoroughly scientific and quite original way, the most important principles of meteorology. The book is divided into four parts. The first, entitled "Air and the Atmosphere," treats of the physical properties of the air. The second part treats of the conditions of equilibrium and of motion in the air, and is composed of three chapters: Air pressure and wind; causes of motion, laws of motion. The third part takes up the meteorological factors which depend upon the normal distribution of temperature and the periodic oscillations of temperature and their consequences. The fourth part embraces irregular meteorological phenomena and the art of weather prediction. In this section the paths of cyclones over Europe are shown on a chart, and their easterly movement is ascribed to the difference in density of the warm easterly and cold westerly winds.

Treatise on Meteorological Apparatus and Methods, by Cleveland Abbe, Annual Report of the Chief Signal Officer, 1887, Part 2.—This exhaustive work treats in five sections the subject of the measurement of temperature, pressure, the motion of the air, aqueous vapor, and precipitation. The section on thermometry opens with a chapter on the history of the thermometer, and then proceeds to discuss the theory of the air thermometer and its relation to the absolute thermo dynamic scale, the normal mercurial and its reductions to the air thermometer, and ordinary thermometers with their reductions to the normal mercurial. After the theory of the instrument, the subject of thermometer exposure is treated with corresponding fullness, and the conditions necessary for obtaining the true air temperature are clearly set forth. Chapters on miscellaneous forms of thermometers and on thermographs complete the section. The third section—that treating of the motion of the air—also commands special attention on account of the originality and depth of its treatment. The results of a large amount of research in theoretical and experimental hydrodynamics are discussed in their relation to the theory of the action of the different classes of anemometers. The last chapter of the section takes up the various methods of measuring upper currents by means of observations of clouds, and describes the different methods of using the nephoscope. The section on hygrometry gives a similarly complete exposition of the theory of the psychrometer.

A Popular Treatise on the Winds, by William Ferrel.—"It is with no ordinary degree of satisfaction that we hail the publication of Professor Ferrel's treatise. The work is a "popular" treatise, but popular only in the higher sense of the word. A system of movements so complex as those of the earth's atmosphere can not be made clear to any one who is not capable of following a chain of close reasoning, or who is not prepared to bring to the study that concentrated attention that is requisite to master any problem in deductive science.

"The most important and original portion of the book is that which deals with the general circulation of the atmosphere, in relation to which the cyclones and anticyclones that cause the vicissitudes of local weather are but matters of subordinate detail. The magnitude of the work achieved by Professor Ferrel in this field has hitherto been recognized only by the few. It is not too much to say that he has done for the theory of atmospheric circulation that which Young and Fresnel did for the theory of light; and that the influence of his work is not more generally reflected in the literature of the day must be attributed to the want of some popular exposition of the theory.

"Starting with the fundamental conditions of a great temperature difference between equatorial and polar regions and a rotating globe, and postulating in the first instance a uniform land or water surface, it is shown how the convective interchange of air set up by the former must result in producing two zones of maximum pressure in about latitude 30° in both hemispheres, two principal minima at the poles, and a minor depression on the equator, together with strong west winds in middle and high latitudes, and an excess of easterly winds in equatorial regions. The two tropical zones of high pressure determine the polar limits of the trade winds, and the whole system oscillates in latitude with the changing declination of the sun. Further, as a consequence of the fact that the great mass of the land is restricted to the northern hemisphere, whereas the southern hemisphere presents a comparatively uninterrupted sea surface on which the retarding friction is less than in the northern hemisphere, the west winds of middle and high latitudes are much stronger in the latter than in the former, and by their lateral pressure cause a slight displacement of the tropical zones of high pressure and the equatorial zone of low pressure to the north of their normal positions on a hypothetical uniform terrestrial surface.

"The great modification and extension of Hadley's theory thus introduced by Professor Ferrel depends mainly on two points of the first importance. By all previous writers it was assumed that a mass of air at rest relatively to the earth's surface on the equator, if suddenly transferred to some higher latitude—say, *e. g.*, 60°—would have a relative easterly movement in that latitude equal to the difference of rotary velocities on the equator and on the sixtieth parallel, or about 500 miles an hour, the difference being proportional to that of the cosines of the latitudes. This, however, would be true only in the case of rectilinear motion. In reality, as Professor Ferrel was the first to demonstrate, the mass of air would obey the law of the preservation of areas, like all bodies revolving under the influence of a central force, and its relative eastward velocity in latitude 60° would be 1,500 miles an hour, being as the difference of the squares of the cosines. If, on the other hand, any mass of air at rest in latitude 60° were suddenly transferred to the equator, it would have a relative westerly movement of 750 miles an hour, and any mass of matter whatever moving along a meridian is either deflected, or, if like a railway train or a river between high banks it is not free to yield to the deflecting force, presses, to the right of its path in the northern and to the left in the southern hemisphere.

"The second point first established by Professor Ferrel is that, in virtue of centrifugal force, this deflection or pressure to the right in the northern and to the left in the southern hemisphere is suffered in exactly the same degree by bodies moving due east and due west, or along a parallel of latitude, and therefore also in all intermediate azimuths.

"From the first of these principles it will be readily seen why the west winds of middle latitudes are so much stronger than the easterly winds of the equatorial zone; and from the second, how these opposite winds, by their mutual pressure, produce the tropical zones of high barometer and the polar and equatorial regions of low barometer.

"In subsequent chapters are discussed the modes in which the general circulation of the globe affects the climates of different latitudes by determining the distribution of rain-fall in wet and dry zones and inequalities of temperature through the agency of marine currents. Also the causes that modify and disturb the regularity of the ideal system, the chief of which is the mutual interaction of expanses of land and sea.

"Professor Ferrel's book covers very much of the ground of which modern meteorology usually takes cognizance, and in the thoroughness of its treatment we know of no modern work in our language that can be brought into comparison with it." (H. F. Blanford, *Nature*, XLI, p. 124.)

Bibliography of Meteorology.—The bibliography of temperature and the bibliography of moisture, parts of the general bibliography of meteorology, which has been in preparation by the Signal Office since 1884, have been made available to a limited number of meteorologists by the lithographic reproduction of a type-written copy. This great work has been compiled by Mr. C. J. Sawyer with the assistance of the present editor, Mr. O. L. Fassig, and Mr. E. H. Hilton. The card catalogues of Mr. G. J. Symons (18,000) and of Prof. Cleveland Abbe (11,000) have been employed, and many meteorologists throughout the world have co-operated by furnishing lists of meteorological books and memoirs. The classification adopted employs four general divisions, viz, general, theoretical, and applied meteorology, and observations. The first division includes (*a*) history and bibliography, (*b*) general and collected works, (*c*) organization and method, (*d*) instruments. The second division embraces (*a*) the physics of the atmosphere, including (1) temperature, (2) moisture, (3) pressure, (4 and 5) optical and electrical phenomena; (*b*) mechanics of the atmosphere, including (1) general atmospheric circulation, (2) winds, (3) storms, (*c*) cosmic relations of meteorology. The third division embraces weather predictions, agricultural and medical meteorology, and climatology.

The portions now issued are therefore, II, theoretical meteorology, (*a*) physics of the atmosphere, (1) temperature, and (2) moisture. These two parts are subdivided into fourteen and twenty-seven subdivisions respectively. The temperature volume contains 2,000 authors and 4,000 titles; the moisture volume contains 2,500 authors and 4,500 titles. The Chief Signal Officer states that no other portions are to be issued in this manner.

Climates and Weather of India, by H. F. Blanford, London, 1889, pp. 369.—The first ninety pages, constituting Part I of this book, discusses systematically the various meteorological elements—temperature, pressure, winds, humidity, cloudiness, rain, and storms—presenting in each chapter a wealth of meteorological and climatic data. The second part, entitled "Climates and weather of India in relation to health and industry," takes up the climates of the different climatic districts, the weather and weather reports, the storms of Indian seas, and the hydrography. Every chapter and almost every page is replete with interesting and suggestive information.

Instructions for observing clouds on land and seas, By Hon. R. Abercromby.—This pamphlet by Mr. Abercromby presents in a concise way the fundamental conceptions and the methods to be employed in cloud observation and is a convenient and valuable syllabus upon an important class of meteorologic observations.

Seas and skies in many latitudes, by Hon. Ralph Abercromby, London, 1888.—This is a popular, but not the less valuable, book of travels in which descriptive meteorology is the principal purpose of the author. Many important climatic facts have been garnered and the value of the work is increased by good maps and illustrations.

Das Klima des ausser-tropischen Südafrica, by Dr. Karl Dove, Göttingen, 1888.—This book describes the climate of a large region, much of which is but little known. The area is divided into four great districts classified according to the period of occurrence of the rainy season, viz (1) the region of winter rains; (2) the intermediate region of spring and autumn rains; (3) the region of heavy summer rains; (4) the west coast. Under (1) are found the southwest province, the western Karroo, and the Little Namaqua land. Under (2) come the south coast, north and south Karroo, and the southeast mountain land; and under (3) we have the table-land of the upper Orange River, the north Transvaal, the Kalahari, and the Great Namaqua and Damara land. After treating of the geography, the author discusses the possible developments of agriculture in the different districts. In a chapter on the treatment of the rain-fall and its distribution, Dr. Dove concludes with some remarks on the alleged deterioration of the climate by the drying up of the country. This effect he considers to be the outcome of reckless forest destruction. He points out the brilliant results obtained at small cost by the construction of reservoirs, as at Beaufort and at Van Wyk's Vley.

Die Meteorologie, mit besonderer Berücksichtigung geographischer Fragen dargestellt, by Dr. S. Günther, Münich. This work is an attempt to produce a text book of the whole of meteorology in 300 pages, and, so far as the effort is at all possible, the work is successful. Each subject is treated in an excessively condensed manner, and references to the literature are given for what is left unsaid. It is, therefore, rather a good index or reference book than a successor of Kämtz or Schmidt.

Der Einfluss einer Schneedecke auf Boden, Klima und Wetter, by A. Woeikof, pp. 1-115.—In this book Dr. Woeikof sums up all that is at present known of the influence of a snow covering, upon the soil, the climate, and the weather. Data are given which show that the effect of a snow sheet in lowering the temperature of the air is very considerable and certain anomalies in mean winter temperature are explained in view of this relation.

In discussing the effect of a thick winter snow-sheet on springs and rivers, a variation is pointed out which is of importance in its bearings on hydrography. In latitudes where the winter cold is sufficient to freeze the ground to a considerable depth, if heavy snow falls early in the winter before cold has penetrated deeply below the surface, the protection thereby afforded allows the ground to thaw by conduction from the lower strata, and the water from the slow melting of the basal snow layer, and much of that which is produced in the spring thaw, soaks into the soil and affords a supply which maintains the rivers more or less full through the succeeding summer. But if, before snow falls, the soil has been frozen to a great depth, a rapid thaw, setting in in the spring, floods the rivers and the surrounding tracts, while little or none enters the ground, and but little supply is stored up for maintaining the summer flow. (*Nature*, XL, p. 315.)

Greenland Exploration.—At a meeting of the Royal Geographical Society, Dr. F. Nansen told the story of his journey across Greenland in the summer and fall of 1888. He found the country so thickly covered with the ice accumulations of ages that no part of the interior is ever laid bare. This will put to rest the idea that somewhere in Greenland there may be a fertile oasis. He estimates that the ice in places is 6,000 feet deep. The temperature during the expedition reached 90° F below freezing, which was as low as their thermometer registered.

Secular Oscillations in Climate.—R. Sieger has added some new and important contributions to the statistics showing long period oscillations in the level of inland seas. That such variations in level have occurred was pointed out by Hann as early as 1867, and during the past two years has also been elaborately discussed by Dr. Brückner.

The work of Sieger relates to the level of the Armenian lakes. The material available for such an investigation consists, not of exact observations of water level, but of opportune notes of individual travellers, upon the location of places on the sea-shore, the course of the shoreline, the appearance of islands, and finally upon the oscillation of the water surface, derived from the statements of the neighboring inhabitants.

From this heterogeneous material the author endeavors to show secular oscillations in the level of Armenian lakes, especially the Wan, the Urmia Göktscha, and also a series of smaller lakes, and attributes the cause of these oscillations to oscillations in temperature and rain-fall. Similar secular variations of level are adduced for lakes in Iran, in the Alps, and in Italy, and for Lake Valencia, Honey Lake, Pyramid Lake, Winnemucca Lake, and the Great Salt Lake of Utah.

The times of maximum and minimum of level are grouped about the following well-defined periods: (1) Maximum between 1770 and 1780; (2) minimum about 1800 (not pronounced); (3) maximum about 1815; (4) minimum about 1830; (5) between 1835 and 1865 a series of lakes has two maxima, a minor about 1845 and a major about 1860; these are the Armenian lakes, Lake Constance, Lake George in Australia, Great Salt Lake, and some of their neighbors. The remaining, on the other hand, show only a maximum about 1845; (6) a minimum in the sixties, 1860–1870; (7) a rise from 1865 to 1870, followed by an interruption beginning with 1870; (8) a diminution after 1880.

In a memoir entitled "In how far is our present climate permanent?" Prof. Dr. Brückner brings together a store of data from various sources which gives evidence of well-defined oscillations in rain-fall and temperature, oscillations having a period of no definite length, but averaging during the present century from thirty to thirty-six years. The author finds similar and synchronous oscillations in the level of the Caspian, the Black, and the Baltic seas; in the rain-fall throughout Europe, and in Asia, Australia, and in the interior of North America; in the temperature of Europe and New England; in the advance and recession of the lower limit of the Alpine glaciers; in the times of vintage in France, Germany, and Switzerland; and in the opening of Russian rivers.

The rhythm of temperature oscillations corresponds with the rain-fall variations in such a way that warm periods are dry, and cool periods are wet. In the present century the maxima of rain-fall group themselves around the years 1815, 1850 and 1880, the minima around the years 1830 and 1860. The curves also show an increase in the amplitude of oscillation on advancing from the coast into the interior of the continents.

These climatic oscillations in their effect upon agriculture, are believed to be of sufficient magnitude to determine the productivity or non-productivity of vast areas in arid and semi-arid regions.

Storm Warnings.—The possibility of giving successful storm warnings for western Europe by constructing the hypothetical distribution of pressure over the Atlantic is urged by M. de Bort. He points out that from the known monthly normal distribution of pressure, together with the observed isobars of Europe and America, obtained from telegraphic reports, the general pressure over the Atlantic, and especially the existence there of pronounced storm-centers, can be inferred. (*Nature*, XXXVIII, 419.)

In the report of the Meteorological Council (of Great Britain) for the year ending March 31, 1888, Mr. R. B. Scott concludes that it is extremely improbable that telegraphic reports from America can assist in forecasting the weather in Great Britain, and this conclusion is supported by the actual results obtained in dealing with American reports during the year.

Mr. H. N. Dickson has presented a paper to the Scottish Meteorological society on "The weather lore of Scottish fishermen." Prognostications from halos, coronæ, and mock suns are of common acceptation. It is a current belief that when a sun-dog precedes the sun it is a sign of good weather, and when it follows the sun it is a sign of bad weather. Another very general belief relates to the existence of spider's webs in sails and cordage. On the north and west coast it is believed that cirrus running from northeast to southwest is a sign of good weather, and running from southeast to northwest is a sign of bad weather. Many other peculiar beliefs are related.

III.—INSTRUMENTS; METHODS OF REDUCTION; AERONAUTICS.

Gas thermometer.—In the International Bureau of Weights and Measures Dr. Chappuis has conducted a new series of observations on the difference between gas and mercurial thermometers. The latter were made by Tonnelot of hard glass whose chemical composition is specified. Four thermometers were compared having scales from —5° C. to 104°, and a length of 70^{cm}, the length of a scale-division being 5.7^{mm}; and four other mercurial thermometers 54^{cm} long for low temperatures, with scales from —32° to +39°, to which an extension is added which includes scale readings from 95° to 103°. Between these eight mercurial thermometers there were no systematic differences, and they agreed with one another very well, the greatest difference in an observation being 0°.006.

The gas thermometer is constructed so that the portion that is not brought to the temperature of the bath is extremely small. To attain this a large cylinder of iridio-platinum (1.10^{m} high and 36^{mm} diameter) is used as a thermometer box. It is a gas thermometer of constant volume. The pressure at 0° is about 1^{m} of mercury. The description of the manometer and the detail of the comparisons will be found in the *Archives des Sciences phys et nat. Bd.* xx, 1888. The following is an abstract of the differences found between a mercurial thermometer and the three gas thermometers containing hydrogen, nitrogen, and carbonic acid, whose fixed points agreed with the mercurial.

Temperature in centigrade degrees.

Hg	H—Hg	N—Hg	CO_2—Hg
—20°	0.172	0.159	....
0°	0.000	0.000	0.000
20	—0.085	—0.075	—0.042
40	0.107	0.097	0.048
60	0.090	0.085	0.037
80	—0.050	— .052	—0.019
100	0.000	0.000	0.000

Differential barometer.—Director Crnls, of the Rio Janeiro observatory, has invented a new form of differential barometer for use in hypsometry. Each arm of a U-shaped tube terminates in an air-tight box of known capacity provided with a stop-cock. If the U-be partially filled with a suitable liquid and both cocks are opened, the height of the liquid in the two arms will be the same. If now the cocks be closed and the instrument be taken to a station of different elevation and only one of the cocks be opened, the closed box will retain the air-pressure of the first and the open end will assume that of the second station. The level of the liquid will now be different in the two arms by a height which will depend on the difference of pressure at the two stations.

A light liquid like water can be employed, and yet the instrument may be kept of convenient size. If the stop-cocks are small, they may be opened alternately, each just long enough to partially equalize the difference of pressure in the boxes; with each opening the level of the ends of the liquid columns is to be read off, and the sum of the differences measures the difference of pressure. (*Am. Meteor. Journal*, vi, p. 181.)

Aspiration thermometer.—Professor Assmann has replied to the objections which have been raised against his aspiration thermometer, after giving it a thorough test. The instrument, even when exposed to the most intense solar radiation, should record the true temperature of the surrounding air. In order to determine whether the instrument satisfies this requirement, Dr. Assmann spent four weeks on the *Sentis* and found as a result of several thousand observations that this condition is fulfilled by the instrument in its present form. By means of an arrangement of clock-work, a constant current of air is drawn through the metallic tube which surrounds the thermomoter. This clock-work is attached to the upper end of the tube and drives a fan with considerable velocity, thus forcing the air out of the tube at the top and drawing it up from the lower portion of the tube; by this means a rapid current of air is kept streaming over the thermometer. By direct experiment with hot water he found that the temperature is not affected even when the temperature of the metal tube is 20° C. above that of the surrounding air. (*Nature*, XL, p. 660.)

New hygrometer.—H. Dufour has constructed a dew-point hygrometer, the peculiarity of which is that the condensing surface, instead of being a thin silver plate, is a thick silvered copper plate with the thermometer embedded in it. The thermometer thus set is supposed to register the temperature of the condensing surface more accurately than when immersed in the evaporating liquid, and that one of the largest errors incident to the dew-point hygrometer is thereby overcome; but it seems questionable whether this result is secured. (*Bull. Soc. Vaudoise*, 1888, p. 88.)

The Tenth Annual Exhibition of Instruments by the Royal Meteorological Society was held March 19–22, 1889. Special attention was given to the display of all forms of solar radiation apparatus. There were shown specimens of the actinometer of Sir John Herschel, that of Hodgkinson, Pouillet's direct pyr-heliometer, Secchi's apparatus, Balfour Stewart's actinometer, black and bright bulb thermometers in vacuo, Luvini's di-etheroscope, Bellani's lucimeter, Crooke's radiometer, sunshine recorders, and photometers.

Among new instruments exhibited were Fineman's and Galton's nephoscopes, Davis's improved air meter, Negretti and Zambra's recording hygrometer, and de Normanville's self-compensating sympiesometer.

Mr. Clayden showed a working model illustrating the generation of ocean currents. This shows how the prevalent winds over the Atlantic are the chief cause of the circulations of the waters. A number of tubes are so arranged that when an attached blower is worked the circulation of air produced resembles that of the atmosphere; the imitation winds thus set up, re-act upon the surface of the water, creating a system of currents which reproduces the main features observed in the Atlantic.

Mr. Clayden also showed some lantern slides illustrating the spiral circulation of the wind both in a cyclone and an anti-cyclone.

Mr. A. L. Rotch describes the meteorological instruments exhibited at the Paris Exposition, the special features of which are the many novelties presented in self-recording apparatus. (*Am. Meteor. Journal*, VI, pp. 293, 362.)

Professor Tait has devised an instrument, named the stephanome, for measuring the angular size of halos, parhelia, coronæ, etc. It is now used at the Ben Nevis Observatory.

Cloud Photography.—Dr. Riggenbach, of Basle, has adopted special methods for overcoming the difficulties met in photographing cirrus clouds. The blue light of the sky acts with nearly the same active energy as the white light of the clouds. Dr. Riggenbach dulls the blue light of the sky by means of the analyzer of a polarizing apparatus. The blue sky-light is partly polarized, and to the largest extent at the points which are situated 90° from the sun; the plane of polarization passing through the points looked at, the sun, and the eye of the observer. On the other hand, the light from a cloud is polarized only to a slight extent. Instead of a Nicol's prism, a dark mirror, or better, a plate of obsidian may be used; and, when conveniently situated, the surface of a lake may be used as a polarizing mirror. (*Nature*, XXXIX, p. 112.)

Thermometer Shelters.—In vol. X of *Aus dem Archiv der Deutschen Seewarte*, Dr. Köppen contributes a useful paper on the determination of air temperature. The author investigates the influence of radiation on different thermometers and screens, and gives a résumé of the experiments with regard to the latter in various countries, and of the observations on local differences of temperature (including the influence of radiation). These experiments seem to show that screens through which the air can pass freely, are better than large shelters, and that the effect of radiation is lessened by the free circulation of the air, and by the smallness of the thermometer bulbs.

Psychrometry.—Dr. Grossmann has published in the *Meteorologische Zeitschrift* an elaborate paper on the theory of the psychrometer, with an introductory sketch of the development of the subject up to the present time. The following is an outline of his treatment:

(1) If a wet bulb exposed in air (pressure B, vapor pressure p, and temperature t) reads t_1 in consequence of the evaporation of water (the latent heat being λ), and if p_1 be the pressure of saturated vapor at t_1, we may assume that, when the conditions have become steady, the process which has gone on and is going on continuously round the wet bulb is as follows: A quantity of heat is absorbed by evaporation, the vapor pressure of a layer of air containing m_1 grams of dry air being raised from p to p_1; this heat is supplied by the reduction of the temperature of a layer of air, containing m' grams of dry air, from t to t_1.

Equating these quantities of heat, we get, if S be the specific heat of dry air, S_1 the specific heat of moist air referred to unit mass of dry air, σ the specific gravity of vapor referred to air at the same temperature and pressure,

$$p=p_1-\frac{B}{\sigma}\frac{S}{\lambda}(t-t_1)\left[\frac{B-p_1}{B}\,\frac{S_1}{S}\,\frac{m}{m_1}\right]\ldots\ldots\ldots\ 1\,(a)$$

a general and strict formula for the psychrometer under the conditions. This reduces to August's formula on introducing the assumption that the whole of the air which is reduced in temperature becomes at the same time saturated by the evaporation, in other words, that $m_1=m$.

(2) The heat derived from the cooling of m' is assumed to represent all the heat derived from conduction, radiation, local convection, and the independent general motion of the air. By reckoning separately that part of the heat supplied to the bulb by radiation in time Z as equal to $ZOR\,(t-t_1)$ (O being the area of the surface of the bulb, and R the coefficient of radiation), we get

$$p=p_1-\frac{S}{\lambda}\frac{B}{\sigma}(t-t_1)\left(\frac{q'}{q_1}+\frac{R\,O}{q_1\,S_1}\right)\left[\frac{B-p_1}{B}\,\frac{S_1}{S}\right]\ldots\ldots\ (3)$$

where $$q'=\frac{m'}{Z},\ q_1=\frac{m_1}{Z}$$

In order to express the effect of the velocity of motion of the air, assume that q and q' are linear functions of the velocity v with a coefficient ε, that they are equal when v is infinite, and that their values, when v is zero, are q^0_1 and q_0' respectively.

Substituting, we get the following general formula for the psychrometer, in moving air, with spherical bulb, radius r:

$$\left.\begin{array}{l} p=p_1-\dfrac{S}{\lambda}\dfrac{B}{\sigma}(t-t_1)\left(\dfrac{B-p_1}{B}\,\dfrac{S_1}{S}\right)\dfrac{\mathfrak{A}+\mathfrak{B}v}{1+\mathfrak{B}v} \\ \text{where}\quad \mathfrak{A}=\dfrac{q_0'+\dfrac{4\,\pi\,R}{S}\,r\,\dfrac{S}{S_1}}{q^0_1};\ \mathfrak{B}=\dfrac{\varepsilon}{q^0_1} \end{array}\right\}\ldots\ldots\ldots\ (5)$$

The values of S, λ, σ, and R are known, and can be substituted ($\lambda=606.5-0.695\,t$, for water-covered bulbs, and $685.5-0.695\,t$, for ice-covered bulbs).

The values of q^0_1 and q'_0 are not known *à priori*, but they may be regarded as constant for a given velocity, so $\mathfrak{A}$ and $\mathfrak{B}$ can be determined from observations with the psychrometer upon air of known humidity moving with known velocity, and thus a numerical formula of reduction obtained. It is assumed that the radiation effect is the same in moving air as in still air.

(3) From this general equation the formulas hitherto employed can be deduced by the introduction of the special assumptions upon which they are respectively based. To obtain August's formula (corrected for radiation) K' must be put equal to D, and β equal to $\beta_{,}$. In Maxwell's formula β and $\beta_{,}$ are both infinite. Ferrel's formula for moving air is

obtained by altering the expression for radiation, so as to make it follow Dulong and Petit's law, and assuming β to be equal to $\beta_{\prime\prime}$, and each to be inversely proportional to the velocity of the air. Though the theoretical expressions for $\mathfrak{A}$ and $\mathfrak{B}$ are different, the resulting formula is identical *in form* with (5), except that the factor $\frac{B-p_{\prime}S_{\prime}}{B\,S}$ is omitted. A numerical table shows the effect and importance of the missing factor, which is moreover shown to be required by some observations of Ferrel's, from which it appears that the factor A of the temperature difference in the typical psychrometer formula, $p=p_{\prime}-AB\,(t-t_1)$, where A is supposed to have a known value in making reductions, increases more rapidly with the temperature of the wet bulb than can be accounted for by the mere variation of the latent heat.

(4.) The values of A for $t_1=0$) are also tabulated for a series of velocities; they show considerable variation for small velocities, and also greater variation with a large bulb than with a small one. On this account it is evidently necessary to provide for a constant ventilation of the psychrometer, and to determine the factor A for the specific arrangement of the psychrometer by comparison with a dew-point instrument. For small velocities the constants for each psychrometer must be determined as far as possible under the conditions that will obtain when the instrument is subsequently used for observations.

(5) The use of psychrometer tables, founded upon Regnault's formula, certainly gives on the average, for psychrometers with small bulbs, too small values for the humidity.

(6) The question as to whether different values for the latent heat of vaporization should be substituted in formulæ for water-covered bulbs and ice-covered bulbs respectively was raised by Sworykin upon his finding, in comparisons before mentioned, that the formula for water-covered bulbs gave satisfactory reductions when applied to ice-covered bulbs. It is pointed out (following Ekholm) that practical importance is to be attached to the fact that below the freezing-point the pressure of water vapor has one value over ice, and another over water at the same temperature. The latter is the greater, and should be taken in dew-point experiments, whereas Regnault's table (used by Sworykin in the comparison) gives the ice-vapor pressure. When account is taken of this, it is shown that Sworykin's observations do not entitle us to abandon the change in the value of λ for ice-covered bulbs, which is required on theoretical grounds. (*Quar. Journal Roy. Meteor. Soc.*)

Mr. S. A. Hill has made a series of comparative observations with the psychrometer and condensing hygrometer under a wide range of conditions. He discusses the difficulties and the limits of error in the use of the dew-point apparatus, which he adopts as a standard for comparing with the results given by the psychrometric formula of Regnault. His principal results are: The factor A of the psychrometer formula is approximately, if not entirely, independent of the air pressure, and also independent of the relative humidity. On an occasion of extreme dryness the direct dew-point determination gave the vapor pressure 5.2^{mm}, while Regnault's formula gave 3.9^{mm}, a value considerably too small.[*]

Anemometry.—Prof. C. F. Marvin, in presenting the results of the recent experiments of the U. S. Signal Office for determining anemometer factors, points out that an important consideration has hitherto been overlooked in applying the factors obtained in whirling-machine experiments to anemometers in actual use. In the experiments the velocity is uniform, in practice the wind is extremely variable. The revolving parts of an anemometer exposed to a variable wind will, in consequence of their inertia, fall short of their proper rate of revolution when the velocity of the wind is increasing, and then catch up and run too fast when the wind velocity diminishes, and it has been generally assumed that the amount lost in the one case is balanced by that gained in the other, so that the mean rate is not affected. But such is not the case, from the fact that the resultant force acting on the cups when the wind is increasing and tending to increase the velocity of the cups is much greater than the corresponding force in action when the wind is diminishing in intensity and tending to retard the velocity of the cups. In one case it is a question of the resistance of the air to the concave side of the cups, and in the other that to the convex surface; the latter being smaller, the cups will always continue to move more rapidly and longer after the wind has diminished in velocity, than they lagged behind when the wind was increasing, so that the mean velocity of the cups exposed to a variable wind is appreciably higher than it would be in a uniform wind having the same mean velocity. Consequently, if the equation of an anemometer whose constants have been determined upon a whirling machine be used to reduce ordinary observations, the computed wind velocities will be too high by an amount depending upon the moment of inertia of the cups and the extent of the wind variations. Since these latter are too complicated to be determined, it is impossible with anemometers of the Robinson type to obtain accurate measures of the mean velocity of a variable wind unless the moment of inertia of the cups is very small in relation to the wind pressures, and even then the result is still affected by another error.

For accurate results a formula involving the square as well as the first power of the velocity of the cups must be used, as in the form $V=a+bv+cv^2$ where V=velocity of wind and v=mean velocity of the cup centers. Now, when the wind is variable the third term should not be cv^2 but $\frac{c}{n}(v_1^2+v_2^2+v_3^2+v_4^2+ \quad . \quad . \quad . \quad . \quad v_n^2)$; even in ordinary circumstances this difference between the square of the mean velocity and the mean of the squares is appreciable.

The constants for a small anemometer of extreme lightness were determined with great precision on a whirling machine and comparisons in open air made between it and a Signal Service anemometer of the ordinary pattern whose constants had also been determined. These comparisons have shown that the wind velocities computed from the ordinary anemometer average 10 per cent. higher than the wind velocities given by the little anemometer having very small inertia, and that the percentage of excess increased with decrease of wind velocity. An anemometer was then tried whose cups were weighted, and still greater percentages of excess were discovered. (*Am. Meteor. Journal*, VI, p. 115.)

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[*] Ferrel's tables would have made the vapor pressure 6.0^{mm}, which is a fair approximation to the value given by the direct hygrometer.—G. E. C.

The wind force committee of the Royal Meteorological Society, consisting of Mr. G. M. Whipple and Mr. W. H. Dines, has made a report on experiments with anemometers at Hersham. A whirling apparatus of 29 feet radius was rotated by means of a small steam-engine. On the arms of the whirler four different anemometers were placed; each experiment lasted fifteen minutes, the steam-pressure remaining constant during the run. For the new standard anemometer with arms 2 feet long the experiments gave a mean value for Robinson's factor of 2.15, and for two smaller instruments the factor is 2.51 and 2.96. Mr. Dines' helicoid anemometer gave very satisfactory results, the mean factor being 0.996. (*Nature*, XXXVIII, p. 191.)

Air pressure against moving plates.—The results of Recknagel's experiments on the resistance of the air to the motion of plates on a whirling-machine agree in general with those of other experimenters. He finds that the total resistance is greater the smaller the distance of the plate from the axis of rotation. The total resistance R which a circular plate of diameter D experiences when rotated at a distance L from the axis of rotation is from his experiments—

$$R = H \cdot \frac{\pi D^2}{4}\left(1.12 + \frac{3.21\,D - 0.632\,D^2}{L}\right),$$

in which H represents the pressure due to the velocity, or $\frac{\rho\, v^2}{2\, g}$. A formula is given for the distribution of pressure over the front of the plate, and the diminution of pressure behind the plate is shown to depend largely on its diameter. (*Meteorologische Zeitschrift*, 1889, VI., p. 3.)

Mr. W. H. Dines has conducted a series of whirling-machine experiments to determine the pressure on various shaped objects placed at the end of the arm and whirling with different velocities. The machine was operated by steam-power. The pressure upon a plane plate three-eighths inch thick, and either round or square, was found to be 1.51 pounds per square foot for a velocity of 20.86 miles per hour. The pressure upon the same area is increased by increasing the perimeter. The pressure upon a one quarter-foot plate the author states to be proportionately less than upon a plate either one-half or double its size, and the pressure upon any surface was found to be but slightly altered by a cone projecting at the back. (*Quar. Jour. Roy. Met. Soc.*, XV, p. 187.)

Measurement of rain-fall.—Prof. Cleveland Abbe has discussed at length the sources of error in the collection and measurement of rain-fall. From a study of published observations of rain-fall measures at different heights above ground he finds that the deficit in collection equals 6 per cent. multiplied by the square root of the altitude in meters. It is further shown that the error of collection may be eliminated by observing two gauges at different altitudes and applying a formula of reduction. (*Am. Meteor. Journal*, VI, p. 241.)

Errors in thermometer readings.—Messrs. W. A. Rogers and R. S. Woodward, in a paper on errors in reading mercurial thermometers, discussed an instrumental error hitherto generally overlooked. The theoretical study of Professor Woodward led to a dynamical equation expressing the motion of the end of the column in terms of its mass, the volume and elasticity of the bulb, and the frictional and other forces, including surface tension. When the latter forces are constant, and when the rate of temperature rise is constant, the formula shows: (1) That the motion of the end of the column is by pulsations of regular recurrence, the reading of the thermometer being alternately too great and too small; (2) that the period of recurrence in the same thermometer varies directly as the square root of the rate of temperature rise; (3) that the amplitude is sensibly constant; (4) that, since the amplitude is nearly constant and the period thus dependent on the rate of the temperature rise, the danger of error in thermometer readings is greater for slow than for rapid temperature changes. (*Report Am. Assoc. A. S.*, 1889.)

Method of obtaining daily mean temperatures.—Dr. W. Köppen has sought for the most accurate method of deriving the true daily mean temperature from observations made at 8 A. M., 2 and 8 P. M., and the minimum temperature. The author assumes, as recommended by the Vienna Congress, that the true mean temperature m is given by the formula $m=n-k\ (n-min)$ where n is the arithmetical mean of the three observed values, and k a constant to be determined. The author has determined the value of k for different seasons at different places where hourly temperatures have been recorded, and finds that in spring and summer the value is almost the same at all stations. In a similar way the author investigates the accuracy of the combination of the 8 A. M., 2 P. M., and 8 P. M. without the minimum temperature. (*Meteorologische Zeitschrift*, 1889, VI, p. [1].)

Reduction of air pressure to high levels.—As a supplement to his paper on the construction of isobars for the level of 2,500 meters (*Meteorologische Zeitschrift*, December, 1888), Dr. Köppen gives the following table of constants for obtaining the mean temperature of the air column, or the temperature at 1,250 meters, by subtracting them from the sea-level temperatures:

Barometer.		Below 760^{mm}.	760–765^{mm}.	Above 765^{mm}.
Weak, easterly winds, northeast to south.	Temp. below 0° C	4°	1°	−2°
	Temp. 0° to 15° C	5°	3°	0°
	Temp. above 15° C	6°	5°	2°
Neutral condition		7°	6°	4°
Strong westerly winds, southwest to north.		9°	8°	7°

(*Meteorologische Zeitschrift*, 1889, VI, p. 348.)

Balloon observations.—Lieutenant Mödebeck has made a contribution to the meteorology of the upper currents by observations made in a balloon voyage March 31, 1888, from Berlin. The marked phenomenon experienced was the influence of rivers. After the balloon had risen to a height of 300 to 500 meters, it sank so rapidly while passing over the Spree that when it was about 50 meters above the earth a large quantity of ballast had to be thrown out. At an elevation of 1,200 meters he met with a long narrow rain-cloud, in passing through which the dry-bulb thermometer registered 1°.5 C., the wet bulb 1° C.; at an elevation of 1,300 to 1,400 meters both thermometers registered the same temperature, 2°.5 C. At this height, and in circumscribed areas, a few very small semi-soft hailstones were observed.

Soaring of birds.—Mr. G. K. Gilbert, in a paper read before the Philosophical Society of Washington, shows that the soaring of birds is rendered possible by the differential motions of the air. The following paragraphs are extracted from his paper:

"The soaring bird, with wings expanded, is formed so as to move forward with little friction and downward with great friction. We may conceive him as having two coincident motions—a forward motion, initiated by muscular action; and a downward motion, under the pull of gravity. In order that the resultant may be horizontal, it is necessary (1) that the forward component be directed obliquely upward, and (2) that it exceed a certain minimum amount.

"However small may be the friction created by the forward motion it is not *nil*, and, unless the energy it consumes is in some way replaced, the forward motion is eventually so reduced that the horizontal motion cannot be maintained. It is proposed to show that the needed compensatory energy may be derived from the differential motions of the air.

"Let us assume that the air currents above and below a certain horizontal plane have the same direction but different velocities, the upper moving the faster by a certain amount, i. A soaring bird is moving through the lower air in the opposite direction, and the bird's velocity with reference to the air is V, then the velocity of the bird with reference to the upper current is $V+i$.

"Now let the bird change his course, turning obliquely upward and passing into the upper current. His velocity with reference to the air in which he is immersed is at once increased from V to $V+i$. Next let the bird wheel, to the right or to the left, until the direction of his motion is coincident with that of the wind. His velocity with reference to the upper current is still $V + i$, but the reversal of his direction has changed his relation to the currents. He is passing the lower and slower current more rapidly than he passes the upper, and his velocity with reference to the lower current is greater by their difference; it is $V + 2i$. Now let him descend obliquely and enter the lower current. His velocity is not affected by the transfer. Finally let him wheel in the lower current until his direction is once more directly opposed to that of the wind. The cycle of evolutions leaves him with the velocity $V + 2i$, referred to the lower current, in the place of his initial velocity V, referred to the same datum. He has gained a velocity of $2i$, or double the velocity of one air current referred to the other, and he has resumed his original relation to the currents. Manifestly he can repeat the process indefinitely.

"Add now that velocity thus gained is the required compensation for the velocity lost by friction, and the essence of the theory is stated."

When the orbit of the bird is circular and lies in an inclined plane rising toward the wind, and when the horizontal velocity of the air diminishes uniformly from the highest point to the lowest point of the orbit, the velocity gained by the bird in making the circuit is equal to the differential velocity of the highest and lowest layers of air traversed, multiplied by $\frac{\pi}{2}$ into the cosine of the angle of inclination of the plane of the orbit.

IV.—PHYSICAL PROPERTIES OF THE ATMOSPHERE AND THE OCEAN.

Height of the atmosphere.—M. Soret criticises the assumption made by Liais in his method for determining the height of the atmosphere. Liais found that the light of the sky is still polarized in a plane passing through the sun when the sun is 18° below the horizon, and concluded from this that the sun must have directly illuminated the air particles in the zenith of the observer. Soret shows that by diffusion of the second order a mass of air in the shade may be illuminated by the particles in the sunlight. Mathematical analysis shows that in this case also, the light diffused by the mass in shade is polarized in the plane determined by the point considered and the eye of the observer. Hence the results of Liais are subject to the error of his assumption. (*Comptes Rendus*, CII, p. 103.)

Specific volume of aqueous vapor.—Dr. Dieterici has presented to the Physical Society of Berlin the results of his researches on the determination of the specific volume of saturated vapor at 0° C. His new method is to measure the amount of water that must be converted into vapor at 0° C. in order to fill completely a known space with saturated vapor by means of the heat which becomes latent during its evaporation. The vessel containing the water was immersed in an ice calorimeter and was connected with a large space, which could be rendered both vacuous and dry. The water was then allowed to evaporate until the space was filled with saturated vapor; the amount of heat requisite to produce the observed evaporation was determined from the amount of mercury which was expelled from the calorimeter, and this then gave the amount of water evaporated. One outcome of the experiments is that Gay-Lussac's law holds good almost up to the temperature of saturation. The mass of water that must be evaporated in order to saturate a space of 1 liter capacity at 0° C. was found to be 4.886 milligrammes; hence the specific volume of aqueous vapor saturated at 0° C. is 204.7 liters, and its pressure is 4.62 millimeters.

Spectrum of ozone.—Mr. W. N. Hartley has examined the absorption spectrum of ozone by photographing the ultra-violet rays transmitted through measured quantities of the gas, and finds that it possesses most extraordinary absorptive powers. Cornu's experimental proof that the ultra-violet rays of the sun are absorbed with energy by the atmosphere is attributed by Hartley to the ozone which is a constituent of the atmosphere, and which he states is in greater proportion in the upper regions than near the earth's surface.

The explanation that the blue color of the sky is caused by reflection from minute particles which on account of their size most readily reflect blue rays is rejected as incompetent, and a theory suggested by his own experiments is presented. He announces that ozonized oxygen is highly fluorescent, and that the color of the fluorescence is a beautiful steel blue. Oxygen also is believed to be fluorescent, though this has not been proved.

He concludes (1) that the extreme limit of the solar spectrum observed by Cornu is caused by the gases in the atmosphere, probably both by oxygen and ozone; (2) that the blue of the sky is caused in part by fluorescence, and probably ozone and oxygen are the chief fluorescent substances; (3) that ozone is generally present in the air in sufficient quantity to render its characteristic absorption spectra visible, and that therefore it gives a blue color to the atmosphere by absorption. (*Nature*, XXXIX, p. 475.)

Specific heat of sea water.—Messrs. Thoulet and Chevalier have presented to the Paris Academy of Sciences the results of a series of measurements on the specific heat of sea water at different degrees of dilution and concentration. The results are applied to explaining the enormous influence exercised by the sea in modifying climates.

The law of thermal radiation.—H. F. Weber derives for the total radiation of a body the formula $S=CFT^{aT}_{e}$ where C is a constant depending on the nature of the substance and the properties of its surface, and a is a constant for all solids. F is the radiating surface and

T the absolute temperature. He believes that this formula will hold good for all temperatures between the melting point of ice up to the melting point of platinum, while Stefan's law gives results too high for low temperature and much too low for high temperatures.

Herr Graetz (*Wiedemann's Annalen*, 1889, p. 857) criticises Weber's claims, and maintains that Stefan's formula has a much better basis IV, than this new formula. (*Meteorologische Zeitschrift*, 1889, VI, p. 38.)

Laws of thermal radiation.—In an article on the law of the thermal radiation, Professor Ferrel has compared with observational data, both Dulong and Petit's law and Stefan's law of radiation. He finds that Dulong and Petit's law holds through only a comparatively short range of temperature, and the same is true of any function of the same general form; but by giving different values to the constant in the formula (taken by Dulong and Petit as 1.0077), increasing with lower temperatures, the rate of cooling may be represented through a considerable range of temperature. For temperatures from 50° C. to 137° C. Stefan's law agrees with the observed rates of cooling, but for higher temperatures an exponent of 4.2 is required instead of 4.

Professor Ferrel then shows that the law of radiation for the resultant radiation of all wave lengths must differ very much in different bodies in which the radiativities differ considerably from that of a surface of maximum radiativity, according as the predominating wave lengths in the radiations are toward the one or the other end of the spectrum. The application of the various radiation formulæ in obtaining the temperature of the sun is shown to give no reliable results. (*American Journal of Science*, XXXVIII, p. 3.)

V.—SOLAR RADIATION AND ATMOSPHERIC ABSORPTION; TEMPERATURE.

Solar radiation.—During the summer of 1888 a series of observations of solar radiation were made on Mont Ventoux (altitude 1,907 meters) with a continuously recording photographic Crova actinometer. The following is a brief statement of results:

Continual oscillations are apparent in the solar curve; but with a smaller amplitude than at Montpellier.

A regular midday depression of the curve takes place to the same degree as at Montpellier. This is due to the vertical ascent of vapor, and not to the influence of the sea.

The solar constant, computed from the observations at Montpellier, is nearly 3 calories, and it is believed that the registering apparatus and the method when used at greater heights will give a value greater than 3 calories.

The polarization of the sky-light appears as a rule to be greater the greater the solar constant and the smaller the diathermancy. Consequently the degree of polarization may furnish useful indications for judging the relative diathermancy. (*Meteorologische Zeitschrift*, 1889, VI, p. 62.)

R. Ssawelief has made a series of solar radiation observations at Kiew, extending through 1888. The instrument used was a Crova actinometer. The yearly range of the intensity of solar radiation at Kiew agrees with that at Montpellier. The principal maximum, 1.39 calories, occurred on May 8; the principal minimum, 1.13 calories, at the winter solstice (on account of the small altitude of the sun). Since the values of the radiation are very nearly the same at Montpellier and at Kiew, though the latter is 7 degrees farther north, it follows that in Russia the air has a materially greater diathermancy. The variations of the diathermancy at Kiew are smaller than at Montpellier.

The value of the solar constant is computed from a series of forty-two measures, made on January 7, and found to be 2.86 calories. In obtaining this result, the observations at midday were excluded because they showed a smaller radiation than both before and after noon, resulting from a diminished diathermancy. This large solar constant, which approaches very nearly to Langley's values, shows again the extraordinary purity of the air in Russia on fine winter days. (*Comptes Rendus*, CVIII, p. 287.)

Actinometric observations made in 1888 at the Montpellier Observatory are reported by M. A. Crova as confirming the results established by previous observations, namely, that a primary maximum of intensity always occurs in spring and a secondary maximum in autumn. (*Nature*, XXXIX, p. 504.)

Dr. J. M. Pernter has discussed the solar radiation observations made by Lephay at Cape Horn with a Pouillet pyr-heliometer, and despite the fact that such a crude instrument was used, some valuable results are derived.

A selection of the best observations, tabulated according to the zenith distance of the sun, shows that the diathermancy of the atmosphere increases materially with the zenith distance of the sun. The cause of this is attributed to the fact that rising air currents are most frequent in summer and in the middle of the day, and that aqueous vapor carried to great heights by these convective currents is condensed in the upper air strata even on clear days when the amount of condensation is not sufficient to be visible. The average summer value of the midday diathermancy is 0.55; the average winter value is 0.80. (*Meteorologische Zeitschrift*, 1889, VI, p. 130.)

Atmospheric absorption.—Dr. Ångström has communicated to the Royal Academy of Sciences at Stockholm a contribution on the absorption of radiant heat through the various components of the atmosphere.

Distribution of heat over the earth's surface.—Dr. Zenker has made an elaborate research on the distribution of heat over the earth's surface, taking account not only of the radiation from the sun and absorption of heat by the atmosphere, but also of the effect of the distribution of land and water over the earth's surface. In previous researches on the distribution of heat, the mean values have been determined and based upon empirical observations; but Dr. Zenker has calculated the distribution of heat over the surface of the sea with the help of Hann's isothermal charts, starting with the temperature of a point on its surface which was quite uninfluenced by the neighboring continents, and was consequently equally unaffected by any warm or cold current. Using this factor, and the formulæ deduced in the theoretical part of his paper, he has calculated the distribution of heat from the pole to the equator for each successive parallel, and compared it with the distribution of solar radiation. As a basis for the distribution of heat over the surface of the land, it was first necessary to determine the condition under which the influence of the neighboring sea is either nothing or minimal in amount. The starting point for this was the fact that the temperatures on the continents exhibit very great variations, and from these was determined for each area, as a percentage, the relative influences of the sea and continent upon its temperature. The region where the influence of the sea was proved to be *nil* (or where the "continentality" was 100 per cent.) was in the neighborhood of the east coast of Asia, whereas all other points were found to be affected by the neighboring sea to a greater extent. The observed temperature on the land was therefore only partly dependent upon the position of the place on any given parallel, other influences making themselves more or less felt. Hence it was possible to calculate for each parallel the real and "accessory" temperature. The amount of heat radiated from the sun was compared with these temperatures, and was found to be about the same for each 10° C. of difference in temperature; from 0° to 10° C. however, quite considerable differences in radiation were necessary. In conclusion, Dr. Zenker compared the temperatures which really exist on the earth's surface with those which he had deduced, and found that in reality the climate on the sea in the southern hemisphere is colder than it should be according to calculations—a result which must be attributed to ocean currents of cold water. The continental climate in the northern hemisphere is slightly too warm, in consequence of the effect of the Gulf Stream. (*Nature*, XXXVIII, p. 48.)

Terrestrial temperature.—Mr. Arthur Searle in a paper entitled "Atmospheric economy of solar radiation," discusses the manner in which the assumed protective action of the atmosphere maintains terrestial temperatures. Since the supposed effect of selective absorption whereby the atmosphere was supposed to be more diathermanous to solar than to terrestrial radiation, has been largely disproved by Langley's experiments, the author points out that heat transferred from the earth to the air by conduction and convection is not radiated into space with the same facility as it would be if radiated directly from the earth's surface. The increase of energy thus accumulated in the atmosphere is checked by the development of atmospheric movements. Warm air is transferred to cold regions where it is cooled by conduction of its heat to the cold surface.

In comments on this paper Professor Ferrel accepts the fact of the heating of the air by conduction and by convection as outlined by Mr. Searle, but thinks that this circumstance can not have much effect in modifying terrestrial temperature. "The whole amount of heat conveyed away from the earth's surface in the form of latent heat, is perhaps one-tenth of that received from the sun and absorbed, and so radiated again into space. If this one-tenth part only has a little less facility for escaping into space than it would have if radiated directly from the earth's surface, the protective effect can not be very much." In fact the primary assumption that the earth's mean temperature would be much lower than it is, if it were not protected in some way by the atmosphere, is by no means certain. (*Am. Meteor. Journal*, VI, p. 173.)

Decrease of temperature with height.—Dr. Hann, in a discussion of the results of observations on the Sonnblick, derives some important formulæ expressing the decrease of temperature with height. For yearly means he finds that the temperature at any elevation is represented by the formula $T = 8^\circ.0 - 0^\circ.482h' - 0^\circ.0018h'^2$, in which h' is the height of the station above Salzburg (450 meters) expressed in hectometers. This equation shows that the true mean temperature of the air column between the summit and base is 0°.8 higher than the arithmetical mean of the temperature at the summit and base—a result of great importance in practical hypsometry. For the different seasons the formulæ for decrease of temperature are as follows:

Winter : $T = -2^\circ.1 - 0.136h' - 0.0111h'^2$
Spring : $T = 7.1 - 0.537h' - 0.0011h'^2$
Summer: $T = 18.0 - 0.637h'$
Autumn: $T = 9.7 - 0.433h' - 0.0031h'^2$

(*Meteorologische Zeitschrift*, 1889, VI, p. 33.)

Mr. S. A. Hill has conducted an extended series of observations of temperature and humidity at different heights above ground at Allahabad, and the results are published in Vol. IV, Part VI, Indian Meteorological Memoirs (see Bibliography). From observations at heights of 4, 46, 104, and 166 feet, the daily range of temperature was found to diminish with altitude at a rate represented by the formula

$$\log. r = 1.324 - 0.118h + 0.023h^2$$

in which h is given in hundreds of feet. The formula can be extended scarcely 100 feet beyond the observed values. Normal hourly temperatures for each month of the year are given for each of the four elevations, and from these the temperatures for altitudes of every 20 feet up to 200 feet are given for 6 A. M., 2 P. M., and 10 P. M. for each month and the year. The results of the observations are also graphically represented by contours giving for every 5° the variations of temperature in the diurnal period at different heights. Thirteen such sets of isothermal contours give the means for each month and the year. The observations of vapor pressure show a minimum from 3 to 4 P. M., followed by a rapid increase; in the months from November to February a maximum is reached at 7 P. M., while from March to May the maximum occurs at about 7 A. M.; but the curve from 8 P. M. to 7 A. M. is high and has but little rise. From June to October there are two maxima, one at 8 A. M., the other at 8 P. M.

Periodicity of temperature.—In the treatment of the mean daily temperature curves at polar stations, Prof. H. Fritz has perceived a regular sequence in the maxima that seems to him to be not purely accidental. By placing together the maxima at Jan-Mayen, Godthaab, Fort Rae, Uglamie, Vivi on the Congo, and Zurich, he has derived a 13.84-day period, or better, the half of a 27.687-day period. This agrees almost exactly with the period of sunspots and auroras (see Fritz; *Das Polarlicht*, p. 206), as well as with the period found by Buys Ballot from the temperatures at Zwanenburg, Harlem, and Danzig.

For further proof of the reality of this period, the observations of Kane in Smith's Sound, of the second German North Polar expedition to Sabine Island, and of other arctic observers are reduced to the same period and compared with the maximum of sunspots. These data also show an agreement.

Temperature anomaly.—Dr. Hellmann calls attention to the long-continued temperature anomaly which has prevailed over Germany and other neighboring countries of western Europe from the beginning of 1885 to the present time (April, 1889). By means of graphic charts of monthly temperature he shows that the temperature in western Germany has for the most part lagged behind the normal. In Löningen, of the fifty-one months from January, 1885, to March, 1889, 71 per cent. have been too cold. Previous cold periods have occurred in the years 1835–'38 and 1784–'87. (*Meteorologische Zeitschrift*, VI, p. 275.)

Secular variation of temperature.—Mr. William Ellis has tabulated and discussed the temperature observations made in England from 1849 to 1888 with the following results:

Five-year periods.	Departure of the mean temperature of five-year periods from the forty-year average at stations between latitudes—			
	51° and 52°.	52° and 53°.	53° and 54°.	At Greenwich.
1849–1853	−0.29	−0.15	−0.19	+0.08
1854–1858	+0.01	+0.11	+0.40	−0.04
1859–1863	+0.55	+0.43	+0.05	+0 32
1864–1868	+0.60	+1.03	+0.55	+0.69
1869–1873	+ .27	+0.39	+0.22	+0.22
1874–1878	+0.50	+0.53	+0.29	+0.38
1879–1883	−0.88	−1.08	−0.68	−0.83
1884–1888	−0.74	−1.23	−0.63	−0.79
Mean temperature	49.02	48.24	47.50	49.49

(*Quar. Journ. Roy. Met. Soc.*)

VI.—ATMOSPHERIC MOISTURE; CONDENSATION, FOGS, HAZE, AND CLOUDS; RAIN, SNOW, HAIL, AND FLOOD.

Amount of water in cloud.—Dr. J. Hann has put together the observations on the amount of moisture contained in a given volume of cloud at different temperatures. In the experiments made by A. and H. Schlaginweit in 1851, the amount of moisture in the water particles in a cloud was in every case less than the uncondensed vapor in the same volume. (*Meteorologische Zeitschrift*, VI, p. 304.)

London fog.—In an address on the relation of smoke to fogs in London, Mr. F. A. R. Russell shows that the characteristic London fogs are produced by the mechanical combination of particles of water with particles of coal or soot. The conditions of their development are a still air, lower temperature near the ground than at a height of some hundreds of feet, high relative humidity, a cloudless sky, and free radiation into space.

The darkness and peculiar coloring occur with greatest effect when a very large quantity of coal is being burnt in domestic fires, hence 8 to 10 A. M. of the winter season is the time of thickest and darkest fogs. The fogs are formed by the mixture of soot and smoke with an already existing white fog. A thick layer of these carbonaceous particles prevents the sunshine from reaching and evaporating the particles of natural fog. Thus the fog is blackened and its dissipation is retarded. The author estimates that the loss from all sources due to this wasteful method of burning coal is for London alone about £5,000,000 a year. (*Nature*, XXXIX, p. 34.)

Haze.—Mr. Russell has made an elaborate analysis of the causes and character of haze, of which the following is a partial summary:

Unlike fog, haze commonly occurs when the lower air is in a state of unusual dryness. Haze does not prevail on the Continent of Europe or in the interior of North America to anything like the same extent as in England. On the east coast of Scotland, and over all north Britain, it is exceedingly common, especially in the spring, and during the prevalence of east wind. The conditions favorable for the production of haze are: (1) A gentle wind from east-southeast to northeast, inclusive, and east wind in general, especially with dry weather in spring and summer. If the east wind be established up to a great height, the lower air is usually clear, but if the upper current is from a westerly direction, haze prevails. (2) Fine settled weather, with variable currents, a dry air, and little dew. (3) Opposition of currents—such as occurs when several shallow barometric depressions exist over the country—and the atmospheric state preceding thunder-storms. (4) Damp weather, with light winds and varying temperature.

The very condition to which haze in England is commonly, and in a certain sense correctly, attributed—namely, atmospheric humidity—is, if sufficiently uniform and extended, least favorable to its manifestation. A constant moisture-laden westerly breeze would give a climate nearly as clear as that of the southwest corner of France.

Two principal factors go to the production of ordinary haze; the first, a rather large amount of vapor between the earth and a great altitude, say 60,000 feet; and the second, a mixture of two heterogeneous masses of air. Evidence of the correctness of this proposition is to be found in the geographical distribution of haze and the state of the winds when it occurs.

In the majority of cases of east wind, and especially when this wind is of brief duration, local or gentle, a westerly wind flows above it at no great distance from the surface of the earth. Considering the perpetual rapid interchanges (hardly to be called diffusion) going on in the atmosphere, the lower wind must be largely mixed with air of a different condition derived from the westerly current. If a cold, dry east wind be permeated by patches and filaments, however minute, of moister and warmer air, they must be cooled by contact with the polar wind, and a slight deposition of vapor may take place. Or the countless invisible dust particles may, by increased radiation towards space through a drier air, either cause a deposition of moisture upon themselves or collect still smaller particles together, as dust is known to collect on cold surfaces in a warm air. If deposition of moisture take place, the dryness of the air prevents the water particles from growing to anything like the size of the particles of a fog; a relatively small diffused quantity of vaporous air in minute parcels could not produce by condensation any but extremely small and transitory water particles, in the aggregate visible through long distances, but probably individually beyond the power of the miscroscope to discern. They may be compared to the blue mist escaping from the safty-valve of a boiler under high pressure, the invisible steam turns for a moment blue and then to the ordinary white of visible steam. The haze may possibly be equally momentary in duration, disolving long before reaching the white stage, but fresh filaments are perpetually keeping up the process and giving the appearance of a persistence like that of smoke or dust.

The evidence concerning the appearance of haze by irregular transmission of light due to unequally heated currents of transparent air seems to be quite insufficient, and however great the heat near the surface of the ground, say in the desert, with consequent distortion of images, it does not, as a rule, bring about the haze so common in temperate climates. (*Nature*, XLI, p. 60.)

In a communication in *Nature*, Mr. J. H. Poynting suggests that common summer haze may be due to local convection currents, which by reason of their difference of temperature and density render the air optically heterogeneous. The light received from any object is more or less irregularly refracted, and on account of the motion of the currents its path is continually varying. The outline of the object has a tremulous motion, and so becomes ill defined. At the same time reflection occurs where there is refraction at the surfaces of separation of heterogeneous portions, and the reflected light is diffused as a general glare. The combination of the quivering of outline and the loss of direct light, as a diffused glare, may possibly give the appearance called haze which is seen in the middle of a hot, cloudless, summer day. The author mentions other cases of haze which may possibly be likewise due to optical heterogeneity. (*Nature*, XXXIX, p. 323.)

In a series of letters to *Nature* by a number of prominent scientists, a valuable contribution has been made to our knowledge of the peculiar characteristics of different types of haze. Professor Tyndall opened the subject by reporting his observations of the prevalence in the valleys of the Alps of a fine haze appearing as long horizontal striæ. Amid the haze were often patches of cloud which disappeared under the sun's rays, leaving the permanent haze behind. From this fact he is certain the haze is not aqueous, and suggests that it may be due to autumn pollen in the air.

Mr. Johnston-Lavis corroborates the observation that haze assumes the form of horizontal strata, and supports the theory of its micro-organic nature. During the hottest and driest weather of summer, haze similar to that observed by Professor Tyndall in the Alps can be seen in the Gulf of Naples and other parts of the Mediterranean coasts at an average altitude of 1,500 feet and rarely reaching 2,000 feet.

M. d'Abbadie furnishes a valuable list of special names by which such haze is designated in many warm countries, where it is most frequently observed. In Ethiopia, where it is called *qobar*, this haze is of extraordinary density and hides all the features of the landscape beyond the distance of a mile, and conceals stars of the third magnitude even in the zenith. Observations of its occurrence are quoted from Peru, Hayti, Switzerland, Spain, and other localities; its color is a light buff, and when dense, a "lurid gray, verging to blackness."

W. Clement Ley reports that the horizontal layers of haze may be frequently seen throughout the British Isles at times when the atmosphere at the earth's surface is nearly calm and moderately dry. He has given it the specific name of dust-haze, and distinguishes it from the ordinary water haze by its color; dust-haze appears of a reddish buff tint, while water haze usually appears gray or blue in reflected light, and yellow, orange, or red in transmitted light.

Classification of clouds.—Prof. H. H. Hildebransson has submitted to the Meteorological Congress at Paris (September, 1889) a report on the classification of clouds, adopted by Mr. Abercromby and himself, and urges its general adoption. This classification distinguishes ten forms, as follows:

1. Cirrus. 2. Cirro-stratus, a thin cloud veil composed of thickly compacted cirrus fibres, indicative of rain. 3. Cirro-cumulus, small

globular cloudlets, commonly called mackerel sky. 4. Cumulo-cirrus or alto-cumulus, a form intermediate between cirro-cumulus and strato-cumulus, of a large globular form like white wool packs. 5. Strato-cirrus or alto-stratus, a thick gray or bluish layer of cloud at an average elevation of 17,000 feet. 6. Strato cumulus, large rounded masses of gray cloud, sometimes called roll-cumulus. 7. Nimbus. 8. Cumulus. 9. Cumulo-nimbus, the thunder cloud. 10. Stratus, an elevated sheet of mist or fog.

In order that the exact forms of cloud to which the names apply may be learned and the names be properly used, the author calls attention to the photographs in his *Classification des nuages*, and also recommends the new cloud-atlas, published by Gustav Seitz Nachfolger, of Hamburg.

Results of rain, river, and evaporation observations made in New South Wales during 1888, by H. C. Russell.—This volume contains a most valuable collection of meteorological and hydrographic data, tabulated, charted, and discussed. The rain-fall from eight hundred and seventy stations is given for each month and the year, together with the greatest daily rain-fall in each month, the mean annual, and the number of years of observation. The mean annual precipitation ranges in different districts from 10 to 68 inches. The year 1888 is the driest upon record, and in striking contrast with 1887, the wettest on record. The rain-fall for the year is charted on a large scale map by red circles at each station of observation. The monthly distribution of rain is shown for each square degree by twelve blocks proportional in length to the monthly amount. A third diagram shows the stage of the rivers above mean summer level. Tables of average daily evaporation for each month are given for nine stations. These show a range of 36 to 65 inches in the total evaporation for the year. Comparative observations on the amount of evaporation from water, grass, and earth surface show that when the soil is saturated the evaporation proceeds at a rate greater than from a water surface. The value of these observations would have been increased if surface temperatures had been taken.

For the Murray River the annual discharge ranges from 20 to 40 per cent. of the rain-fall over its catchment area, while for the Darling the discharge is in general less than 3 per cent. of its rain-fall.

Rain-fall of India.—Parts III and IV, vol. III, Indian Meteorological Memoirs, published in 1888, bring to a close Blanford's great monograph on the rain-fall of India. Part II treats of the variability of the rain-fall, and is summarized in Part III. As a rule stations having the smallest average rain-fall are those at which the variability of rain-fall is greatest; this is specially true of stations situated in dry plains or table lands which yield but little local evaporation, and where the winds from opposite quarters are strongly contrasted in point of dryness and dampness.

In a brief discussion of the relations of forests to rain-fall, three cases are adduced all of which confirm the view that the forests have increased the rain-fall, but the evidence is stated to be in no case absolutely conclusive.

The remaining portion of the memoir is occupied with the consideration of the questions as to whether any laws of coincidence or sequence can be derived; whether certain regions are, as a rule, subject simultaneously to similar or alternative conditions, whether any physical connection can be traced between abnormal meteorological conditions in a given region and the excess or deficiency of rain-fall there or elsewhere, and whether there are valid reasons for believing that the rain-fall of India is subject to any periodic law.

Diurnal period of rain-fall at Calcutta.—Mr. H. F. Blanford has found the following times of maximum and minimum in the daily period of rain at Calcutta. In the cold season (November to February) the principal minimum occurs at noonday, the maximum from 6 to 9 P. M.; in the hot season (March to May) there is only one well defined maximum and minimum, the former falling from 6 to 8 P. M., the latter between sunrise and 11 A. M. In the rainy season the principal maximum occurs from 2 to 4 P. M., and the principal minimum a little before 11 A. M. The daily period in the amount of rain agrees, in general, with the period of rain frequency. (*Indian Meteor. Memoirs*, IV, pp. 39-46.)

Diurnal period of rain-fall.—Dr. Hellmann has reported to the German Meteorological Society some of the results of his investigations upon the daily period of precipitation. He shows that the different curves may be reduced to types which are characteristic of certain localities and seasons. The afternoon maximum prominent in many places may be shown to rise naturally from the daily period of thunder-storms, whilst the equally widely extended nocturnal maximum, which is especially prominent in winter, and in all seasons on the west coast of Europe, appears to be connected with certain peculiarities in the daily period of air pressure, temperature, and wind velocity. (*Meteorologische Zeitschrift*, 1889, VI, p. 271.)

Diurnal periodicity of rain-fall at Hong Kong.—In the rainy season, from June to August, the diurnal variation is most strongly marked. The rain curves then run from a forenoon maximum at 9 A. M. to an evening minimum at 11 P. M. From March to May, the principal maximum falls at noon and the principal minimum at 7 P. M. In autumn and winter the diurnal curve is quite irregular. (*Ibid.*, p. 350.)

Daily period of rain-fall at Vienna.—The general results of the self-recording rain-fall observations for seven years at Vienna have been worked out by Hann. When given in groups of two months each no regular diurnal period is manifest, but in the total of seven months from April to October a periodicity is clearly expressed. The principal maximum of both frequency and quantity of rain occurs at the hour between 8 and 9 P. M. Hours of minimum amounts of rain-fall are 4 to 5 A. M. and 11 A. M. to noon. For the results of the spring, summer, and autumn months, which by themselves are somewhat irregular, a periodic formula of two terms is computed, and from examination of the computed results the essential periodicity of the various groups is discoverable. For all three seasons the minimum of rain-fall is between 9 A. M. and noon, the maximum in spring falls from 10 to 11 P. M., that in summer from 7 to 8 P. M., and in autumn from 2 to 3 A. M., with a secondary maximum from 8 to 9 P. M. The late hour of the maximum is in contrast with the general assumption that it occurs in the early afternoon.

Secular variation of rain-fall.—Professor Frank Waldo gives three tables containing the mean residuals of five-year periods from the mean rain-fall. The first is taken from Dr. Wild's "Regen Verhältnisse des Rus. Reiches;" the second is from Dr. Lang's article, "Der säculare Verlauf der Wetterung als Ursache der Gletcherschwankungen in den Alpen," and the third is compiled from American observations. The writer finds evidence in these tables of a long-period inequality having the following periods:

	Min. to max	Max. to min.	Total period.
	Years.		
Table I	18.5	19.6	38
Table II	20.4	13.4	34
Table III	12.9	12.3	25

The maximum at one station is often found to occur at the time of a minimum at another.* (*Am. Meteor. Journal*, v, p. 412.)

Causes of rain.—Professor von Bezold, in a paper before the Berlin Meteorological Society, discussed the manner of formation of precipitation. The mixing of warm moist air with cold air by which the temperature falls to the mean of the two can but seldom produce an appreciable precipitation. Precipitation occurs only when a mass of moist air is directly cooled, as in nature, chiefly by radiation and ascension. Clouds are most dense in the center of a cyclone where the pressure is a minimum, and are progressively less dense toward the periphery.

Mr. H. F. Blanford, in a letter to *Nature*, discusses the humid climate that fosters the rank exuberance of the Aruwhimi forests traversed by Stanley's expedition. He believes the excessive rain-fall is due to the equatorial position of the Aruwhimi basin where ascending convection currents prevail on a gigantic scale. By dynamic cooling these currents

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*This is different from the result derived by Dr. E. Brückner. (*Ante*, pp. 217, 218.) G. E. C.

part with nearly the whole of their vapor in the act of ascending and so do not carry away to other regions the water evaporated from the surface; the same water is evaporated and precipitated again and again, and the only loss of water to be supplied by outside winds is that carried off by river drainage, probably less than half of the rain-fall. "As the result of a long study of the rain-fall of India, I have become convinced that dynamic cooling, if not the sole cause of rain, is at all events the only cause of any importance, and that all the other causes so frequently appealed to, such as the intermingling of warm and cold air, contact with cold mountain slopes, etc., are either inoperative or relatively insignificant." (*Nature*, XXXIX, p. 583.)

Accuracy in measuring rain-fall.—From a long series of rain-fall observations Dr. A. Riggenbach draws the following important conclusions as to the accuracy attainable:

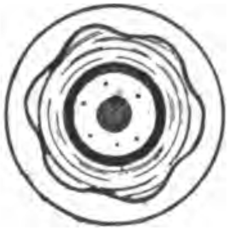
(1) The irregularity in the areal distribution of rain-fall is so great that the amounts collected in neighboring gauges, similarly exposed, differ on the average by 0.3^{mm} and in extreme cases by 5^{mm}. Add to this 0.2^{mm} for instrumental errors, and it will be seen that an accuracy of 0.5^{mm} is sufficient in the individual daily readings.

(2) In counting the days of precipitation the minimum amount of precipitation considered should not be under 0.5^{mm}.

(3) In monthly totals, fractions of a millimeter have no meaning.

(4) Yearly totals which agree to 0.5^{cm} are to be treated as identical. (*Meteor. Zeitschrift*, 1889, VI, p. 156.)

Hailstones, structure of.—Mr. E. E. Robinson gives the accompanying diagram of the cross-section of a hailstone measuring 2.9 centimeters in diameter. The center was circular and consisted of opaque ice, about the size of an ordinary hailstone; this was surrounded by a circle of almost perfectly clear ice, this again by a circle of opaque ice, and this once more was surrounded by almost clear ice, but with fine circular lines in it, and bounded by a frilled outline of opaque ice, which imitated in shape the spheroidal state of a drop of water. Outside this again was a thick layer of clear ice of crystalline form. The diagram is drawn natural size, the dark spots representing white opaque ice. Other large stones showed the same construction. Mr. C. D. Holt, in examining hailstones, has detected a metallic taste and also a flavor of ozone. All the stones showed an air-bubble at the center. (*Nature*, XL, p. 151.)



Floods in the middle Atlantic States from May 31 *to June* 3, 1889.—The following description of unprecedented rains and floods is collated from articles by Prof. T. Russell, of the Signal Service, and Prof. Lorin Blodgett, of Philadelphia, in the Monthly Weather Review for May and June:

A general storm, central in the Ohio valley on the 30th, passed slowly eastward over the Alleghanies in Virginia, where it remained until the evening of the 31st. Its slow movement was attended by conditions favorable to continued heavy rains,—east and southeast winds along the coast, high temperature, and dense saturation. As this saturated condition reached the higher ridges of the Alleghanies it developed the most excessive rain-fall of the century for so large an area, depositing a uniform sheet of from 6 to 8 inches of rain-fall during a continuous storm of from twenty-four to fifty-six hours' duration. This rain-fall was in sheets or masses rather than in drops, being described as "cloud-bursts" by observers in localities from Pennsylvania to Virginia. The intensity of the barometric depression was not large, but on the other hand rather small. At Pittsburgh the fall of the barometer was about one-fourth of an inch only, and the lowest isobars were 29.7 on the 30th and 29.8 on the 31st.

A table is given containing all the rain-fall observations made in the middle Atlantic States during the period of heavy rains, and approximate isohyetal lines are presented on charts. The times of beginning of rain are not very accordant, but the observations indicate that the progress of rain was from the coast inland, and from the south toward the north. The greatest rain-fall was in the northeast quadrant of the barometric depression, where there was a steep temperature gradient, the temperature increasing from 40° in the lake region to 70° on the Atlantic coast.

The floods resulting from these rains extended from southern New York over Pennsylvania, Maryland, and the Virginias, were unprecedented in height, and the most destructive ever occurring in the United States. A detailed statement of the enormous loss of life and property in the flooded region would require more space than is here available. Cities were inundated, bridges swept away, canals washed out, harbor improvements damaged, and millions of dollars worth of property destroyed. As a culmination of these disasters, the dam on the South Fork above Johnstown, Pennsylvania, gave way, and the immense body of water in the reservoir swept down upon that city and adjoining villages, causing the loss of nine thousand lives and thirty million dollars' worth of property.

VII.—WINDS AND OCEAN CURRENTS; GENERAL ATMOSPHERIC CIRCULATION.

The Helm wind.—Mr. W. Marriott has presented to the Royal Meteorological Society a report on the Helm-wind—a wind peculiar to the Cross Fell Range of mountains in Cumberland. This range is high, and runs from north-northwest to south-southeast without being cut through by any valley. From the top of the mountains to the plain on the west there is an abrupt fall of from 1,000 to 1,500 feet in a mile and a half. At times, when the wind is from some easterly point, the

Helm forms over the district, the chief features of the phenomenon being the following: A heavy bank of cloud rests along the Cross Fell range, at times reaching some distance down the western slopes, while at a distance of 2 or 3 miles from the foot of the Fell a slender roll of dark cloud appears in mid-air and parallel with the Helm cloud; this is the Helm bar. The space between the Helm cloud and the bar is usually quite clear, while to the westward the sky is at times completely covered with cloud. A cold wind rushes down the side of the Fell and blows violently till it reaches a spot nearly underneath the Helm bar, when it suddenly ceases. The Helm wind was observed sixty-three times in 1886, and nineteen times in 1887. (*Nature*, XXXIX, p. 431.)

Wind velocity in the United States.—Mr. F. Waldo, in an extended paper on the distribution of wind velocity in the United States, divides the signal service stations into typical groups, and plots the annual march of wind velocities for each group. The relation of wind velocity to areas of low pressure and the variation with altitude are discussed, and charts are given showing the January, July, and annual distribution. (*Am. Meteor. Journal*, VI, pp. 219, 300, 368.)

Sea-breeze.—Prof. W. M. Davis has made a report to the New England Meteorological Society, giving the results of special observations of the sea-breeze at one hundred stations, chiefly in Massachusetts.

The general theory that the sea-breeze is caused by the difference of temperature between the land and water requires the breeze to begin at the shore and to extend its area seaward, while observation shows that the breeze begins out at sea and works its way in-shore. It may be explained by supposing that the circulation of air is not established, but in process of establishment, and that the quick morning expansion of the land air causes a reverse gradient at the shore line, turning the surface winds toward the sea. This gradient disappears as the expansion of the air causes an upper outflow, and then the inland progress of the sea-breeze is effected. There should in this case be a difference of barometric pressure at land and sea stations, and such observations of pressure and temperature have been made by Blanford in India.

The depth of sea-breeze was determined by balloon observations at Coney Island to be between 300 and 400 feet. (*Am. Meteor. Journal*, VI, p 4.)

Ocean currents.—Prince Albert of Monaco has presented to the Paris Academy of Sciences a paper on the surface currents of the North Atlantic. Of 1,675 floats cast into the sea from the *Hirondelle*, 146 have already been recovered at various points. These apparently demonstrate a circular movement of the surface waters round a point situated somewhere to the southwest of the Azores. The outer edge of this current sets east-northeast to the neighborhood of the English Channel, where it is deflected southward along the coasts of Europe to the Canaries, thence trending southeast to the equatorial current, thus completing the circuit by merging in the Gulf Stream. (*Nature*, XL, p. 167.)

General circulation of the atmosphere.—M. Möller contributes an article to *Aus dem Archiv der Deutschen Seewarte* on the circulation of the atmosphere between the equator and the poles. The results arrived at differ from those of Professor Ferrel, especially with regard to the force of westerly winds in latitude 38° in the lower and upper strata.

M. Weyher has instituted a most interesting series of experiments designed to illustrate the cyclonic phenomena of the atmosphere, especially tornadoes, water-spouts, and dust whirls, and has found that his results agree in every point with those deduced from the mathematical theory of their movement. In one of his first experiments M. Weyher shows the analogy between water eddies and air whirls. In the water eddy the source of action must be at some distance below the surface, while in the air whirls the source of action must be located in the *upper* part of the air column, and the motion is communicated downwards. The most interesting of his experiments are those in which he artificially produces the phenomena of the water-spout. By means of a rotating tourniquet placed over cold water, an aerial eddy is caused which draws up the water, in the form of a spout composed of drops, to a considerable height; but when the water is heated, a clearly defined condensed vapor-spout makes its appearance. With from fifteen hundred to two thousand rotations per minute the vapor from the heated water is found to condense itself into a visible sheet enveloping a clearly defined and rarefied-central nucleus, conical, and tapering downwards. Besides this vapor-spout, water drops are carried up as in natural water-spouts until they are thrown out beyond the influence of the upward current; the pressure and temperature conditions in different parts of the area are also investigated by means of a manometer. It was found that the rarefaction at the center of the tourniquet is transmitted almost unaltered in intensity to the center of the whirl on the surface, while the thermometer at the same time at first shows a fall and then a rise of temperature, the latter evidently due to the friction of the rapidly moving air against the surface.

The analogous phenomena of a cyclone are very fairly imitated by an apparatus consisting of a large tourniquet placed over a table covered with a number of pins mounted with movable threads of wool; the tourniquet is mounted so as to be capable of translation as well as rotation, and changes of pressure are registered by a manometer which connects with a hole in the surface of the table by means of a rubber tube. On rotating the tourniquet and giving it a forward motion, the directions and positions of the threads show both the horizontal and vertical components of the winds thus produced, including the region of calm in the center as well as the outward and downward motion at the anti-cyclonic border. The variations of pressure, when pointed out, show a curve similar to that in a symmetrical cyclone.

Hail is explained as being caused by vapor drawn up into the center of a cyclonic system, and is essentially similar to the explanation given by Ferrel and Möller. These experiments do not of course fulfill all the conditions which prevail in nature, since, in that case, the rotation is doubtless kept up by the upward movement along the axis and the consequent aspiration of the surrounding air into the area of gyration, but in general the analogy seems quite complete. (*Nature*, XXXVIII, p 104.)

Mr. Ralph Abercromby has made special observations on the upper wind currents over the equator in the Atlantic Ocean. In December (1888) the northeast and southeast trades both turned into a common, light-surface, easterly current along the line of the doldrums; low clouds from southeast drove over the northeast trade up to 15° north, and from 300 miles south of the equator, a very high current from northwest prevailed over the southeast trade. From the equator southward 300 miles, no high observations were obtained. In May a somewhat different system prevailed. The northeast trade turned to north as it approached the doldrums, instead of towards the east, as in December. In the calm belt, it met a light, easterly current without producing much rain; while further south the regular southeast trade was experienced as far south as 8° south, when the northeast monsoon prevailed along the Brazilian coast nearly down to Rio Janeiro. No southeast wind could be discovered at any level over the northeast trade.

These observations are held to confirm in a striking manner his previous discovery "that the highest air current over the equatorial doldrums is from the eastward, lying between the southwest current which flows on one side over the northeast trade, and the northwest current which flows on the other side over the southeast trade."

With respect to the general circulation of the atmosphere we know that the surface trades either die out at the doldrums or unite into one moderate east current; that the low and middle currents over the doldrums are very variable, but that the winds at these low and middle levels, 2,000 to 20,000 feet, come usually from the southeast over the northeast trade, and from the northeast over the southeast trade, and that the highest currents—over 20,000 feet—move from east over the doldrums, from southwest over the northeast trade, and from northwest over the southeast trade.

What we do not know is the relation of the southeast low and middle current over the northeast trade to the southeast trade on the other side of the equator, nor do we yet know what becomes of this middle current in the northern hemisphere.

The simple scheme which assumes nothing but an upward current over the doldrums, and a return current toward each pole is not confirmed by observations. There is always a regular vertical succession of the upper currents as we ascend according to the hemisphere. (*Nature*, XL, p. 297.)

Thermo-dynamics of the atmosphere.—Dr. W. von Bezold has continued his contribution upon the thermo-dynamics of the atmosphere in the Sitzungsberichte of the Berlin Academy. The whole series of papers are being prepared by Professor Abbe for publication in this Report.

VIII.—BAROMETRIC PRESSURE AND ITS VARIATIONS; HYPSOMETRY.

Die Vertheilung des Luftdruckes über Mittel- und Süd-Europa dargestellt auf Grundlage der 30jährigen Monats- und Jahres-Mittel, 1851–1880; *nebst allgemeinen Untersuchungen über die Veränderlichkeit der Luftdruck-Mittel und Differenzen, sowie deren mehrjährige Periode, von* J. Hann, *Wien,* 1887. This volume is "Band II, Heft 2," of the *Geographische Abhandlungen,* published by Prof. Dr. A. Penck. It is divided into the following sections: Introduction. Chapter I. On the methods for obtaining comparable mean air pressures and for drawing correct isobars. II. Monthly and annual isobars of central and southern Europe. III. Annual period in the air pressure relations of Europe. IV. Connection between the air pressure anomalies of Europe and the temperature anomalies in central Europe. V. The mean and absolute variability of the monthly and annual means of air pressure. VI. The probable error of the thirty-year mean air pressure. VII. The variability of the differences of the mean air pressure of two places. VIII. Reduction to the normal period of 1851–1880. IX. Reduction of the mean air pressures to the same level. X. Many year period of the air pressure. The three plates at the end of the volume contain the isobars for each month and the year reduced to sea-level, and for January, May, July, October, and the year reduced to 500 meters elevation. The annual period of air pressure in various portions of Europe is presented by diagrams. The curves are quite irregular, showing the phases from continental to oceanic climates. The relation between the temperature and air pressure deviations for the seasons is investigated by Professor Hann. One conclusion reached is that "in all cases of very warm winter in central Europe the air pressure in the northwest over the Atlantic Ocean was too low; if central Europe alone be considered, very cold winters occur just as frequently by high as by low pressure." The following table shows approximately the dependence on the latitude of the average variability of the monthly and annual air pressures:

Latitude.	60°	56°	52°	48°	46°	43°	36°	32°	20°
	mm.	*mm.*	*mm.*	*mm.*	*mm.*	*mm.*	*mm.*	*mm.*	*mm.*
Mean monthly	3.06	2.92	2.58	2.34	1.95	1.80	1.48	1.00	0.40
Annual.................	1.14	0.96	0.78	0.72	0.59	0.56	0.48	0.36	......

(F. Waldo, *Am. Meteor. Journal,* v, p. 511.)

Effect of lunar attraction on the atmosphere.—Professor Börnstein, of Berlin, has taken up the question of the effect of the moon's attraction on the atmosphere. At Singapore, Melbourne, St. Helena, and Batavia observers have succeeded in establishing a daily variation in the barometric pressure dependent upon the moon, and having two maxima and two minima, with an amplitude varying from 0.079 to 0.2 millimeters. But opposed to these are the observations of Laplace on the variations of the barometer at Paris, as also of Kreil in Prague, and Bessel's observations on atmosphere refraction. All these last-named observers found that the action of the moon on the earth's atmosphere is either *nil*, or else the reverse of that described. Professor Börnstein then discussed the question whether any ebb or flow of the atmosphere could possibly be detected by the means at our disposal, and showed that the mercurial barometer can never give indications of such action, since it is itself affected by the alterations of gravity that are due to the varying position of the moon. He explained the phenomena observed at the four stations mentioned above as due to the fact that they are situated on the sea-coast at places on the earth's surface where the ebb and flow of the sea is very considerable. The barometric effect is, then, a secondary one, due to the changing position of the sea-level. (*Nature*, XXXIX, p. 600.)

Charts of barometric pressure.—The Meteorological Council have published charts showing the mean barometric pressure over the Atlantic, Indian, and Pacific Oceans. These are issued in the form of an atlas, and give in a very complete manner the barometric means and rain over all oceans. Separate charts are given for February, May, August, and November, which are selected to represent the characteristic distribution of pressure for the respective seasons. The number of observations used in the preparation of the charts is, for the Atlantic Ocean, 339,300; Indian Ocean, 163,000; Pacific Ocean, 88,300. The barometric means are given for areas of 5 degrees of latitude by 5 degrees of longitude in large figures, and in smaller figures are given the mean for areas of 2 degrees in latitude and longitude, the several means being obtained from the daily averages; the isobars are given for each tenth of an inch. The general charts which give the isobars of the globe show very clearly the prevalence of high pressure areas in each ocean in each of the four seasons; it is seen that these areas oscillate in position and alter somewhat in intensity with the seasons, but there are many characteristics in common. The Northern Indian Ocean, which is much more surrounded by land, is however an exception, the high pressure being situated over the northern part of the ocean in November and February and decreasing southwards, whilst in May and August the pressure is lowest in the north and increases southwards, this change being closely related to the monsoon winds. These charts are considerably in advance of any previous work of a similar nature, and will materially aid in explaining the general circulation of the wind over the globe. (*Nature*, XXXVIII, p. 196.)

Diurnal variation of the barometer.—Mr. Henry F. Blanford has made an important study of the relations of the diurnal barometric maxima to certain critical conditions of temperature, cloud and rain-fall. The author has re-examined the suggestion made by Espy (1840) and Kreil

(1861), that the morning maximum of pressure is due to a reaction of the upper cloud layers against the expanding lower air, and finds that the results of observations at Calcutta, Melbourne, and Batavia are, on the whole, favorable to this hypothesis, since the morning maximum of pressure approximately coincides with the instant when the temperature is rising most rapidly. At tropical stations the barometric maximum follows the time of most rapid heating by a shorter or longer interval, but this may probably be attributed to the action of convection which must accelerate the time of most rapid heating near the ground surface; while the barometric effect, if real, must be determined by the condition of the atmosphere up to a great height. With reference to Lamont's criticism of Espy's theory, a condition is pointed out which alters the data of the problem, viz, the resistance that must be offered to the passage of the pressure wave through the extremely cold and highly attenuated strata of the upper atmosphere. With respect to the evening maximum of pressure it is pointed out that in India, and also at Melbourne, there is a strongly marked minimum of cloudiness between sunset and midnight, which on the average coincides with the evening maximum of the barometer. In the author's opinion these and other facts seem to indicate a compression and dynamic heating of the cloud-forming strata, and that therefore the diurnal barometric oscillations are dynamic phenomena. (*Nature*, XXXVIII, p. 70.)

Mr. H. H. Clayton has a paper on the annual and diurnal periods of the barometer. Referring to the result pointed out by General Greely that the epochs of maxima and minima of air pressure show a coincidence, the author traces the probable cause of the occurrences of the maxima to the expansion and overflow of air from Asia and America to the pole; and of the minima at the pole to the fact that the overflow from the pole towards those continents is not replaced by an influx in that direction from the oceans. The retardations of the annual maximum from the Arctic region to the equator, and of the minimum from the southern parts of the continent to the Arctic region, is also attributed to the relative heating and cooling of the continent and oceans. (*Am. Meteor. Journal*, VI, p. 150.)

Mr. A. Angot, in a paper on the diurnal variation of the barometer (*Annuaire Soc. Méteor. de France*), finds that the diurnal variation results from the superposition of two distinct waves. One of these is expressible as a harmonic function, the constants of which depend on the latitude and geographical features; this wave is due to the diurnal variation of temperature of the air near the earth's surface.

The second wave has a semi diurnal period and its amplitude varies with the latitude of the place and with the declination of the sun.

Dr. J. Hann has made an exhaustive investigation of the diurnal range of the barometer over the globe. He has calculated the harmonic co-efficients for each month, and for the year, for a large number of places, and has investigated the variation both of the phases and of the amplitudes of the single and double oscillations. The latter show a remarkable independence of geographical and seasonal influences and appear to be connected with a cosmical origin. The investigation also shows that the amplitudes of the semi-diurnal oscillation decrease with height in exact proportion to the pressure, and have a marked dependence upon latitude. The yearly range exhibits two maxima at the equinoxes and also a third maximum which falls in January in both hemispheres, while over the whole globe the amplitude of the double daily oscillation is smallest in July. (*Nature*, XXXIX, p. 517.)

Mr. F. C. Bayard has reduced the hourly records of the barometer at the nine observatories, in Great Britain and Ireland for the years 1876 to 1880, and has compared the resulting curves of diurnal range.

The curves of inland places are smoother than those of sea-coast stations, and the curves of places to the westward are more irregular than those of places to the eastward. In going toward the north the diurnal range diminishes. (*Nature*, XXXIX, p. 623.)

Krakatoa air waves.—Part II of the report of the Krakatoa committee of the Royal Society, has been prepared by General R. Strachey and investigates the extraordinary air waves and sounds caused by the Krakatoa eruption. Barometer traces from forty-seven stations scattered over the whole world exhibit the passage of air waves travelling around the world not less than seven times. The general velocity at which the wave spread outward in concentric circles from Krakatoa as a center was 700 miles per hour, which is slightly less than the velocity of sound at zero Fahr., viz, 723 miles. A decided variation of velocity was discovered in those portions of the wave which moved with or against the earth's rotation, such variation being due to the prevalent drift of the winds.

In the extra-tropics the wave moving from west to east was accelerated, and that moving from east to west retarded, by about 14 miles per hour; within the tropics the wave which passed through Mauritius was affected in a reverse manner, the passage eastward being retarded, while the westward was comparatively unaffected, the amount corresponding to an east wind of about 10 miles per hour. These amounts are almost precisely those given by Ferrel for the easterly and westerly components of the prevailing currents at their respective latitudes.

The area over which the sound of the eruption was heard is estimated at one-thirteenth of the entire earth's surface. (*Nature*, XXXIX, p. 566.)

A curious sudden barometric oscillation passed over central Europe on the evening of January 31, 1889. Dr. E. Herrmann, of the Deutsche Seewarte, traces it from Kertum (latitude 54° 54′), where it occurred at $7^h\ 50^m$ P. M., Berlin time, to Pola (latitude 49° 42′), which it reached at $4^h\ 38^m$ A. M., on February 1, having travelled at the rapid rate of about 71 miles an hour. The cause of the phenomenon is unexplained.

Hypsometry.—Dr. J. M. Pernter gives the following hypsometric formula:

$$h = 18399.8 \left(1 + \alpha \frac{t' + t''}{2}\right) \times$$
$$\left[1 + 0.378 \cdot \tfrac{1}{2}\left(\frac{e'}{b'} + \frac{e''}{b''}\right)\right] \times$$
$$(1 + 0.00259 \cos 2\,\varphi) \times$$
$$\left(1 + \frac{5}{8} \cdot \frac{2\,z + h}{6371103}\right) \log\left(\frac{b'}{b''}\right) \cdot$$

in which z is the altitude above sea-level of the lower station, and b', b'' are barometer heights corrected, not only for temperature and instrumental error, but for differences in gravity between the two places. Accompanying the formula there is given a series of tables which for the most part have been newly computed. (*Repertorium der Physik*, 1888, p. 161.)

IX.—CYCLONES; TORNADOES; THUNDER-STORMS; WATER-SPOUTS; GENERAL WEATHER RELATIONS.

Hurricane theories.—Hon. Ralph Abercromby compares the old and the modern views of hurricanes. The old conception was of a circular-shaped eddy, round which the wind blew in circles. Modern research shows that a hurricane is an oval eddy, and that the wind blows in an incurving spiral round the vortex, not round the center of the oval, and that the incurvature is less in front than in rear of the vortex. A hurricane is always changing its shape, so that the vortex is one day on one side of the oval, and towards another side on the next.

No rule is possible for determining absolutely the bearing of the vortex by observations on board a single ship, whereas it used to be stated that facing the wind the vortex bore eight points to the right in the northern and to the left in the southern hemisphere.

We can say now only, that when fairly within the storm field and facing the wind, the vortex will be from eight to twelve points to the right of the wind in the northern hemisphere and to the left in the southern hemisphere. Greater precision can be obtained in certain circumstances. For example, if a ship is nearly in front of the vortex, the bearing of the vortex will probably not be much more than eight points to the right or left, and in the rear of a hurricane the vortex may bear twelve points to the right or left of the wind, because the wind is there very much incurved. A ship should then always lie to till the barometer begins to rise, otherwise she will be liable to run right into the vortex.

Modern research has proved that a hurricane is usually imbedded in some prevailing trade or monsoon, and that there is a belt of intensified trade-wind outside the true storm field. This belt is always on the side of the hurricane farthest from the equator. A ship in this belt experiences an increasing trade without change of direction, and with a falling barometer, though she may be far away from the line of the vortex. Now she would experience the same things if she were in the line of progression; but as there is no means of knowing which is the case, the empirical rule is: lie to till the mercury has fallen 0.6 inch before beginning to run. (*British Association Report*, 1888, p. 586.)

Theory of cyclones.—From a mathematical study of cyclonic motion M. Henri Lasne finds that the "eye" of tropical cyclones (the small area of calm, and of clear sky, in which the air is relatively warm and dry) is accounted for and explained by theory. A regular cyclonic motion of great intensity like that in tropical hurricanes makes possible a feeble descending motion at the center. In the irregular cyclones of temperate latitudes having large horizontal and small vertical extent no such phenomena can be developed; in these the center is not the locus of greatest energy. (*Annuaire Soc. Météor. de France*, 37^e année, p. 126.)

Tropical cyclones.—W. Doberck discusses the relation of the wind at Hong Kong to the typhoons occurring in 1886 and 1887. Only those within 300 miles of the observatory are considered. No connection is found between the distance from the center and the direction of the wind, but the latter depends on the bearing of the center. The wind has a tendency to blow along the southern coast of China when a typhoon is raging in the China Sea, so that the wind in such cases veers only about half as much while the typhoon moves westward as in other cases, and for the same reason the angle between the wind and the radius vector is larger than usual when the center is situated to the south of Hong Kong. A cut is given showing the direction of incurvature on all sides of the storm center when in the vicinity of Hong Kong. (*Nature*, XXXIX, p. 301.)

Mr. H. F. Blanford has given the results of his study of the incurvature of the winds in tropical cyclones as observed in the Bay of Bengal. In order to derive practical rules for navigators he has measured the angle between the wind direction and the radius vector instead of between the wind direction and the isobar, as is done by Professor Loomis, and has restricted the measurements to wind observations of ships at sea within the influence of the storm, and to good observations on the coast. His results confirm the general fact of a great incurvature obtained by Professor Loomis, but differ somewhat in the amount:

(1) The mean of one hundred and thirty-two observations between latitudes 15° and 22°, within 500 miles of the storm center, gives the angle 122° between the wind direction and its radius vector.

(2) The mean of twelve observations between the same latitudes, within 50 miles of the storm center, gives the angle 123°.

(3) The mean of sixty-eight observations between N. latitudes 8° and 15°, within 500 miles of the storm center, gives the angle 129°.

For the guidance of navigators Mr. Blanford formulates the following rules:

(1) In the north of the Bay of Bengal, standing with the back to the wind, the center of the cyclone bears about five points on the left hand or three points before the beam.

(2) In the south of the Bay it bears about four points to the left hand or four points before the beam.

(3) These rules hold good for all positions within the influence of the storm up to 500 miles from the storm center.

The author concludes by pointing out that these facts are fatal to the cyclone theories of M. Faye. (*Nature*, XXXVIII, p. 181.)

Mr. S. R. Elson, an experienced East Indian pilot, comments on Mr. Blanford's rules for avoiding cyclones, and shows that a number of modifications must be introduced in applying these rules in special localities and under special circumstances. One of these is the strong currents setting in in advance of cyclones that drift the vessel far out of its course and towards the "eye of the storm." In and off the Hooghly River, whatever be the direction and motion of the cyclone, the first wind invariably blows from the northeast, and the regular rules are inapplicable without taking account of this peculiarity. Mr. Elson thinks Mr. Blanford's rules for finding the storm's center are perplexing and liable to misconstruction. (*Nature*, XXXIX, p. 69.)

In vol. IV, part V, of the Indian Meteorological Memoirs, Mr. F. Chambers, has presented a study of the cyclone of the 25th of May to the 2d of June, 1881, in the Arabian Sea. After a painstaking preparation of the data, it is classified with respect to the gradient, with respect to the distance from the center, and with respect to the octant of the cyclone. And many relations of the pressure, the force and directions and incurvature of the wind in different parts are derived. The results show no regular increase of the angle between the radius and the wind in approaching the center, though no doubt this angle is greater near the center than farther away from it, but the observations are too few to give averages showing a regular progression. As an observational fact it was found that the cyclone moved from that side where the wind was strongest to that side where it was weakest, and Mr. Chambers explains this by showing that the first effect of the approach of a tropical cyclone is to neutralize the normal wind, and so to cause less than the normal amount of air motion. As one practical outcome of the study, rules are formulated for the guidance of the navigator when caught in a cyclone in the Arabian Sea, and some interesting relations are suggested between the direction of the swell and the direction of the wind as throwing additional light on the position of the cyclone center.

Paths of cyclones.—In vol. IX of *Aus dem Archiv der Deutschen Seewarte* Dr. van Bebber investigates typical weather conditions and traces the influence of cyclonic areas upon the weather with a view to the discovery of the laws governing the changes in direction of their tracks and of their rates of progression. It is shown that the depressions move along certain tracks with greater than average velocity.

Thunder-storms.—Karl Prohaska has collected and discussed a mass of thunder-storm observations from about three hundred stations in Steiermark, Kärten, and Oberkrain for four years, 1885 to 1888. About nine thousand reports were received annually, making an average of thirty reports from each station.

The average duration of a thunder-storm was 1.4 hours, being 1.2 in spring, 1.4 in summer, and 1.6 in autumn. The average velocity of passing was 30 kilometers per hour; hence the extent of the thunder-storm cloud was at the highest 43 kilometers; but if it be remembered that the above computed duration represents the mean time between first and last thunder, the average extent of the usual thunder-storm cloud does not exceed about 37 kilometers. The velocity of propagation of the thunder-storms is materially less in these districts of the southern Alpine mountains than in southern Germany. Thus the afternoon velocity is 10 kilometers per hour greater for the latter than for the former. This is due to the large number of local storms, "wärmegewitter," with their slow rate of movement. In hot summer days in the Alps, in spite of a high barometer, frequent local thunder-storms arise, which seem to be almost unprogressive. On certain selected days in July, 1887, for the hours from noon to 6 P. M., there were 1,193 reports, and 218 for the remaining eighteen hours. Thunder-storms occur most frequently when the barometer is about normal; those from the north and south have the smallest area of extension, those from the west the greatest.

In addition to these statistical results, Prohaska undertakes to explain the occurrence of thunder-storms and rain with a rising air pressure. The basis of his theory rests on the assumed backward inclination of the axes of cyclones. This assumption leads to the conclusion that the rise of the barometer immediately following the passage of barometric minima is occasioned by dense heavy air adjacent to the earth's surface pressing into the region of low pressure. Now, as heavy air masses come into a region of lower pressure they experience a continually smaller compression, and consequently there is developed an upward gradient, and a rise of the air strata lying thereon must ensue. Dynamic cooling is thus brought into play, so that in higher air strata a fall of temperature takes place whilst the barometer is still falling. Thus there is a causal connection between rising air pressure and the formation of precipitation and thunder-storms, inasmuch as the rising air pressure consists in the formation and condensation of a cloud swell advancing in front like a true wave movement. (*Meteorologische Zeitschrift*, 1889, VI, p. 226.)

The report of the director of the Hong Kong Observatory for 1888 contains a special study of thunder-storms in the colony during the past five years. Dr. Doberck states that they are most frequent in May, and that they have not occurred in November, December, and January. In diurnal period they are most frequent about 1 A. M., and least so at about 8 A. M., in the proportion of about two to one.

Professors Mohn and Hildebrandsson have published a study of thunder-storms in the Scandinavian peninsula. (Upsala, 1888, 55 pp.) This monograph supplies for the Scandinavian peninsula statistical information about thunder-storms similar to that so richly collected in the states of central Europe. The meteorological conditions favorable to thunder-storms in eastern Norway are given in detail, and by their aid thunder-storms can be predicted from the morning weather map.

Dr. E. Wagner has investigated the periodicity of thunder-storms in Bavaria, Wurtemburg, and Baden, and finds that they have a period of twenty-nine days, containing three maximum points, the chief of these being in the last half quarter, the next at new moon, and the least at full moon. No physical explanation for this is attempted. (*Meteorologische Zeitschrift*, 1889, VI, p. 299.)

Doctor Wagner has tabulated the observations of thunder-storm frequency in Bavaria and Wurtemburg with respect to the phases of the moon, and considers that they each show a well-marked maximum between the last quarter and the fourth octant. (*Ibid*, p. 300.)

Dr. Karl Lang has reported to the German Meteorological Society the results of his investigations upon the velocity of propagation of thunder-storms in south Germany. He finds a close connection between the velocity of propagation and the proximity of storm tracks. Thus in the winter months, when van Bebber's cyclone track No. IV has its most southerly position, the velocity of thunder-storms is greatest, and geographically the velocity diminishes from north to south. Thunder-storms coming from the west and west-southwest most frequently arise in the southern border of cyclones and travel the fastest, while those from northwest to northeast travel the slowest. (*Ibid*, p. 271.)

Dr. Franz Horn finds from a study of thunder and hail storms in Bavaria during the years 1880 to 1888 that no hail has ever been reported without a simultaneous observation of electrical discharge. The hours of greatest thunder-storm frequency are in the afternoons; in the winter between 2 P. M. and 3 P. M., and in summer an hour later. (*Ibid*. p. 272.)

Tornado charts.—Lieut. J. P. Finley has published in successive issues of the *American Meteorological Journal* State tornado charts showing paths of tornadoes, each accompanied by a brief table of statistics. These ought to be a concise presentation of valuable information collected on this subject; but as shown by Professor Hinrichs, they contain rather a large amount of mis-information due to the utter absence of scientific criticism in the compilation of the data.

Tornadoes and derechos.—In a paper entitled "Tornadoes and Derechos," Professor Hinrichs describes the characters of tornadoes and of the peculiarly destructive squalls of Iowa, which he has named the derecho.

He defines the derecho as a violently progressing mass of cold air, moving destructively onward in slightly diverging straight lines, in Iowa generally towards the southeast. The barometer rises suddenly and the thermometer falls greatly under the blow of this cold air of the upper strata suddenly striking the ground. While occurring occasionally in the spring and early summer, the derecho has its period of greatest frequency and intensity in the midsummer months, July and August. In these two months the tornado does not occur in Iowa. The writer shows that the list of Iowa tornadoes published by Lieutenant Finley is untrustworthy, and then gives a corrected list of all the authentic tornadoes that can be verified by reports. These have occurred in April, May, June, and October. (*Am. Meteor. Journal*, V, p. 385.)

Water-spouts.—Mr. S. R. Elson reports several water-spouts observed on the Hooghly. One was seen "projected from the level vapor-plane of a towering cumulus cloud; through a telescope it showed well the downrush on the inside of the tube, and its counterpart the whirling uprush on the outside, twisting and coiling round and round against the watch hands (face upwards). In another case after a water-spout had been observed for some time and it was beginning to shrink and to draw itself upwards, "the inside downrush was again seen to advantage and the simultaneous upward whirl around the dense remains of the tube, which I can not do better than liken to the turning inside out of a coat-sleeve, only the end of the tube was always ragged; and here, where the reversing process was taking place, there was great commotion in the air currents. I had a good telescope, observed these phenomena very carefully, and was on the alert for optical illusions." (*Nature*, XXXIX, p. 334.)

Rain-fall and cyclones.—The Report on the Meteorology of India in 1887 (Calcutta, 1889) calls attention to the relation that has previously been shown to obtain between rain-fall and cyclones during the southwest monsoon period. There is a very marked tendency for cyclonic rain-storms to run along the trough of low pressure, the mean position of which during the rains stretches from Sind or Cutch in an east southeast direction to the eastern districts of the central provinces, Orissa and Chutia Nagpur. An examination of the storm tracks of 1887 and 1888 has shown that the great majority of these storms marched across the coast in the direction of the belt of lowest pressure at the time of their formation, and hence it may be inferred that if a depression forms during the rains in the bay it will very probably run along the belt of lowest pressure or the trough of minimum pressure in existence immediately antecedent to its formation. Since the chief characteristic of such a barometric depression is light and variable winds, it will be seen that this principle virtually coincides with the rule that cyclonic storms in the bay march in the direction of least relative air motion immediately antecedent to the formation of the cyclone, which is a more general rule than the former.

Abnormal weather.—The eastern as well as the western hemisphere was visited by extraordinary spring weather in 1889. The following note is from *Nature*, June 20, 1889:

"It appears that the somewhat eccentric weather of western Europe during the present year finds a parallel in both China and Japan, where people complain bitterly of the sudden changes of temperature, the premature heat followed by cold "snatches," the storms in quick succession and of great intensity. In northern China there has not been known such an inclement spring since foreigners have resided in the country. A warm week in February broke up the ice on the Peiho River prematurely, but afterwards cold set in with great severity, and March was characterized by a succession of gales, lasting sometimes a week without intermission, and as late as the 24th the ground was covered with snow."

Dr. B. Andries has investigated the so-called cold period in May which is popularly supposed to prevail about the 10th of that month; he finds that while each year frosts occur in May, after a period of warm weather has excited a hope for continuous rise of temperature, yet the same thing occurs in April and June, and the weather of May is more uniform than all the other months except October. (*Das wetter*, June, 1889.

X.—ATMOSPHERIC ELECTRICITY; LIGHTNING; TERRESTRIAL MAGNETISM; AURORAS.

Atmospheric electricity.—Dr. Less (Berlin) has studied the occurrence of rain, hail, and snow, in connection with thunder-storms. He concludes that on days of thunder-storms in winter, the temperature diminishes with altitude at a much greater rate than on days of precipitation, and thunder-storms seem to cease entirely when there is considerable amount of condensed moisture in the atmosphere. He considers that both these results afford substantial confirmation of Sohncke's theory that the electricity of thunder-storms first arises from the friction between ice and water particles, but additional consideration must be added in order to explain quantitatively the high potential of the lightning discharge.

Mr. Angus Rankin has a paper in the Journal of the Scottish Meteorological Society on the conditions of the occurrence of St. Elmo's fire on Ben Nevis. He finds that the fifteen observed cases occurred a few hours after the passage of the center of a cyclonic depression, when the temperature was rapidly falling, the pressure rising, the wind west-northwest, and heavy showers of snow and snow-hail prevailing. (*Nature*, XL, p. 439.)

Globular lightning.—At the British Association meeting in 1888 Sir William Thompson expressed the belief that ball lightning is altogether physiological. A vivid flash produces an intense action on the center of the retina, and when the eyes are moved, a spot of light follows, which is the marvellous ball lightning frequently reported.

At the American Association meeting in 1889, Prof. Dr. T. C. Mendenhall presented a series of observations of ball lightning, the concurrent testimony of which had convinced him of the reality of the phenomenon.

Lightning.—Mr. W. G. McMillan describes a lightning discharge which struck a house in Calcutta. The instantaneous discharge of ribbon-lightning was apparently converted on entering the house into a relatively slowly moving fire-ball. The effect is described as that of an intensely brilliant ball of yellow fire about 6 or 7 inches in diameter which passed across the room at a pace sufficiently slow to allow it to be followed by the eye; about half way across, it appeared to be momentarily checked, and then, seeming to burst with a deafening report, which shook the whole house, it scattered and passed onward. A large portion of the oxygen in the air immediately surrounding the path of the flash was converted into the oxides of nitrogen. (*Nature*, XL, p. 295.)

Lightning conductors.—A discussion on lightning and lightning conductors was held at the British Association meeting in 1888, in which Professor Lodge, Mr. W. H. Preece, Lord Rayleigh, Professor Forbes, Sir William Thomson, and others participated. The discussion took a wide range, though primarily designed to elucidate the question as to the relative superiority of iron and copper wire. Mr. Preece stated that both iron and copper are efficacious. Sir William Thomson knew of no experiment which proved iron less efficient, and it is preferable because of its higher melting point as well as on account of its cheapness. Contrary to the opinion of Mr. Preece, Professor Lodge believed that lightning conductors are sometimes inefficient even when erected in accordance with all the demands of electrical science, and, as a case in point, he instanced M. Melsen's hotel at Brussels which had been struck and burned although elaborately protected.

Terrestrial magnetism.—Prof. Arthur Schuster has presented to the Royal Society an elaborate investigation on the diurnal variation of terrestrial magnetism, in which he makes use of the method of harmonic analysis to separate internal from external causes of variation.

If the magnetic effects can be fairly represented by a single term in a series of harmonics so far as the horizontal forces are concerned, there should be no doubt as to the location of the disturbing cause, for the vertical force should be in the opposite direction if the origin is outside from what it should be if the origin is inside the earth. If it be then a question simply of deciding whether the cause is outside or inside, without considering a possible combination of both causes, the result should not be doubtful, even if we have only an approximate knowledge of the vertical forces. He had previously shown that the leading features of the horizontal components for diurnal variations could be approximately represented by the surface harmonic of the second degree and first type, and that the vertical variation agreed in direction and phase with the calculation, on the assumption that the seat of the force is outside the earth. In the present more complete investigation the matter has been more fully taken up and the original conclusions have been confirmed.

The observations taken at Bombay, Lisbon, Greenwich, and St. Petersburgh are used, and the potential is computed in thirty-eight terms of a series of surface harmonics by means of the horizontal components only. From the potential thus computed the vertical force is deduced both on the assumption of an inside and an outside origin of the variation. By tabulating the amplitude and phase of the forces computed on each assumption and by comparing the results with the actually observed values a complete disagreement is found with the results obtained on the assumption that the disturbing force is inside the earth and nearly complete agreement on the alternative hypothesis.

The observed amplitudes are found in all cases to be considerably smaller than the computed ones.

In an appendix Prof. H. Lamb shows that if the earth be heated as a conducting sphere, in which induced currents are excited by an external cause, this reduction in amplitude may be accounted for.

Prof. Balfour Stewart's suggestion that convective currents in the atmosphere moving across the lines of the earth's magnetic forces are the causes of the daily variation, gains much in probability by this investigation. If the daily variation of the barometer is accompanied by a horizontal current in the atmosphere similar to the tangential motion in waves propagated in shallow canals, and if the conductivity of the air is sufficiently good, the effects on the magnetic needles would be very similar to those actually observed. (*Nature,* XXXIX, p. 622.)

Auroras.—Mr. H. Hildebransson gives a summary of the result of the elaborate observations of the aurora made by the Swedish polar expedition at Bossekop (situated in the maximum zone of auroras, on the coast of northern Norway.)

(1) A mean of 371 measures gave the azimuth of the summit of the auroral arch in S. 24° 12′ E.

(2) A mean of 87 measures on the position of the center of the corona gave its altitude 79° 55′, azimuth S. 7° 12′ E. This point is nearly in the magnetic zenith, but not in the same vertical as the highest point of the arch.

(3) The breadth of the auroral arches varies with their elevation above the horizon. The arches consist of rays running in the direction of the breadth of the arch and converging toward the magnetic zenith.

(4) The auroral light sometimes formed a true spherical zone parallel with the earth's surface, thus floating in space as a horizontal layer of light.

(5) The movements of the arches from north to south and from south to north were almost equally frequent.

(6) Anomalous forms of arches were very frequent,

(7) Often, waves of light run along the arches; eastward and westward motion of the waves were equally frequent.

(8) The author rejects the classification of auroral forms given by Weyprecht, and distinguishes only two different forms of auroral light, viz, zones, or horizontal layers of light, and arches, composed of rays parallel to the dipping needle. The arches are of four varieties: (1) arch, or a regular band; (2) band or drapery; (3) spiral; (4) pseudo-arch.

(9) The auroral light is of two kinds: (1) the yellow light, entirely monochromatic; (2) the crimson or violet light.

(10) No sound was ever heard from the aurora.

(11) The aurora was never seen to descend below the mountains or lower clouds. Only two or three times it is possible that the light was seen below the upper clouds. Direct measures of the parallax from the end of a short base (573 meters) gave an average height of 55.1 kilometers, and by several other methods about 2 kilometers was found to be the probable mean height of the aurora.

(12) No annual variation could be discovered.

A daily variation having its maximum at 3 P. M., and minimum at 8 A. M., local time, was computed. (*Nature*, XXXVIII, p. 84.)

XI.—SCINTILLATION; LIGHT AND COLOR OF THE SKY; TWILIGHT GLOWS, ETC.

Scintillation of the stars.—Dr. J. M. Pernter has conducted some scintillometer observations upon the Sonnblick (elevation 3,100 meters), in order to determine whether there is greater steadiness at high than at low levels. Two of Exner's scintillometers were used in the work, and simultaneous observations were made on two nights at the summit and at Rauris (900 meters). The result of these comparisons showed that the scintillation was noticeably greater at the summit than at Rauris. Dr. Pernter properly draws the conclusion that scintillation does not, in all cases and exclusively, take place in the lower air layers, and that many cases occur in which the air above 3,100 meters elevation is more productive of scintillation than that at lower levels. Pernter further concludes that little or nothing is gained in steadiness by building observatories at high elevations; but Exner considers the observations too few to warrant this generalization and considers that it is simply proved that the Sonnblick is not particularly suitable for an astronomical observatory. (*Meteorologische Zeitschrift*, 1889, VI, p. 30.)

Spectro-photometry.—M. Crova has presented a paper to the Paris Academy of Sciences on the analysis of the light diffused by the sky. He made observations on the top of Mont Ventoux with a modified form of his spectro-photometer, which could be directed to any part of the sky.

The curves for zenithal light show a predominance of the more refrangible radiations at sunrise, diminishing towards midday, then increasing towards sunset, but not reaching, in homologous hours after noon, the same values as in the morning. The figures show to what extent the light is bluer than the direct sunlight, and the light of the sky at Montpellier.

Polarization of sky-light.—Mr. J. C. McConnel has made observations with a polarimeter at St. Moritz, Thersis, and Davos, and derives the following results:

(1) The polarization of sky-light is weakest at midday, and is greater the nearer the sun to the horizon.

(2) Snow-covered ground diminishes polarization, and in general the brighter the ground is illuminated, the weaker the polarization.

(3) Polarization is greater at high altitudes than at sea-level. (*Phil. Mag.*, 1889, p. 81.)

Twilight phenomena and the Krakatoa eruption.—Prof. J. Kiessling has published the results of his studies on the twilight phenomena accompanying the Krakatoa eruption in a quarto volume of 169 pages, illustrated with colored plates, charts, and wood cuts.

The author first inquires in what particular the optical phenomena of 1883–'86 differ from ordinary twilight, and finds that it is essentially the intensity and frequent repetition which distinguish the one from the other. The extension of this optical phenomena over the world after the Krakatoa eruption is represented by four charts on which are entered the places and times of observation. Kiessling's experiments teach that the colored suns observed after the eruption are produced by dense clouds of smoke and dust, the single particles of which may be of quite different sizes. He was able to obtain all colors from reddish brown to violet, except the green colors were not pure, but appeared with a yellowish tint. Colored rings are seen around the sun only when the particles are approximately of equal size, and the more nearly equal the size, the brighter the coloring. This forces the conclusion that the rings are phenomena of diffraction. In the discussion of twilight phenomena, Kiessling separates the colored horizonal bands from the after-glows (purpurlicht). The latter are phenomena of the same kind as Bishop's ring; the former are due essentially to absorption. Of special interest is the question whether the particles that produced the abnormal phenomena were smoke, dust, or water. It is possible to exclude dust (staub) at once, since dust particles would not possess the necessary uniformity of size; and the assumption that such uniformity was attained by gradually falling is inadmissable, because the phenomena were seen immediately after the eruption. As between smoke and water particles it is not easy to descriminate, and probably both were engaged in the production of the phenomena.

Report of Royal Society on the Krakatoa Eruption.—The committee appointed by the Royal Society to report upon the eruption of Krakatoa have finished their work, and published a quarto volume of 490 pages, together with diagrams, charts, and colored plates. The report upon the optical phenomena following the eruption has been made by Mr. E. Douglas Archibald and Hon. Rollo Russell, and occupies over 300 pages. The following résumé of the report is taken from *Nature*. The different sections contain discussions of the following topics:

(1) The proximate cause of the abnormal twilights, and an explanation, as far as was possible, of the way in which they differed from ordinary twilights, both in quality and intensity.

(2) The colored suns, large corona round the sun and moon, and the sky-haze or eruption cloud which evidently caused them.

(3) The geographical distribution, the height and duration of the glows, a list of analogous phenomena on former occasions, opinions put forward to account for the present series, and finally, a general analysis of their connection with the eruptions of Krakatoa in detail, each in a separate section.

To give some idea of the principal facts and conclusions, we will commence with the abnormal twilights, considered as local phenomena.

A normal sunset consists of a series of bands of color parallel to the horizon in the west in the order from below upwards—red, orange, yellow, green, blue, together with a purplish glow in the east over the earth's shadow, called the "counter-glow." As the earth's shadow moves upwards towards the zenith, and passes invisibly across it, a reddish or purplish glow suddenly appears above the colored layers in the west, in a spot which previously appeared of a peculiarly bright whitish color. This purple glow is substantially the "primary glow," or more definitely "erste purpurlicht." It is peculiar in appearing above the horizontal colors, and in not extending far on either side of a vertical plane through the sun and the spectator. As this glow sinks on the horizon and spreads out laterally, it forms the first red sunset. After its disappearance, under favorable conditions, a second edition of twilight colors analagous to the first commences with a similar bright spot (dämmerungschein), out of which a second purple light appears to be suddenly developed, and sinks on the horizon as the secondary or "after-glow."

These are the normal phases of a complete sunset, according to Dr. von Bezold, and the present series appear to be abnormal only in exhibiting certain peculiar yellow and greenish tints, a less defined boundary of the earth's shadow, together with a much greater brilliancy, extension, and duration of the first, and particularly of the second, purple glows. The horizontal layers were less conspicuous than usual, and the abnormal extension of the purple light made it appear as though there was an inversion of the usual order of tints from below upwards.

In order to explain these and other peculiarities, Mr. Russell starts with the observed fact of a sky-haze which, in the tropics, tended to transmit blue or green rays in preference to red, and assuming that al- the usual elements which are included under the term "optical diffusion" were present, viz., diffraction, refraction, and reflection, describes what should be the effects, (1) assuming a haze composed of opaque particles, and (2) one composed of very thin reflecting plates into which condition a large proportion of the pumice ejected from Krakatoa is shown to have been transformed. His conclusion is that the distinctive features of the Krakatoa glows were due mainly to reflection from these fine laminæ, of rays already tinted in a certain order by diffraction through the dust of the haze layer and the lower atmosphere, as well as by the selective absorption which ordinarily takes place in the more humid horizontal layers near the earth's surface. The direct as well as diffuse reflection by the plates and by the opaque dust, (which lay, as Mr. Archibald has shown in Section IV, at a height of from 50,000 to 100,000 feet,) of rays tinted in succession, as both the direct and reflected twilight boundaries followed the descending sun, and the peculiar transmissive quality of the stratum for the more refrangible rays, appear to afford a reasonable explanation of the peculiar silvery glare, the unusual coloring, and the unusual extension of the purple glows.

It is admitted that diffraction played an important part, as it does in ordinary sunsets (Lommel, for example, attributes all the red tints to this cause); but both in this section and those that follow, many considerations are urged against the view held by Professor Kiessling that the development of the primary glow is chiefly due to diffraction, while the secondary glow is as confidently asserted to be due to reflection. One of the principal objections to the reflection hypothesis in explanation of both the ordinary, as well as the present extraordinary, development of the purple glow is its limitation at first to a narrow band, a fact which cannot be explained by absorption, and which is equally at variance with Fresnel's law of reflection from small globular dust, which would be equal in all directions. On the lamina, and particularly the vitreous lamina assumption however, it is intelligible, since the maximum reflection would then be like that from the sea, in the vertical plane through the sun and the eye.

Moreover, the richly-colored and prolonged secondary glows, which were the most characteristic feature of the Krakatoa twilights, are shown by Mr. Archibald, when dealing with their secular duration, to have reached a distinct minimum when the large diffraction corona round the sun, from Professor Ricco's observations, appeared at its greatest brilliancy, while the curve of their duration, representing Dr. Riggenbach and Mr. Clark's observations, shows that they never again reached the same brilliancy or duration as in the two or three months immediately succeeding their first appearance in Europe. Both these facts aid the conclusion arrived at by Mr. Russell and indorsed by Professor Kiessling, that they were reflections by the haze stratum of the primary glows. But if these were reflections, the question naturally arises, Why not the primary also? And until more effective arguments are brought against this view, as well as Professor Ricco's objections to Professor Kiessling's theory of diffraction alone, which are detailed in Section I (*c*), page 250, Mr. Russell's view of the origin of both glows seems to be the more probable, as well as reasonable, of the two. The haze stratum appears to have been capable of exerting two influences: One, diffraction of the sun's rays by its smallest particles, which, with the absorption and diffraction usually affected by the dust and vapor present in the lower atmosphere, caused the horizontal tinted layers; the other, reflection by its larger particles or laminæ of the horizontal layers, particularly of the lowest red one, when the earth's shadow had arrived at about 25° above the western horizon and into a position whence the maximum reflective effect could be seen unmasked by a diffusely illuminated background.

The question of the blue and green coloration of the sun is next discussed by Mr. Archibald, particularly with reference to its intrinsic characteristics and physical origin. In Section VII, in which the distribution of the twilight glows and the blue suns on their first circuit of the globe is compared, it is shown that the mean limit of the band of colored suns was about 11° north and south of the latitude of Krakatoa right around the equator, while that of the glows lay 5° beyond this on either side. Along the latitude of Krakatoa the colors were mostly white or silvery, and in one or two cases coppery. The colors thus evidently depended on the density of the stream, the glows appearing on its borders or fringes where it was less dense. A similar relation to density appears from a study of the diurnal changes with varying solar altitude, the sun appearing to change from blue near the zenith, through green or yellow, or disappearance on the horizon. No direct physical explanation of such phenomena appears forthcoming, since, according to the physical laws ennunciated by Lord Rayleigh and Professor Stokes, the diffraction of light by particles of the same order of magnitude as a wave length tends to sift out the shorter blue and preserve the longer red waves of light. Repeated reflections by small particles tend to the same result.

It can therefore only be explained as an effect of absorption, due to some particular absorptive property of the materials which composed the haze. The phenomenon of a blue or green sun has been observed under natural conditions, many of which are quoted, and in most cases where the air was filled with fine dust from a great variety of sources. It has also been artifically reproduced by Professor Kiessling with dust-filled air and vapor of water, and particularly of sulphur. Several accounts are given in section V of blue suns seen in connection with former eruptions, and Mr. Whymper's observations during an eruption of Cotopaxi are conclusive as to the ability of the finest volcanic ejecta to cause such an appearance. The problem which still awaits solution is, what was the precise nature of the particles or gases which produced the absorption? It seems probable that they were metallic sulphides.

Mr. Archibald next deals with the sky-haze and its peculiar effects, more particularly on astronomical definition. Here again it seems to have possessed a selective absorption of the red rays, for in two separate lunar eclipses, 1884 and 1885, the usual coppery tint of the moon was conspicuously absent. He then passes on to the peculiar large corona round the sun and moon, which was first observed by Mr. Bishop, at Honolulu, on September 5, and which, though less striking than the twilight glow, was, if anything, more uncommon, more constant, and more prolonged in duration. It was a true diffraction corona with a reddish border, and of almost exactly the same size as the ordinary ice-halo, viz, 45° in diameter. It lasted from September 5, 1883, up to October 15, 1886, since which date it has entirely disappeared. Its diameter has afforded an approximate determination of the mean radius of the smaller dust particles composing the haze, which Mr. Archibald calculates to be 0.00006 of an inch.

In section III, Mr. Russell works out the geographical distribution of the optical phenomena, including blue suns and glows, up to the end of 1883, by which time they had virtually covered the whole earth. The general conclusion is that the phenomena all propagated themselves (with the exception of a narrow offshoot towards Japan) at first due west from Java, at a rate of about 76 miles an hour right round the earth parallel to the equator, and in a band symmetrically disposed for 16° on either side of the latitude through Krakatoa. A second circuit with wider limits, 30° north and south of Krakatoa, was traced at the same rate, after which the motion became indistinguishable. They then gradually spread in latitude, and ultimately the haze which caused them appears to have invaded our latitudes, like the anti-trade, from southwest to northeast. These circumstances may be best realized from a survey of Mr. Russell's maps, especially that showing the successive limits of the appearances for the first 9 days succeeding the eruption. The march of the optical phenomena which is shown in Mr. Russell's maps is the only direct evidence we have of the fact that at 100,000 feet above the earth, in the immediate vicinity of the equator, the air in August, and probably, as Mr. Archibald shows, at other times, moves in a rapid and constant current from east to west. Both in section III (*b*), and section VII, he discusses this question in detail and shows its agreement with the theory of the general circulation of the atmosphere, as well as the motions of the upper clouds so far as they have been observed.

In section IV, Mr. Archibald investigates the height of the stratum, from observations in all parts of the world where the durations of the primary or secondary glow have been recorded with any attempt at accuracy. Proceeding on the hypothesis that the primary glow was a first reflection of the sun's rays by the stratum, and the secondary a reflection of the primary glow, for which ample evidence is adduced, he concludes that the height of the upper or middle part of the stratum above the earth diminished from 121,000 feet in August, 1883, to 64,000 in January, 1884, the lower limits being practically indeterminate. Also, since from Dr. Riggenbach's and Mr. Clark's observations, the glows continued less brilliantly and less prolonged after the first few months right up to the end of 1885, while a decided minimum in the duration, and, therefore, presumably the height, of the reflecting layer, was reached in April, 1884, the important conclusion is arrived at that by that date the larger and more effectively reflecting particles had descended to a lower level, leaving the finest particles suspended at nearly the same elevation as at first. This is further corroborated by the remarkable fact that the large corona reached its maximum intensity during the same month.

Finally, in section VII, Mr. Archibald gives a general analysis of the connection between all the optical phenomena and the eruptions of Krakatoa, both in May and August, in which the various objections on the ground of the initially rapid transmission of the appearances, insufficiency of fine, solid ejecta, length of time of its suspension, and the occurrence of apparently similar phenomena on dates previous to the great August eruption are discussed in turn. The time of suspension of the finest dust in particular is shown—by an application of Professor Stokes's formula, $V=\frac{2g}{9\mu'}\left(\frac{\sigma}{\rho}-1\right)a^2$, for the velocity of a small particle descending in air, and in which viscosity is properly considered—to be over two years between 50,000 and 100,000 feet, even assuming the particles to be spherical, which is the most unfavorable supposition. If, as is most probable, they were thin plates, the time would be much longer. A final summary is then given of the direct and local connection between the optical phenomena and the eruptions, both of May and August, which the subsequent discovery of the relative though minor importance of the May eruption rendered necessary.

Sunset glows.—Prof. Cleveland Abbe has published a paper on the sunset glows of 1884–'85 (written in November, 1885), in which he shows that the phenomena can not be produced by refraction and consequent dispersion through small drops, but are explicable only as diffraction effects in which the nature of the substance, whether minute drops of water or non-transparent particles of dust, is immaterial.

The Bishop's Ring is attributed to particles so far removed from the earth's surface as to remain sensibly permanent through many seasons, while the red twilights are diffraction rings due to similar and slightly larger particles in the lower atmosphere. In a prefatory note written February, 1889, Professor Abbe gives the following as his present conclusions:

(1) Vapor haze is more important than dust haze.

(2) A shallow layer, sparsely filled with such minute particles of vapor haze generally accompanies every area of high pressure and clear air, and appears to produce the diffraction necessary for the phenomena that are still observable.

(3) A deeper layer, more densely filled with minute and also with still larger particles suffices to explain the phenomena of 1883–'84.

(4) The dust and haze needed to produce red coloration of light by selective absorption and reflection is always present in the lowest air stratum.

(5) The Krakatoa eruption sufficed to throw sufficient moisture into the atmosphere to explain the diffractive phenomena of 1883–'84 and its gradual subsidence since then.

(6) The daily weather reports printed in the *Signal Service Bull. Int. Simul. Obs.* shows that the distribution of Krakatoa vapor must have been largely influenced by disturbances in the lower atmosphere, and we do not need to assume an exclusive influence of general upper currents, either easterly or westerly. (*Am. Meteor. Journal*, V, p. 529.)

Mr. S. E. Bishop, in a letter dated Honolulu, July 25, 1889, reports a re appearance, beginning on July 13, of sunset glows like those of 1883–'84, but of less brilliancy. The glows were brightest on the 14th and the 15th and were visible until the 20th, with decreasing intensity. A space of 15° radius around the sun was occupied by a whitish glow, like that in "Bishop's Ring." A noticeable peculiarity of the present glows is the occurrence of a tertiary glow in addition to the primary and secondary. Another difference is the much earlier time at which the glows take place, and the rapidity with which they follow each other, indicating that the reflecting stratum of haze is very low down as compared with the Krakatoa haze. The reflected rays of the sun, traversing a smaller extent of the lower atmosphere, show less red, having less of the other colors interrupted. For the same reason they retain force enough for a third reflection, in which a very pure, though faint, red appears. (*Nature*, XL, p. 415.)

Mr. J. W. Backhouse reports a feeble re-appearance over western Europe of a great corona around the sun during August and September, 1889. (*Nature*, XL, p. 519.)

Noctilucous clouds.—O. Jesse gives the following discription of the luminous night clouds that have been visible in Europe during the months of June and July since 1885. They are visible only in that portion of the evening or morning sky which is illuminated by the twilight and bounded by the twilight arc. These clouds disappear as soon as the twilight arc passes over them. In the evening the clouds appear when the sun is about 10° below the horizon, and continue visible throughout the duration of twilight. In the morning the phenomena are inverted. They are very similar to cirri in form and structure, but when an ordinary cirrus cloud is present it looks much darker than the twilight sky surrounding it, while luminous clouds are brighter. (*Meteorologische Zeitschrift*, 1889, VI, p. 184.)

Prof. John Le Conte discusses in *Nature* the origin and source of the light in noctilucous clouds, and refers to a collection of observations of this phenomenon made by Arago, and to his conclusion that the clouds are self-luminous. Professor Le Conte has observed on the coast of

Georgia a luminosity sufficient to plainly indicate the road to the traveller in instances when low-lying dense masses of clouds involved the whole firmament. In some cases the noctilucous condition may be caused by the prolonged twilights due to the reflection of sunlight from attenuated solid particles suspended in the supra-cirrus strata of the atmosphere, and in other cases may be traced to cloud-obscured auroral lights. Whether these sources of luminosity are sufficient to explain the various observed phenomena without supposing a condition of self-luminosity is still a matter of question.

Mr. D. J. Rowan, Dublin, reports luminous night clouds appearing between 10 P. M. and midnight, June 7, 1889, for the first time during the present year. He has found them for several years to be an annual phenomenon. (*Nature* XL, p. 151.)

XII.—PERIODICITY AND SUN SPOTS; HYDROLOGY; FORESTS AND CLIMATE; CLIMATES OF GEOLOGIC EPOCHS.

Sun-spot period in Indian weather.—Mr. Eliot in his last Meteorological Report for India, referring to sun spots and weather in India, says that the period of minimum sun spots is apparently associated with the largest and most abnormal variations of meteorological conditions. Thus exceptionally heavy snow-fell in the northwest Himalayas in 1866, and again in 1876 and 1877: the most disastrous famines of recent years in India have occurred near the period of minimum sun spots; and the largest and most intense cyclones apparently have a tendency to occur shortly before the minimum. For example, in the great Calcutta cyclone of 1864, 60,000 people were drowned, and in the still larger Backerganj cyclone of 1876, 100,000 lives were lost by drowning.

Hydrology in Galicia.—Annual tables of rain-fall and river heights in Galicia for 1887 and 1888, have been published (see bibliography) under the direction of Professor Karlinski, director of the Cracow observatory. The volume for 1887 contains daily observations of river heights at seventy-two stations and precipitation measures at one hundred and thirty-five stations; that for 1888, ninety-two river stations and one hundred and twenty-nine rain fall stations. The daily rain-fall tables are given only for June in 1887, and in 1888, for July, August, and September. Isohyetals are drawn presenting graphically the distribution of rain-fall. The tables furnish a valuable contribution of data for the study of the relation of surface and climatic conditions to the flow of streams.

Hydrology of the Saale.—Dr. Ule (Halle) has investigated the relation of the discharge of the Saale to the total precipitation over its watershed, as determined by reports from forty-five stations. He finds that for the period from 1883 to 1886 only 30 per cent. of the precipitation was discharged by the Saale. The total annual precipitation was 606 millimeters; no evaporation observations were made. (*Meteorologische Zeitschrift*, 1889, VI, p. 272.)

Under-ground waters.—The supervising engineers of the coal-mines in the lower Rhone basin have studied the relation of the flow of water in the mines, to the rain-fall. In the copious rains of October and November the rain-water sinks into the strata, following fractures and lines of erosion, and reaches the mine from twenty-four to thirty-six hours after the rain-fall. Areas of different geological structure show different periods of infiltration. In the mines of Fuveau and Gréasque the water enters in two periods; the first some hours after the end of the rain, proceeding from quite local infiltration, whilst the second, arriving some days later and continuing much longer, comes from more distant regions. (*Ibid.* p. 80.)

Commission météorologique du Department des Vosges; Observations faites en 1887–1888. Epinal, 1889.—In addition to a full summary of meteorological observations this report contains important phænological and hydrographic data. The rivers attain their flood heights in the winter months. The Moselle carries off about 48 per cent. of the precipitation that falls within its catchment basin. The united discharge of the Meuse and Mouson at Neufchateau is 47 per cent. of the rainfall; that of the Vair at Soulouse 35 per cent.; that of the Meuse at Maxey-sur-Meuse, 40 per cent. The Moselle rises on the average about 20 centimeters at Epinal, when the rain-fall in the upper part of the water-shed amounts to 1 centimeter. Monthly averages of precipitation at low level and mountain stations show the effect of elevation. The mean annual rain-fall at 320 meters elevation is 840 millimeters; at 450 meters is 1,347 millimeters, and at 750 meters elevation is 1,672 millimeters.

The hydrographic department of Russia has devoted, since 1837, a good deal of attention to the secular rising of the coasts of the Baltic Sea, and a number of marks have been made on the rocky coasts of the Gulfs of Bothnia and Finland in order to obtain trust-worthy data as to the rate of the upheaval of the coasts. Since 1869 observations have been carried on in a systematic way for measuring the changes in the level of the Baltic at several of these marks, and the results of the observations are now summed up by Colonel Mikhailoff, in the *Izvestia* of the Russ. Geographical Soc. XXIV, 3.

Taking only those stations at which the secular change could be determined (from observations from 1839 to 1878), the rise of the coast in a century would appear to be as follows: Aspo, 20.3 inches; Island of Kotkö, 26.7; Island of Skotland, 12.5; Hangöudd, 33.7; Island of Jussair, 31.6; Lehtö, 11.5.

Forest and climate.—Dr. H. E. Hamberg has issued Part III of his investigations on the relation of forests to climate in Sweden.

The following are his conclusions:

The excess of water supplied to the atmosphere by the forest vegetation, above that which would be supplied by the same area of bare soil, is certainly considerable, and if that amount of vapor remained over the forest or was restored to the soil in the form of rain, it would be of great utility, but the wind carries this vapor away and disperses it on all sides, so that its useful effect to our own country is scarcely, if at all, perceptible. Accordingly, the difference of absolute as well as of relative humidity between the cultivated patches in our extensive forest districts and the cultivated plains of our country is very slight or almost *nil.* It is true that relative humidity is greater under trees, and as the relation mentioned does not apply to open spaces surrounded by wood, this difference in humidity can have no practical importance.

The lakes and large swamps as well as marshes have a much greater influence on atmospheric humidity than the forests. The evaporation from the latter, for equal areas, is far less than from the former. The draining of lakes and swamps has not been regarded with serious alarm.

One effect of forests on atmospheric humidity which seems to be useful to vegetation and agriculture is the increase of dew in clearances; but this increase of dew is not attributable to a greater abundance of vapor in the forests, but to the increase of terrestrial radiation induced by the forest.

The agriculturists of Småland and of Jemtland have preferred bare, dry, elevated lands, exposed to wind, to fields at a lower level, wooded and moist, but more subject to frost.

If all the forests were cleared what would be the result for the atmospheric humidity in Sweden? Supposing that this clearance did not materially modify the quantity of rain that falls, and it is not proved that it would, it seems to us that the amount of vapor contained in the stratum of air in which we live would not be altered in a way which would materially influence vegetation. Probably relative humidity would be slightly reduced in summer, because temperature would rise slightly.

In Bulletin No. 2 of the Forestry Division of the Department of Agriculture, Mr. G. H. Parsons discusses the relation of the climate of Colorado to the growth of trees. The author finds that the great range of temperature, the warmth of the sun's rays in cold weather, the low humidity, small rain-fall, rapid evaporation, small cloudiness, and the northers and chinooks are unfavorable to tree growth. Even irrigation can only partially supply the tree with the moisture it needs, and can never give it the luxuriant foliage characteristic of moist climates.

Effects of forest destruction.—Mr. W. E. Abbott has observed that de-forestation in New South Wales has been followed by a more abundant flow of water in the streams; springs have broken out, dry water-courses have begun to flow, and the change is apparently permanent.—(*Journ. Roy. Soc. New South Wales*, XXII, p. 59.)

Dr. O. Birkner finds that in Saxony the forests interpose an obstacle to the rapid run-off of the rain-fall in heavy rains and thereby prevent floods in the river valleys. This result is effected in three different ways:

(1) The foliage of the trees catches a portion of the rain and holds it until evaporated.

(2) On steep slopes forests furnish a permeable covering of earth, which acts in a high degree as a protection against a rapid run-off and prevents the rapid and complete denudation of the surface covering itself.

Ruthless de-forestation in Lusatia has opened the way for disastrous floods. (*Meteorologische Zeitschrift*, 1889, VI, p. 261.)

Forests and rain-fall.—The annual report of the Commissioner of Agriculture for 1888 contains an interesting review by Dr. B. E. Fernow of the literature on forests and rain-fall. The numerous attempts to prove statistically the effect or the non-effect of forests in modifying the precipitation are shown to be inconclusive.

Forestry in Burmah.—The first annual report of the conservator of the forests of Upper Burmah shows that much has been done in a short time towards the protection of forests. A staff of eleven assistants has been employed, and in some cases escorts have protected these officers in their work. Fifty-seven people have been convicted of offenses against the forestry regulations, but serious loss has still resulted to the forest revenue from the plundering of unmarked timber by local traders.

Forestry in China.—By a government proclamation, well directed and determined efforts are to be put forth toward afforestation in China. China is a treeless country, and to this are perhaps due the devastating floods that have caused such repeated damage. Slight attempts have previously been made to plant extensive tracts with forest trees, but the strong northerly winds which prevail soon uprooted those that had not been planted to a sufficient depth nor in well-chosen places.

The methods now to be adopted are those of education in forest culture and local encouragement and reward for successful work. (*Nature*, XXXIX, p. 594.)

Climate of geologic epochs.—Dr. Neumayer, in a paper before the Society for the Extension of the Natural Sciences, in Vienna, argues against the theory of a uniform climate over the earth in any geological epoch. He shows that the occurrence of any given flora or fauna does not prove any definite climatic conditions, because plants are able to adapt themselves to different environment. Again the theory of a uniform flora over the earth in the carboniferous age can not now be admitted. The climate of Greenland and Grinnell Land since tertiary time has grown colder by an amount not much less than 30° C. Europe also shows an important cooling. But in the opposite hemisphere at the same latitudes the cooling since tertiary time has been strikingly less. In the miocene flora of Japan there is no sure evidence of a climate warmer than that of to-day and in the pliocene flora there are indications of a colder climate. These facts point to a change in the position of the earth's axis of rotation, and this supposition is confirmed by similar phenomena in the southern hemisphere. (*Meteorologische Zeitschrift*, 1889, p. [85].)

Mr. H. H. Howorth adduces a variety of evidence going to prove that Siberia during the mammoth age was possessed of a temperate climate and was probably occupied by forests to the borders of the Arctic Ocean. Schmidt and others have shown that rooted trunks of trees are found in the beds containing mammoth remains far north of the present range of trees.

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HOW RAIN IS FORMED.*

By H. F. BLANFORD, F. R. S.

In certain villages in the Indian Central Provinces, besides the village blacksmith, the village accountant, the village watchman, and the like, there is an official termed the *gàpogàri*, whose duty it is to make rain. So long as the seasons are good, and the rain comes in due season, his office is no doubt a pleasant and lucrative one. It is not very laborious, and it is obviously the interest of all to keep him in good humor. But if, as sometimes happens, the hot dry weather of April and May is prolonged through June and July, and week after week the *ryot* sees his young sprouting crops withering beneath the pitiless hot winds, public feeling is wont to be roused against the peccant rain-maker, and he is led forth and periodically beaten until he mends his ways and brings down the much-needed showers.

You will hardly expect me, and I certainly can not pretend, to impart to you the trade-secrets of the professional rain-maker. Like some other branches of occult knowledge which Madam Blavatsky assures us are indigenous to India, this art of rain-making is perhaps not to be acquired by those who have been trained in European ideas; but we can at least watch and interrogate nature, and learn something of her method of achieving the same end; and if her scale of operations is too large for our successful imitation, we shall find that not only is there much in it that may well challenge our interest, but it may enable us to some extent to exercise prevision of its results.

Stated in the most general terms, nature's process of rain-making is extremely simple. We have its analogue in the working of the common still. First, we have steam or water vapor produced by heating and evaporating the water in the boiler; then the transfer of this vapor to a cooler; and finally we have it condensed by cooling, and reconverted into water. Heat is communicated to the water to convert it into vapor, and when that heat is withdrawn from it, the vapor returns to its original liquid state. Nature performs exactly the same process.

In the still, the water is heated until it boils; but this is not essential, for evaporation may take place at all temperatures, even from ice. A common little piece of apparatus, often to be seen in the window of the philosophical instrument maker, and known as Wollaston's cryophorus, is a still that works without any fire. It consists of a large glass tube with a bulb at each end, one of which is partly filled with water; and, all the air having been driven out of the tube by boiling the water, it is hermetically sealed and allowed to cool. It then contains nothing but water and water vapor, the greater part of which re-condenses when it cools. Now, when thus cold, if the empty bulb be surrounded by ice, or, better, a mixture of ice and salt, the water slowly distils over, and is condensed in the colder bulb, and this without any heat being applied to that which originally contained the water. And this shows us that all that is necessary to distillation is that the condenser be kept cooler than the evaporator.

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* A lecture delivered at the Hythe School of Musketry on November 19, 1888.—(*Nature*, January 3, 1889, vol. XXXIX., pp. 224-229.)

Nevertheless, at whatever temperature it evaporates, water requires heat, and a large quantity of heat, merely to convert it into vapor; and this is the case with the cryophorus; for if the evaporating bulb be wrapped round with flannel, and so protected from sources of heat around, the water cools down until it freezes. That is to say, it gives up its own heat to form vapor. A simple experiment that any one may try with a common thermometer affords another illustration of the same fact. If a thermometer bulb be covered with a piece of muslin, and dipped into water that has been standing long enough to have the same temperature as the air, it gives the same reading in the water as in the air. But if when thus wetted it be lifted out and exposed to the air, it begins to sink at once, owing to the evaporation of the water from the wet surface, and it sinks the lower the faster it dries. In India, when a hot wind is blowing, the wet bulb sometimes sinks 40° below the temperature of the air.

Now this is a very important fact in connection with the formation of rain, because it is owing to the fact that water vapor has absorbed a large quantity of heat, (which is not sensible as heat, but must be taken away from it before it can be condensed and return to the liquid state,) that vapor can be transported as such by the winds for thousands of miles, to be condensed as rain at some distant part of the earth's surface.

I have said that the quantity of absorbed heat is very large. It varies with the temperature of the water that is evaporating, and is the greater the lower that temperature. From water that is on the point of freezing it is such that 1 grain of water absorbs in evaporating as much heat as would raise nearly 5½ grains from the freezing to the boiling point. This is called the latent heat of water vapor. As I have said, it is quite insensible. The vapor is no warmer than the water that produced it, and this enormous quantity of heat has been employed simply in pulling the molecules of water asunder and setting them free in the form of vapor, which is merely water in the state of gas. All liquids absorb latent heat when they evaporate, but no other known liquid requires so much as water.

Many things familiar in every one's experience find their explanation in this absorption of latent heat. For instance, we feel colder with a wet skin than with a dry one, and wet clothes are a fruitful source of chills when the body is in repose; although, so long as it is in active exercise and producing a large amount of heat, since the evaporation only carries off the excess, no ill consequence may ensue. Again, if a kettle be filled with ice-cold water and put on a gas stove, suppose it takes ten minutes to bring it to boil. In that ten minutes the water has absorbed as much heat as raises it from 32° to 212°, an increase of 180°. Now, if it be left boiling, the gas-flame being kept up at the same intensity, we may assume that in every succeeding ten minutes the same quantity of heat is being absorbed by the water. But it gets no hotter; it gradually boils away. And it takes nearly an hour, or more than five times as long as it took to heat it, before the whole of the water has boiled away, since all this heat has been used up in converting it into steam. It was by an experiment of this kind that Dr. Black in the last century discovered the fact of latent heat, and determined its amount; and it was the knowledge of this fact that led James Watt to his first great improvement in the steam-engine.

One more example I may give, which those who have been in India will be able to appreciate, and which those who intend to go there may some day find useful to know. Nothing is more grateful in hot dry weather than a drink of cold water. Now, ice is not always to be had, but when a hot wind is blowing nothing is easier than to get cold water, if you have a pot or bottle of unglazed earthenware, such as are for sale in every bazaar, or what is better, a leather water-bottle, called a *Chhàgal*, or a water-skin. All these allow the water to soak through and keep the outside wet, and if any one of them be filled with water and hung up in a hot wind in the course of half an hour or an hour the evaporation from the outside will have taken away so much heat that the contents may be cooled 20° or 30°, notwithstanding that the thermometer may stand at 110° or 115° in the shade. Sodawater may be cooled in the same way if wrapped in straw and kept well wetted while exposed to the wind. But it is of little use to do as I have seen natives do sometimes, viz, put the bottles into a tub of water in a closed room. It is the evaporation that carries off the heat, otherwise the water is no cooler than the air around.

Now to return to our subject. The atmosphere always contains some vapor which the winds have taken up from the ocean, lakes, rivers, and even from the land, for there are but few regions so dry and devoid of vegetation that there is no moisture to evaporate. The quantity of water thus evaporated from large water surfaces is a question of some importance to engineers, who have to take account of the loss from reservoirs and irrigation tanks, and a good deal of attention has been given to measure the amount lost by evaporation. In England it has been found to vary in different years from 17 to 27 inches in the year, or to say from 1½ to 2¼ inches per month on an average. Now, since in the east of England the rain-fall is only about 24 inches in the year, it follows that in that part of the Kingdom the loss by evaporation from a water surface is not very much less than the rain falling directly on the surface.

In dry countries the evaporation may exceed the local rain-fall. In the tropics it has been found to average from 3½ to 6 inches per month in the dry season. In the case of a large tank at Nagpur, constructed to supply the city with water, it was found that the loss by evaporation in the hottest and dryest weather was two and a half times as great as the quantity supplied for consumption.

These statistics will give some idea of the enormous evaporation that goes on from the water surfaces of the globe, and to this must be added all that takes place from the land. In the case of light showers nearly the whole of the rain is re evaporated, and probably on an average half of the total rain-fall on the land is thus lost sooner or later, leaving not more than half for the supply of springs and rivers.

The quantity of vapor in the air is very variable. To us, in England, the west and southwest winds are the dampest, coming direct from the Atlantic, and northeast winds are the driest. The cause of their extreme dryness I shall endeavor to explain presently. It is no doubt partly due to the fact that they reach us from the land surface of Europe, but partly also to another cause to which I shall have to advert later on.

The quantity of vapor in the air is usually ascertained by the hygrometer, the ordinary form of which is a pair of thermometers, one having the bulb wet, the other dry, and observing the depression of the wet bulb. The principle of this I have already explained. But the same thing may be ascertained more directly by passing a measured quantity of air through a light apparatus containing sulphuric acid, or some other substance that absorbs water vapor greedily, and weighing the whole before and afterwards. The increase of the second weighment gives the weight of water absorbed. By such means it has been ascertained that air at 60° can contain as much as 5¾ grains of vapor in each cubic foot, and that air at 80° can contain rather less than 11 grains in the same space. The quantity that air can hold increases therefore very rapidly with the temperature. But it is seldom that it contains this maximum amount, especially at the higher temperatures.

In order to condense any part of this vapor we must take away its latent heat. It is not sufficient merely to cool it till it reaches the temperature of condensation, but we have further to abstract 5½ times as much heat as would raise the condensed water from the freezing to the boiling point. Before however proceeding to consider how this cooling is effected, the question arises, What is the condensing point ? For obviously, since water can evaporate at all temperatures, so we should expect that it may condense at all temperatures. On what then does the condensing point depend?

I mentioned just now that air at the temperature of 60° can contain as much as 5¾ grains of vapor, and at 80° rather less than 11 grains in each cubic foot. Obviously then if air at 80°, containing this maximum quantity, be cooled to 60°, it must get rid of more than 5 grains, or nearly half its vapor, and this excess must be condensed. I speak of air containing these quantities, but in point of fact it makes no appreciable difference whether air be present or not. An exhausted glass vessel of one cubic foot capacity can hold 5¾ grains of vapor at 60° and no more, and nearly 11 grains at 80° and no more; and if, when thus charged at 80°, its contents be cooled to 60°, more than 5 grains will be condensed. If however it contain only 5¾ grains at 80°, none will condense until the temperature falls to 60°, but any further cooling produces some condensation. Thus then the condensing point depends on the quantity of vapor present in the air, and is the temperature at which this quantity is the maximum possible for that temperature.

This preliminary point being explained, we may now proceed to inquire what means Nature employs to condense the vapor in the air, producing at one time dew and hoar-frost, at another time fog and cloud, and at another, rain, hail, and snow.

Let us take the case of dew and hoar-frost first, as they are comparatively simple. And in connection therewith I may relate a little incident that took place at Calcutta some years ago. A gentleman, who had not much acquaintance with physical science, was sitting one evening with a glass of iced brandy and water before him. It was in the rainy season, when the air, though warm, is very damp, and he had a large lump of ice in his tumbler. On taking it up, he noticed to his surprise that the glass was wet on the outside, and was standing in quite a little pool of water on the table. At first he thought his tumbler was cracked, but putting his finger to his tongue he found the fluid tasteless. "Very odd," he remarked; "the water comes through the glass but the brandy doesn't."

Now however, with our present knowledge, we may be inclined to smile at the simplicity of this remark, it so happens that up to the end of the last century very much the same explanation was popularly held to account for dew. It was supposed to be a kind of perspiration emitted from the earth, and no satisfactory explanation of the phenomenon had been arrived at by the physical philosophers of the day. It remained for Dr. Wells to prove, by a long series of observations and experiments, which have been quoted by Sir John Herschel and Mr. John Stewart Mill as a typical instance of philosophical inquiry, that the cold surface of grass and shrubs condenses the vapor previously held in suspension in the air, these surfaces being cooler than the air, and below its point of condensation. And such of course is also the case of the glass tumbler containing ice. Any one may try the experiment for himself. To produce hoar-frost, it is only necessary to cool the condensing surface below the freezing point, which may be done by crushing some ice and mixing it with salt. A tin pot is better than a glass to make this experiment.

When not only the ground, but also the air to a considerable height above it, is cooled in like manner, we have the production of fog, fog being the form in which the vapor is first condensed, and consisting of water in drops too minute to be separately visible. The formation of fog is very much aided if the air be laden with smoke. Smoke consists of extremely minute particles of unburnt coal or other fuel, and these cool faster than the air at night, and so cool the air in contact with them. Each one of them, too, condenses water on its surface, and being thus weighted they sink and form that dense fog that Londoners know so well.

Clouds are essentially the same as fog, but formed high up in the air. But in their case, and that of rain, snow, and hail, another and different cooling agency comes into play, and this will require some preliminary explanation.

I dare say that some of you may at some time or other have charged an air-gun. And if so, you will be aware that when so charged the reservoir becomes pretty warm. Now this heat is produced, not, as might be supposed, by the friction of the piston in charging, but is due to the fact that work has been done upon the air by compressing it into a very small space; in other words, work has been converted into heat. If the compressed air be allowed to escape at once, its heat is reconverted into work. It has to make room for itself by thrusting aside the atmosphere into which it escapes, and when thus expanded it is no warmer than before it was compressed. Indeed, not so warm, for it will already have parted with some of its heat to the metal chamber which contained it. And if when compressed it is allowed to cool down to the ordinary temperature, and then to escape, it will be cooled below that temperature just as much as it was heated by compression. Thus, if in being compressed it had been heated 100°, say from 60° to 160°, and then allowed to cool to 60°, on escaping it will be cooled 100° below 60°, or to 40° below zero, which is the temperature at which mercury freezes. This is the principle of the cold air chambers now so extensively employed on ship-board for the transport of frozen provisions from New Zealand and Australia.

Bearing in mind, then, this fact—that air in expanding and driving aside the air into which it expands is always cooled,—let us see how this applies to the case before us, the production of cloud and rain.

The volume of a given weight of air—in other words, the space it occupies—depends on the pressure to which it is subject; the less this pressure the greater its volume. If we suppose the atmosphere divided into a number of layers superimposed on each other, the bottom layer is clearly subject to the pressure of all those that rest on it. This is equal to about 14¾ pounds on every square inch of surface. Another layer, say 1,000 feet above the ground, will clearly be under a less pressure, since 1,000 feet of air are below it; and this 1,000 feet of air weighs slightly less than half a pound for every square inch of horizontal surface. At 2,000 feet the pressure will be less by nearly 1 pound per square inch, and so on. If, then, any mass of air begins to ascend through the atmosphere it will be continually subject to less and less pressure as it ascends, and therefore, as we have already seen, it expands and becomes cooler by expansion. Cooling from this cause is termed dynamic cooling. Its rate may be accurately computed from the work it has to do in expanding.

It amounts to 1° for every 183 feet of ascent if the air be dry or free from vapor, and if, as is always the case, it contains some vapor, the height will not be very much greater so long as there is no condensation. But so soon as this point is passed, and the vapor begins to condense as cloud, the latent heat set free retards the cooling, and the height through which this cloud-laden air must ascend to cool 1° is considerably greater and varies with the temperature and pressure. When the barometer stands at 30 inches, and at the temperature of freezing, the air must rise 277 feet to lose 1°, and if the temperature is 60° nearly 400 feet.

Conversely, dry air descending through the atmosphere and becoming denser as it descends, since it is continually becoming subject to an increased pressure, is heated 1° for every 183 feet of descent; and fog and cloud-laden air at 30 inches of pressure and the freezing point will be warmed 1° in 277 feet only, or if at 60° nearly 400 feet of descent, owing to the re-evaporation of the fog or cloud and the absorption of latent heat.

Now, let us see how these facts explain the formation of cloud, and first I will take the case of the common cumulus or heap-cloud, which is the commonest cloud of the day-time in fine weather.

When after sunrise the air begins to be warmed, the lowest stratum of the atmosphere, which rests immediately on the ground, is warmed more rapidly than the higher strata. This is because the greater part of the sun's heat passes freely through a clear atmosphere without warming it, and is absorbed by the ground, which gives it out again to the air immediately in contact with it. So soon as the vertical decrease of temperature exceeds 1° in 183 feet the warm air below begins to ascend, and the cooler air above to descend, and this interchange gradually extends higher and higher, the ascending air being gradually cooled by expansion, and ceasing to rise when it has fallen to the same temperature as the air around it. This ascending air is more highly charged with vapor than that which descends to replace it, since, as was mentioned before, most land surfaces furnish a large amount of moisture, which evaporates when they are heated by the sun. This process goes on until some portion of the ascending air has become cooled to the point of condensation. No sooner does it attain this than a small tuft of cumulus cloud appears on the top of the ascending current, and the movement which was invisible before now becomes visible. In a calm atmosphere each tuft of cloud has a flat base, which marks the height at which condensation begins, but it is really only the top of an ascending column of air. No sooner is this cloud formed than the ascent becomes more rapid, because the cooling which checked its further ascent now takes place at a much slower rate, and therefore the cloud grows rapidly.

On a summer afternoon when the air is warm and very damp such cumulus cloud ascends sometimes to very great heights and develops into a thunder-cloud, condensing into rain. Rain differs from fog and cloud only in the size of the water drops. In fog and cloud these are so minute that they remain suspended in the air. But as the cloud becomes denser a number of them coalesce to form a rain-drop, which is large enough to overcome the friction of the air. It then begins to fall, and having to traverse an enormous thickness of cloud below it grows larger and larger by taking up more and more of the cloud corpuscules, so that when finally it falls below the cloud it may have a considerable size.

Such then is the mode in which rain is formed in an ordinary summer shower; and the more prolonged rain-fall of stormy wet weather is the result of a similar process, viz, the ascent and dynamic cooling of the moist atmosphere. But in this case the movement is on a far larger scale, being shared by the whole mass of the atmosphere, it may be, over hundreds or thousands of square miles; and to understand this movement we shall have to travel somewhat farther afield, and to inquire into the general circulation of the great atmospheric currents set in movement by the sun's action in the tropics, and modified by the earth's diurnal rotation and the distribution of the continents and oceans on its surface.

Before however entering on this subject, which will require some preliminary explanation, and in which we shall have to take account both of ascending and descending currents on a large scale, I will draw your attention to another and simpler case, in which both these classes of movements are prominently illustrated, and in which they exhibit their characteristic features in a very striking manner.

In the valleys of the Alps, more especially those to the north of the central chain, in Switzerland and the Tyrol, there blows from time to time a strong, warm, dry wind, known as the Föhn. It blows down the valleys from the central chain, melting the snows on its northern face, and although there is more or less clear sky overhead, all the southern slopes of the mountains are thickly clouded, and heavy rain falls on the lower spurs and the adjacent plain, replaced by snow at the higher levels up to the passes and the crest of the range. Cloudy weather also prevails to the north in Germany, and the weather is stormy over some part of western Europe.

It is only since the general introduction of telegraphic weather reports and the construction of daily weather charts, have enabled us to take a general survey of the simultaneous movements of the atmosphere over the greater portion of Europe, that this Föhn wind has been satisfactorily explained.* It is found that when a Föhn wind blows on the north of the Alps, the barometer is low somewhere to the north or northwest in Germany, northern France, or the British Isles, and high to the southeast in the direction of Greece and the eastern Mediterranean. Under these circumstances, since the winds always blow from a place of high barometer to one of low barometer, a strong southerly wind blows across the Alps. On their southern face it is forced to ascend, and therefore, as just explained, it is cooled and gives rain in Lombardy and Venetia, and snow at higher elevations. But having reached the crest of the mountains, it descends to the northern valleys, and being by this time deprived of a large part of its vapor, it becomes warmed in its descent, owing to compression, absorbs and re-evaporates the cloud carried with it, and is then further warmed at the rate of 1° for every 183 feet of descent. Thus it reaches the lower levels as a warm, dry wind, its warmth being the effect of dynamic heating.

Other mountain chains afford examples of the same phenomenon. A very striking instance, which much impressed me at the time, is one that I witnessed many years ago in the mountains of Ceylon; and it was afterwards mentioned to me by Sir Samuel Baker, who had been equally struck by it. My own experience is as follows: In June, 1861, I paid a week's visit to the hill sanitarium of Newara Eliya, at an elevation of 6,200 feet, on the western face of Pedro Talle Galle, the highest mountain in the island. The southwest monsoon was blowing steadily on this face of the range; and during the whole time of my stay it rained (as far as I am aware) without an hour's intermission, and a dense canopy of cloud enveloped the hill face, and never lifted more than a few hundred feet above the little valley in which Newara Eliya is built. But on leaving the station by the eastern road that leads across the crest of the range to Badulla, at a distance of 5 miles one reaches the *col* or dip in the ridge near Hackgalle, and thence the road descends some 2,000 feet to a lower table-land which stretches away many miles to the east. No sooner is this point passed than all rain ceases and clouds disappear, and one looks down on the rolling grassy hills bathed in the sunshine of a tropical sun, and swept by the dry westerly wind that descends from the mountain ridge. In little more than a mile one passes from day-long and week-long cloud and rain to constant sunshine and a cloudless sky.

As an almost invariable rule, or at least one with few exceptions, ascending air-currents are those that form cloud and rain, and descending currents are dry and bring fine weather. And this holds good whatever may be the immediate cause of these movements. We may now proceed to consider these greater examples to which I have already referred.

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* The explanation was originally given by Prof. J. Hann, of Vienna.

In the great workshop of nature, in so far at least as concerns our earth, with but few exceptions, all movement and all change, even the movements and energies of living things, proceed either directly or indirectly from the action of the sun. Nowhere is this action more direct and more strikingly manifested than in the movements of the atmosphere. Were the sun extinguished, and to become, as perhaps it may become long ages hence, a solid cold sphere, such as Byron imagined, "wandering darkling in eternal space,"a few days would suffice to convert our mobile and ever-varying atmosphere into a stagnant pall, devoid of vapor, resting quiescent on a lifeless earth, held bound in a more than Arctic frost. From such a consummation, despite the supposed decaying energy of our sun, we may however entertain a reasonable hope that we are yet far distant.

Bearing in mind the all-embracing importance of the sun, let us see how the great movements of the atmosphere are determined by the way in which the earth presents its surface to the solar rays.

Since the quantity of solar heat received on each part of the earth's surface depends on the directness or obliquity of his rays—in other words, on the height to which the sun ascends in the heavens at noon—being greatest where he is directly overhead, as in summer in the tropics, it follows that the hottest zone of the earth is that in the immediate neighborhood of the equator, and the coldest those around the poles.

Did time allow, and were the necessary appliances at hand, it would be easy to show you that both as a matter of experiment, and also as a deduction from physical laws, there must be under such circumstances a flow of air from the colder to the warmer region in the lower atmosphere, and a return current above. And to a certain extent we have these constant winds prevailing for about 30° on either side of the equator in the trade-winds, which blow towards the equator in the lower atmosphere, and the anti-trades blowing in the opposite direction at a great height above the earth's surface.

In the neighborhood of the equator there is a zone extending right round the earth in which the barometer is lower than either to the north or the south. It is due to the greater heat of the sun, and it is towards this that the trade-winds blow. It shifts to some extent with the seasons, being more northerly in the summer of the northern hemisphere, and more southerly in that of the southern hemisphere; and its average position is rather to the north of the equator, owing to the fact that there is more land in the northern than in the southern hemisphere, and that land is more heated by the sun than the ocean.

This simple wind system of the trades and anti-trades does not extend right round the earth, nor beyond 30° or 40° of latitude in either hemisphere. Were the earth's surface uniformly land or uniformly water, there probably would be a system of trade-winds all round the globe, blowing from both hemispheres towards the equator; but even in that case they would not extend much, if at all, beyond their present limits. In the first place, every great mass of land sets up an independent system of air currents, since the land is hotter than the ocean in the summer and colder in the winter. In the summer, therefore, there is a tendency to an indraught of air from the sea to the land in the lower atmosphere, and an outflow above, and in the winter the opposite; and this tendency modifies or interrupts the system of the trades and anti-trades. We have this tendency shown most distinctly in the monsoons of southeastern Asia, where, both in the India and China seas, a southwest wind in the summer takes the place which in the absence of the Asiatic continent would be held by a northeast trade-wind. And it is only in the winter that a northeast wind blows, and this is then termed the northeast monsoon.

In the second place, as I have said, the system of trade-winds could not in any case extend far beyond their present limits in latitude, owing to the fact that the earth is a sphere and not a cylinder. Let us fix our attention for a moment on the anti-trades—the upper winds which blow from the equator towards the poles. The equator, from which they start, is a circle about 24,900 miles in circumference; the poles are mere points, and, therefore, the whole of the air that blows towards the poles must turn back in any case before it reaches the pole, and must begin to turn back before it has gone very far on its journey. And, as a fact, a great part of it does turn back between 30° and 40° of latitude, which I have already mentioned as being the limit of the trade-winds. A part of the remainder descends to the earth's surface, and sweeps the Northern Atlantic and the North Pacific as a southwest wind.

On the chart which represents the average distribution of atmospheric pressure in January, there are two somewhat interrupted zones of high pressure over the ocean in these latitudes. These mark the regions in which the anti-trades descend to the earth's surface, and from which the trade-winds start. Over the ocean in all higher latitudes, both in the northern and southern hemispheres, the barometer is low—for the most part, indeed, much lower than over the equator; and the region intervening between the zones of high pressure and the seat of lowest pressure is that of predominant southwest, or at all events westerly winds. Since our islands are situated on the border of this region of low pressure, southwest are our prevailing winds.

But now two questions arise: First, why are these winds westerly, and not simply south winds? And second, how is it that the barometer is so low over the North Atlantic and North Pacific oceans, and also in the southern hemisphere in high latitudes, seeing that in these latitudes, at least in winter, the sun's heat is so much less than at the tropics? The chart represents the state of things in midwinter of the northern hemisphere, and yet everywhere to the north of latitude 40° the deep blue tint indicates that the pressure is lower than even in the southern tropic, where the sun shines vertically overhead. Clearly this low pressure must be due to some other cause than the warmth of the air.

The explanation of this remarkable distribution of the atmospheric pressure, of the existence of two zones of high pressure in latitudes 30° to 40°, and of very low pressure in higher latitudes, except in so far as they are modified by the alternations of land and water, was first given by the American physicist, Professor Ferrel. Its full demonstration is to be obtained only from the consideration of somewhat recondite mechanical laws, but a general idea of the causes operating may be gathered from very simple considerations, which may be demonstrated with a terrestrial globe.

Starting with the well-known fact that the earth revolves on its axis once in the twenty-four hours, let us see what will be the consequence, if we suppose a mass of any ponderable matter—that is, any substance having weight, no matter whether light or heavy—to be suddenly transferred from the equator to latitude 60°.

As the circumference of the earth at the equator is about 24,900 miles, anybody whatever, apparently at rest at the equator, is carried round the earth's axis at the rate of 1,036 miles an hour. But in latitude 60°, where the distance from the axis is only half as great as at the equator, it is carried round at only half the same rate, or 518 miles an hour; and at the pole it simply turns round on its own axis. Supposing, then, a mass of air to be suddenly transferred from the equator to latitude 60°, with the eastward movement that it had at the equator, it would be moving twice as fast to the east as that part of the earth, and, to any person standing on the earth, would be blowing from the west with a force far exceeding that of a hurricane. It would be moving eastwards 518 miles an hour faster than the earth. Indeed, its movement would really be far greater than this. In virtue of a mechanical principle known as the law of the conservation of areas, which means that anybody revolving round a central point, under the influence of a force that pulls it towards that point, describes equal areas in equal times, instead of only 518 miles, it would be revolving round the earth's axis 1,554 miles an hour faster than that part of the earth. I need not, however, specially insist on this point, because, as a matter of fact, the air which constitutes the anti-trades is not suddenly transferred, but takes a day or two to perform its journey, and in the meantime by far the greater part of its eastward movement is lost by friction against the trade-wind which blows in the opposite direction underneath it. The point on which we have to fix our attention, is that when the anti-trades descend to earth, they still retain some of their eastward movement, and blow, not as south, but as south-west or west-southwest winds.

On the other hand, the trade-wind, which blows towards the equator, is coming from a latitude where the eastward movement is less than at the equator, and its own movement eastward is therefore less than that of the surface over which it blows. A person, therefore, standing on the earth, is carried eastward faster than the air is moving, and the wind seems to blow against him from the northeast. Similarly, to the south of the equator, the trade-wind, instead of blowing from the south, comes from the southeast.

Thus then we have in both hemispheres a system of westerly winds in all higher latitudes than 40°, and a system of easterly winds—viz., the trade-winds—between about 30° and the equator; and if the globe were either all land or all water, these systems would prevail right round the earth.

Now, it is the pressure of these winds, under the influence of centrifugal force, that causes the two zones of high barometer in latitudes 30° to 40°, and the very low pressure in higher latitudes. It is not difficult to understand how this comes about. You are probably aware that the earth is not an exact sphere, but what is termed an oblate spheroid—that is, it is slightly flattened at the poles and protuberant at the equator, the difference of the equatorial and polar diameters being about 26 miles. It has acquired this form in virtue of its rotation on its axis. If you whirl a stone in a sling, the stone has a tendency to fly off at a tangent, and so long as it is retained in the sling that tendency is resisted by the tension of the cord. In the same way, every object resting on the earth, and the substance of the earth itself, has a tendency to fly off at a tangent, in consequence of its rotation on its axis, and this tendency is resisted and overcome by gravity. Were the earth not revolving, its form under the influence of gravity alone would be a true sphere. If it were to revolve more rapidly than at present, it would be still more oblate, flatter at the poles, and more bulging in the tropical zone; if less rapidly, the flattening and bulging would be less.

This is precisely what happens with the west and east winds of which we have spoken. West winds are revolving faster than the earth, and tend to make the atmosphere more protuberant at the equator than the solid earth; hence they press towards the equator, to the right of their path in the northern hemisphere, and the tendency increases rapidly in high latitudes. Easterly winds, on the other hand, tend to render the form of the atmosphere more nearly spherical, and they, too, press to the right of their path in the northern hemisphere or towards the pole. In the southern hemisphere, for the same reason, both press to the left. The result of these two pressures in opposite directions is to produce the two zones of high barometer in the latitudes in which we find them—viz, between the easterly trade-winds and the westerly winds, which are the anti-trades that have descended to the earth's surface. And the low barometer of higher latitudes is produced in like manner by the westerly winds pressing away from those regions.

Thus then we find that all this system of winds, and the resulting distribution of atmospheric pressure as indicated by the barometer is the result of the sun's action in equatorial regions. It is this that gives the motive power to the whole system, so far as we have as yet traced it, and it is this that produces those great inequalities of atmospheric pressure that I have so far described.

It remains now to see how storms are generated by these westerly winds. In so far as they retain any southing, they are still moving towards the pole in the northern hemisphere; that is to say, they are advancing from all sides towards a mere point. Some portion of them must therefore be continually turning back as the circles of latitude become smaller and smaller. But they are now surface-winds, and in order so to return they must rise and flow back as an upper current. This they do by forming great eddies, or air-whirls, in the center of which the barometer is very low, and over which the air ascends, and these great air-whirls are the storms of the temperate zone and of our latitudes. It is the ascent and dynamic cooling of the air in these great eddies that cause the prolonged rain-fall of wet stormy weather. How the eddies originate, or rather what particular circumstance causes them to originate in one place rather than another, we can scarcely say, any more than we can say how each eddy originates in a rapidly-flowing deep river. Some very small inequality of pressure probably starts them, but when once formed, they often last for many days, and travel some thousands of miles over the earth's surface.

Two such storms are represented on the charts of February 1 and 2, 1883, one on the coast of Labrador, the other to the southwest of the British Isles. The first of these appears on the chart of January 28, in the North Pacific, off the coast of British Columbia. On the 29th it had crossed the Rocky Mountains, and was traversing the western part of the Hudson's Bay Territory. On the 30th it had moved to the southeast, and lay just to the west of the Great Lakes, and on the 31st between Lake Superior and Hudson's Bay. On February 1 it had reached the position on the coast of Labrador shown in the chart, and on the 2nd had moved further to northeast, and lay across Davis's Straits, and over the west coast of Greenland. After this it again changed its course to southeast, and on February 4 passed to the north of Scotland, towards Denmark, and eventually on to Russia.

The second storm had originated off the east coast of the United States between January 28 and 29, and on the following days crossed the Atlantic on a course somewhat to north of east, till, on February 2, it lay over England.

These storms always move in some easterly direction, generally between east and northeast, and often several follow in rapid succession on nearly the same track. It is this knowledge that renders it possible for the Meteorological Office to issue the daily forecasts that we see in the newspapers. Were it possible to obtain telegraphic reports from a few stations out in the North Atlantic, these storm warnings could be issued with much more certainty, and perhaps longer before the arrival of the storm than at present. In the case of such storms as that which reached our islands on February 2, we often have such warnings from America, but their tracks are often more to the northeast, in the direction of Iceland, in which case they are not felt on our coasts, and hence the frequent failure of these American warnings.

It is the region of low pressure in the North Atlantic that is the especial field of these storms. As they pass across it, they produce considerable modifications in the distribution of pressure, but some of its main features remain outstanding. Thus there is always a belt of high barometer between the storm region and the trade-winds, and in the winter there is almost always a region of high barometer over North America, and another over Europe and Asia, however much they may shift their places, and be temporarily encroached on by the great storm eddies.

These regions of high pressure are the places where the winds descend, and, as I mentioned in the earlier part of this lecture, these winds are dry, and generally accompany fine weather. On the contrary, the eddies, where the air ascends, are damp and stormy, and especially that part of the eddy that is fed by the southwest winds that have swept the Atlantic since their descent, and so have become charged with vapor.

And now we are prepared to understand why east, and especially northeast winds are generally so dry. They are air that has descended in the area of high barometer that (especially in the winter and spring) lies over Europe and Asia, and has subsequently swept the cold land-surface, which does not furnish much vapor, and therefore they reach us as dry cold winds. To begin with, the air comes from a considerable height in the atmosphere, and in ascending to that height in some other part of the world, it must have got rid of most of its vapor in the way that has been already explained. In descending to the earth's level it must, of course, have been dynamically heated by the compression it has undergone, but all or nearly all this heat has been got rid of by radiation into free space on the cold plains and under the clear frosty skies of Northern Asia and Northern Europe, and it then blows outwards from this region of high barometer over the land, towards the warmer region of low barometer on the North Atlantic Ocean.

Thus we see that, in all cases, rain is produced by the cooling of the air, and that in nearly all, if not all, this cooling is produced by the expansion of the air in ascending from lower to higher levels in the atmosphere, by what is termed dynamic cooling. This last fact is not set forth so emphatically as it should be in some popular text-books on the subject, but it is an undoubted fact. It was originally suggested by Espy some forty years ago, but the truth is only now generally recognized, and it is one of the results which we owe to the great advance in physical science effected by Joule's discovery of the definite relation of equivalence between heat and mechanical work.

ON AËRIAL LOCOMOTION.*

By F. H. Wenham.

The resistance against a surface of a defined area, passing rapidly through yielding media, may be divided into two opposing forces; one arising from the cohesion of the separated particles and the other from their weight and inertia, which, according to well-known laws, will require a constant power to set them in motion.

In plastic substances the first condition, that of cohesion, will give rise to the greatest resistance. In water this has very little retarding effect, but in air, from its extreme fluidity, the cohesive force becomes inappreciable, and all resistances are caused by its weight alone; therefore, a weight suspended from a plane surface, descending perpendicularly in air, is limited in its rate of fall by the weight of air that can be set in motion in a given time.

If a weight of 150 pounds is suspended from a surface of the same number of square feet, the uniform descent will be 1,300 feet per minute, and the force given out and expended on the air, at this rate of fall, will be nearly six horse-power; and, conversely, this same speed and power must be communicated to the surface to keep the weight sustained at a fixed altitude. As the surface is increased so does the rate of descent and its accompanying power, expended in a given time, decrease. It might therefore be inferred that, with a sufficient extent of surface reproduced, or worked up to a higher altitude, a man might by his exertions raise himself for a time, while the surface descends at a less speed.

A man in raising his own body, can perform 4,250 units of work, (that is, this number of pounds raised 1 foot high per minute,) and can raise his own weight (say 150 pounds) 22 feet per minute. But at this speed the atmospheric resistance is so small that 120,000 square feet

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[*] A paper read before the Aëronautical Society of Great Britain, June 27, 1866, "On Aërial Locomotion, and the Laws by which Heavy Bodies Impelled through the Air, are Sustained." (From the *Transactions of the Aëronautical Society*. First Annual Report for the year 1866, pp. 10–40.) Notwithstanding its date, this paper contains so good a presentation of the problem of aëronautics, that it deserves a wider circulation than it has received.

would be required to balance his exertions, making no allowance for weight beyond his own body.

We have thus reasons for the failure of the many mis-directed attempts that have from time to time been made to raise weights perpendicularly in the air, by wings or descending surfaces. Though the flight of a bird is maintained by a constant re-action or abutment against an enormous weight of air in comparison with the weight of its own body, yet, as will be subsequently shown, the support upon that weight is not necessarily commanded by great extent of wing-surface, but by the direction of motion.

One of the first birds in the scale of flying magnitude is the pelican. It is seen in the streams and estuaries of warm climates, fish being its only food. On the Nile, after the inundation, it arrives in flocks of many hundreds together, having migrated from long distances A specimen shot was found to weigh 21 pounds and measured 10 feet across the wings from end to end. The pelican rises with much difficulty, but once on the wing appears to fly with very little exertion, notwithstanding its great weight. Their mode of progress is peculiar and graceful. They fly after a leader in one single train. As he rises or descends so his followers do the same in succession, imitating his movements precisely. At a distance this gives them the appearance of a long, undulating ribbon, glistening under the cloudless sun of an oriental sky. During their flight they make about seventy strokes per minute with their wing. This uncouth-looking bird is somewhat whimsical in its habits. Groups of them may be seen far above the earth, at a distance from the river-side, soaring, apparently for their own pleasure. With outstretched and motionless wings they float serenely high in the atmosphere for more than an hour together, traversing the same locality in circling movements. With head thrown back and enormous bills resting on their breasts they almost seem asleep. A few easy strokes of their wings each minute, as their momentum or velocity diminishes, serves to keep them sustained at the same level. The effort required is obviously slight and not confirmatory of the excessive amount of power said to be requisite for maintaining the flight of a bird of this weight and size. The pelican displays no symptom of being endowed with great strength, for when only slightly wounded it is easily captured, not having adequate power for effective resistance, but heavily flapping the huge wings that should, as some imagine, give a stroke equal in vigor to the kick of a horse.

During a calm evening flocks of spoonbills take their flight directly up the river's course, as if linked together in unison and moved by the same impulse, they alter not their relative positions, but at less than 15 inches above the water's surface, they speed swiftly by with ease and grace inimitable, a living sheet of spotless white. Let one circumstance be remarked,—though they have fleeted past at a rate of near 30 miles an hour, so little do they disturb the element in which they move that not a ripple of the placid bosom of the river, which they almost touch, has marked their track. How wonderfully does their progress contrast with that of creatures who are compelled to drag their slow and weary way against the fluid a thousand-fold more dense, flowing in strong and eddying current beneath them.

Our pennant drops listlessly, the wished-for north wind cometh not. According to custom we step on shore, gun in hand. A flock of white herons, or "buffalo-birds," almost within our reach, run a short distance from the pathway as we approach them. Others are seen perched in social groups upon the backs of the apathetic and mud-begrimed animals whose name they bear. Beyond the ripening *dhourra* crops which skirt the river-side, the land is covered with immense numbers of blue pigeons, flying to and fro, in shoals, and searching for food with restless diligence. The musical whistle from the pinions of the wood-doves sounds cheerily as they dart past with the speed of an arrow. Ever and anon are seen a covey of the brilliant, many-colored partridges of the district, whose long and pointed wings give them a strength and duration of flight that seems interminable, alighting at distances beyond the possibility of marking them down, as we are accustomed to do with their plumper brethren at home. But still more remarkable is the spectacle which the sky presents. As far as the eye can reach it is dotted with birds of prey of every size and description. Eagles, vultures, kites, and hawks of manifold species, down to the small, swallow-like, insectivorous hawk common in the Delta, which skims the surface of the ground in pursuit of its insect prey. None seem bent on going forward, but all are soaring leisurely round over the same locality, as if the invisible element which supports them were their medium of rest as well as motion. But mark that object sitting in solitary state in the midst of yon plain; what a magnificent eagle! An approach to within 80 yards arouses the king of birds from his apathy. He partly opens his enormous wings, but stirs not yet from his station. On gaining a few feet more he begins to *walk* away, with half-expanded but motionless wings. Now for the chance, fire! A charge of No. 3 from 11-bore rattles audibly but ineffectively upon his densely feathered body; his walk increases to a run, he gathers speed with his slowly-waving wings, and eventually leaves the ground. Rising at a gradual inclination, he mounts aloft and sails majestically away to his place of refuge in the Libyan range, distant at least 5 miles from where he rose. Some fragments of feathers denote the spot from where the shot had struck him. The marks of his claws are traceable in the sandy soil, as, at first with firm and decided digs, he forced his way, but as he lightened his body and increased his speed with the aid of his wings, the imprints of his talons gradually merged into long scratches. The measured distance from the point where these vanished to the place where he had stood, proved that with all the stimulus that the shot must have given to his exertions he had been compelled to run full 20 yards before he could raise himself from the earth.

Again the boat is under weigh, though the wind is but just sufficient for us to stem the current. An immense kite is soaring overhead, scarcely higher than the top of our lateen yard, affording a fine opportunity for contemplating his easy and unlabored movements. The cook has now thrown overboard some offal. With a solemn swoop the bird descends and seizes it with his talons. How easily he rises again with motionless expanded wings, the mere force and momentum of his descent serving to raise him again to more than half-mast high. Observe him next, with lazy flapping wings, and head turned under his body; he is placidly devouring the pendent morsel from his foot, and calmly gliding onwards.

The Nile abounds with large aquatic birds of almost every variety. During a residence upon its surface for nine months out of the year, immense numbers have been seen to come and go, for the majority of them are migratory. Egypt being merely a narrow strip of territory, passing through one of the most desert parts of the earth and rendered fertile only by the periodical rise of the waters of the river, it is probable that these birds make it their grand thoroughfare into the rich district of Central Africa.

On nearing our own shores, steaming against a moderate head-wind, from a station abaft the wheel the movements of some half-dozen gulls are observed, following in the wake of the ship in patient expectation of any edibles that may be thrown overboard. One that is more familiar than the rest comes so near at times that the winnowing of his wings can be heard; he has just dropped astern, and now comes on again. With the axis of his body exactly at the level of the eyesight, his every movement can be distinctly marked. He approaches to within 10 yards, and utters his wild, plaintive note, as he turns his head from side to side, and regards us with his jet black eye. But where is the angle or upward rise of his wings, that should compensate for his descending tendency, in a yielding medium like air? The incline can not be detected, for, to all appearance, his wings are edgewise, or parallel to his line of motion, and he appears to skim along a solid support. No smooth-edged rails, or steel-tired wheels, with polished axles revolving in well-oiled brasses, are needed here for the purpose of diminishing friction, for nature's machinery has surpassed them all. The retarding effects of gravity in the creature under notice, are almost annulled, for he is gliding forward upon a frictionless plane. There are various reasons for concluding that the direct flight of many birds is maintained with a much less expenditure of power for a high speed, than by any mode of progression.

The first subject for consideration is the proportion of surface weight, and their combined effect in descending perpendicularly through the atmosphere. The datum is here based upon the consideration of safety, for it may sometimes be needful for a living being to drop passively, without muscular effort. One square foot of sustaining surface for every pound of the total weight, will be sufficient for security.

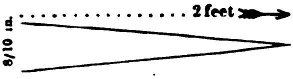
According to Smeaton's table of atmospheric resistance, to produce a force of 1 pound on a square foot, the wind must move against the plane (or, which is the same thing, the plane against the wind), at the rate of 22 feet per second, or 1,320 feet per minute, equal to 15 miles per hour. The resistance of the air will now balance the weight on the descending surface, and consequently it can not exceed that speed. Now 22 feet per second is the velocity acquired at the end of a fall of 8 feet,—a height from which a well-knit man or animal may leap down without much risk or injury. Therefore, if a man with parachute weigh together 143 pounds, spreading the same number of square feet of surface contained in a circle 14½ feet in diameter, he will descend at perhaps an unpleasant velocity, but with safety to life and limb.

It is a remarkable fact how this proportion of wing-surface to weight extends throughout a great variety of the flying portion of the animal kingdom, even down to hornets, bees, and other insects. In some instances however, as in the gallinaceous tribe, including pheasants, this area is somewhat exceeded, but they are known to be very poor flyers. Residing as they do chiefly on the ground, their wings are only required for short distances or for raising them or easing their descent from their roosting-places in forest trees, the shortness of their wings preventing them from taking extended flight. The wing-surface of the common swallow is rather more than in the ratio of 2 square feet per pound, but having also great length of pinion, it is both swift and enduring in its flight. When on a rapid course this bird is in the habit of furling its wings into a narrow compass. The greater extent of surface is probably needful for the continual variations of speed and instant stoppages requisite for obtaining its insect food.

On the other hand, there are some birds, particularly of the duck tribe, whose wing-surface but little exceeds half a square foot, or 72 inches per pound; yet they may be classed among the strongest and swiftest flyers. A weight of 1 pound suspended from an area of this extent would acquire a velocity due to a fall of 16 feet, a height sufficient for the destruction or injury of most animals. But when the plane is urged forward horizontally, in a manner analogous to the wings of a bird during flight, the sustaining power is greatly influenced by the form and arrangement of the surface.

In the case of perpendicular descent, as a parachute, the sustaining effect will be much the same, whatever the figure of the outline of the superficies may be, and a circle perhaps affords the best resistance of any. Take for example a circle of 20 square feet (as possessed by the pelican) loaded with as many pounds. This, as just stated, will limit the rate of perpendicular descent to 1,320 feet per minute. But instead of a circle 61 inches in diameter, if the area is bounded by a parallelogram 10 feet long by 2 feet broad, and whilst at perfect freedom to descend perpendicularly, let a force be applied exactly in a horizontal direction so as to carry it edgeways, with the long side foremost, at a forward speed of 30 miles per hour, just double that of its passive descent; the rate of fall under these conditions will be decreased most remarkably, probably to less than one-fifteenth part, or 88 feet per minute, or 1 mile per hour.

The annexed line represents transversely the plane 2 feet wide and 10 feet long, moving in the direction of the arrow with a forward speed of 30 miles per hour, or 2,640 feet per minute, and descending at 88 feet per minute, the ratio being as 1 to 30. Now, the particles of air caught by the forward edge of the plane must be carried down eight-tenths of an inch before they leave it. This stratum, 10 feet wide and 2,640 feet long, will weigh not less than 134 pounds; therefore the weight has continually to be moved downwards 88 feet per minute from a state of absolute rest. If the plane, with this weight and an upward rise of eight-tenths of an inch, be carried forward at a rate of 30 miles per hour, it will be maintained at the same level without descending.



The following illustration, though referring to the action of surfaces in a denser fluid, are yet exactly analogous to the conditions set forth in air:

Take a stiff rod of wood and nail to its end at right angles a thin lath or blade about 2 inches wide. Place the rod square across the thwarts of a rowing-boat in motion, letting a foot or more of the blade hang perpendicularly over the side into the water. The direct amount of resistance of the current against the flat side of the blade may thus be felt. Next slide the rod to and fro thwart-ship, keeping all square; the resistance will now be found to have increased enormously; indeed, the boat can be entirely stopped by such an appliance. Of course the same experiment may be tried in a running stream.

Another familiar example may be cited in the leeboards and sliding keels used in vessels of shallow draught, which act precisely on the same principle as the plane or wing-surface of a bird when moving in air. These surfaces, though parallel to the line of the vessel's course, enable her to carry a heavy press of sail without giving way under the side pressure, or making lee-way, so great is their resistance against the rapidly passing body of water, which can not be deflected sideways at a high speed.

The succeeding experiments will serve further to exemplify the action of the same principle. Fix a thin blade, say 1 inch wide and 1 foot long, with its plane exactly midway and at right angles to the end of a spindle or rod. On thrusting this through a body of water, or immersing it in a stream running in the direction of the axis of the spindle, the resistance will be simply that caused by the water against the mere superficies of the blade. Next put the spindle and blade in rapid rotation. The retarding effect against direct motion will now be increased near tenfold, and is equal to that due to the entire area of the circle of revolution. By trying the effect of blades of various widths, it will be found that, for the purpose of effecting the maximum amount of resistance, the more rapidly the spindle revolves the narrower may be the blade. There is a specific ratio between the width of the blade and its velocity. It is of some importance that this should be precisely defined, not only for its practical utility in determining the best proportion of width to speed in the blades of screw-propellers, but also for a correct demonstration of the principles involved in the subject now under consideration; for it may be remarked that the swiftest-flying birds possess extremely long and narrow wings, and the slow, heavy flyers short and wide ones.

In the early days of the screw-propeller it was thought requisite, in order to obtain the advantage of the utmost extent of surface, that the end view of the screw should present no opening, but appear as a complete disk. Accordingly, some were constructed with one or two threads, making an entire or two half-revolutions; but this was subsequently found to be a mistake. In the case of the two blades, the length of the screw was shortened, and consequently the width of the blades reduced, with increased effect, till each was brought down to considerably less than one-sixth of the circumference or area of the entire circle; the maximum speed was then obtained. Experiment has also shown that the effective propelling area of the two-bladed screw is tantamount to its entire circle of revolution, and is generally estimated as such.

Many experiments tried by the author, with various forms of screws applied to a small steam-boat, led to the same conclusion,—that the two blades of one-sixth of the circle gave the best results.

All screws re-acting on a fluid such as water must cause it to yield to some extent. This is technically known as "slip," and whatever the ratio or percentage on the speed of the boat may be it is tantamount to just so much loss of propelling power, this being consumed in giving motion to the water instead of the boat.

On starting the engine of the steam-boat referred to, and grasping a mooring-rope at the stern, it was an easy matter to hold it back with one hand, though the engine was equal in power to five horses, and the screw making more than five hundred revolutions per minute. The whole force of the steam was absorbed in "slip," or in giving motion to the column of water; but let her go and allow the screw to find an abutment on a fresh body of water not having received a gradual motion, and with its inertia undisturbed when running under full way, the screw worked almost as if in a solid nut, the "slip" amounting to only 11 per cent.

The laws which control the action of inclined surfaces, moving either in straight lines or circles in *air*, are identical, and serve to show the inutility of attempting to raise a heavy body in the atmosphere by means of rotating vanes or a screw acting vertically; for unless the ratio of surface compared to weight is exceedingly extensive, the whole power will be consumed in "slip," or in giving a downward motion to the column of air. Even if a sufficient force is obtained to keep a body suspended by such means, yet, after the desired altitude is arrived at, *no further ascension* is required; there the apparatus is to remain stationary as to level, and its position on the constantly yielding support can only be maintained at an enormous expenditure of power, for the screw can not obtain a hold upon a *fresh and unmoved* portion of air in the same manner as it does upon the body of water when propelling the boat at full speed; its action under these conditions is the same as when the boat is held fast, in which case, although the engine is working up to its usual rate, the tractive power is almost annulled.

Some experiments made with a screw, or pair of inclined vanes acting vertically in air, were tried in the following manner: To an upright post was fixed a frame containing a bevel wheel and pinion, multiplying in the ratio of three to one. The axle of the wheel was horizontal, and turned by a handle of 5½ inches radius. The spindle of the pinion rotated vertically, and carried two driving-pins at the end of a cross-piece, so that the top resembled the three prongs of a trident. The upright shaft of the screw was bored hollow to receive the middle prong, while the two outside ones took a bearing against a driving-bar, at right angles to the lower end of the shaft, the top of which ended in a long iron pivot, running in a socket fixed in a beam overhead; it could thus rise and fall about 2 inches with very little friction. The top of the screw-shaft carried a cross-arm, with a blade of equal size at each extremity, the distance from end to end being six feet. The blades could be adjusted at any angle by clamping-screws. Both their edges and the arms that carry them were beveled away to a sharp edge to diminish the effects of atmoshperic resistance. A wire stay was taken from the base of each blade to the bottom of the upright shaft, to give rigidity to the arms, and to prevent them from springing upwards. With this apparatus experiments were made with weights attached to the upright screw-shaft, and the blades set at different pitches, or angles of inclination. When the vanes were rotated rapidly, they rose and floated on the air, carrying the weights with them. Much difficulty was experienced in raising a heavy weight by a comparatively small extent of surface, moving at a high velocity; the "slip" in these cases being so great as to absorb all the power employed. The utmost effect obtained in this way was to raise a weight of 6 pounds on 1 square foot of sustaining surface, the planes having been set at a coarse pitch. To keep up the rotation required about half the power a man could exert.

The ratio of weight to sustaining surface was next arranged in the proportion approximating to that of birds. Two of the experiments are here quoted, which gave the most satisfactory results. Weight of wings and shaft, 17½ ounces; area of two wings, 121 inches—equal to 110 square inches per pound. The annexed figures are given approximately, in order to avoid decimal fractions:

	No. of revolutions per minute.	Mean sustaining speed per hour.	Feet per minute.	Pitch or angle of rise in one revolution.	Ratio of pitch to speed.	Slip.
		Miles.		*Inches.*		*Per cent.*
First experiment	210	38	3,360	26	*⅛	12½
Second experiment	240	44	3,840	15	*1/13	8

* Nearly.

The power required to drive was nearly the same in both experiments—about equal to one-sixteenth part of a horse-power, or the third part of the strength of a man as estimated by a constant force on the handle of 12 pounds in the first experiment and 10 in the second, the radius of the handle being 5½ inches, and making seventy revolutions per minute in the first case and eighty in the other.

These experiments are so far satisfactory in showing the small pitch or angle or rise required for sustaining the weight stated, and demonstrating the principle before alluded to, of the slow descent of planes moving horizontally in the atmosphere at high velocities; but the question remains to be answered, concerning the disposal of the excessive power consumed in raising a weight not exceeding that of a carrier-pigeon, for unless this can be satisfactorily accounted for there is but little prospect of finding an available power of sufficient energy in its application to the mechanism for raising apparatus, either experimental or otherwise, in the atmosphere. In the second experiment, the screw-shaft made two hundred and forty revolutions, consequently, one vane (there being two) was constantly passing over the *same spot* four hundred and eighty times each minute, or eight times in a second. This caused a descending current of air, moving at the rate of near 4 miles per hour, almost sufficient to blow a candle out placed 3 feet underneath. This is the result of "slip," and the giving both a downward and rotary motion to this column of air will account for a great part of the power employed, as the whole apparatus performed the work of a blower. If the wings, instead of traveling in a circle, could have been urged continually forward in a straight line in a fresh and unmoved body of air, the "slip" would have been so inconsiderable, and the pitch, consequently, reduced to such a small angle as to add but little to the direct forward atmospheric resistance of the edge.

The small flying screws, sold as toys, are well known. It is an easy matter to determine approximately the force expended in raising and maintaining them in the atmosphere. The following is an example of one constructed of tin-plate with three equidistant vanes. This was spun by means of a cord wound round a wooden spindle, fitted into a forked handle as usual. The outer end of the coiled string was attached to a small spring steelyard, which served as a handle to pull it out by. The weight or degree at which the index had been drawn was *afterwards* ascertained by the mark left thereon by a pointed brass wire. It is not necessary to know the *time* occupied in drawing out the string, as this item in the estimate may be taken as the duration of the ascent; for it is evident that if the same force is re-applied at the descent, it would rise again, and a repeated series of these impulses will represent the power required to prolong the flight of the instrument. It is therefore requisite to know the length of string and the force applied in pulling it out. The following are the data:

Diameter of screw	inches..	8¼
Weight of screw	grains..	396
Length of string drawn out	feet..	2
Force employed	pounds..	8
Duration of flight	seconds..	16

From this it may be computed that, in order to maintain the flight of the instrument, a constant force is required of near 60 foot-power per minute —in the ratio of about 3 horse-power for each hundred pounds raised by such means. The force is perhaps over-estimated for a larger screw, for as the size and weight is increased the power required would be less than in this ratio. The result would be more satisfactory if tried with a sheet-iron screw impelled by a descending weight.

Methods analogous to this have been proposed for attempting aërial locomotion; but experiment has shown that a screw rotating in the air is an imperfect principle for obtaining the means of flight and supporting the needful weight, for the power required is enormous. Suppose a machine to be constructed having some adequate supply of force, the screw rotating vertically at a certain velocity will raise the whole. When the desired altitude is obtained, nearly the same velocity of revolution and the same excessive power must be continued, and consumed entirely in "slip," or in drawing down a rapid current of air.

If the axis of the screw is slightly inclined from the perpendicular, the whole machine will travel forward. The "slip," and consequently the power, is somewhat reduced under these conditions; but a swift forward speed can not be effected by such means, for the resistance of the inclined disk of the screw will be very great, far exceeding any form assimilating to the edge of the wing of a bird. But, arguing on the supposition that a forward speed of 30 miles an hour might thus be obtained, even then nearly all the power would be expended in giving an unnecessary and rapid revolution to an immense screw, capable of raising a weight, say, of 200 pounds. The weight alone of such a machine must cause it to fail, and every revolution of the screw is a subtraction from the much desired direct forward speed. A simple narrow blade or inclined plane propelled in a direct course at this speed (which is amply sufficient for sustaining heavy weights) is the best—and in fact the only means of giving the maximum amount of supporting power with the least possible degree of "slip" and direct forward resistance. Thousands of examples in nature testify its success and show the principle in perfection,—apparently the only one, and therefore beyond the reach of amendment,—the wing of a bird, combining a propelling and supporting organ in one, each perfectly efficient in its mechanical action.

This leads to the consideration of the amount of power requisite to maintain the flight of a bird. Anatomists state that the pectoral muscles for giving motion to the wings are excessively large and strong; but this furnishes no proof of the expenditure of a great amount of force in the act of flying. The wings are hinged to the body like two powerful levers, and some counteracting force of a passive nature, acting like a spring under tension, must be requisite merely to balance the weight of the bird. It can not be shown, that while there is no active motion, there is any real exertion of muscular force, for instance, during the time when a bird is soaring with motionless wings. This must be considered as a state of equilibrium, the downward spring and elasticity of the wings serving to support the body, the muscles in such a case performing like stretched india-rubber spring would do. The motion or active power required for the performance of flight must be considered exclusive of this.

It is difficult, if not impossible, by any form of dynamometer to ascertain the precise amount of force given out by the wings of birds; but this is perhaps not requisite in proof of the principle involved; for when the laws governing their movements in air are better understood it is quite possible to demonstrate by isolated experiments the amount of power required to sustain and propel a given weight and surface at any speed.

If the pelican, referred to as weighing 21 pounds with near the same amount of wing area (in square feet), were to descend perpendicularly, it would fall at the rate of 1,320 feet per minute (22 feet per second), being limited to this speed by the resistance of the atmosphere.

The standard generally employed in estimating power is by the rate of descent of a weight. Therefore, the weight of the bird being 21 pounds, which falling at the above speed, will expend a force on the air set in motion nearly equal to 1 horse (.84 horse-power) or that of five men; and conversely, to raise this weight again perpendicularly upon a yielding support like air, would require even more power than this expression, which it is certain that a pelican does not possess; nor does it appear that any large bird has the faculty of raising itself on the wing perpendicularly in a still atmosphere. A pigeon is able to accomplish this nearly, mounting to the top of a house in a very narrow compass; but the exertion is evidently severe and can only be maintained for a short period. For its size, this bird has great power of wing; but this is perhaps far exceeded in the humming-bird, which, by the extremely rapid movements of its pinions, sustains itself for more than a minute in still air in one position. The muscular force required for this feat is much greater than for any other performance of flight. The body of the bird at the time is nearly vertical. The wings uphold the weight, not by striking vertically downwards upon the air, but as inclined surfaces reciprocating horizontally like a screw, but wanting in its continuous rotation in one direction, and in consequence of the loss arising from rapid alternations of motions, the power required for the flight will exceed that specified in the screw experiment before quoted, viz, 3 horsepower for every 100 pounds raised.

We have here an example of the exertion of enormous animal force expended in flight, necessary for the peculiar habits of the bird, and for obtaining its food; but in the other extreme, in large heavy birds, whose wings are merely required for the purposes of migration or locomotion, flight is obtained with the least possible degree of power, and this condition can only be commanded by a rapid straight-forward course through the air.

The sustaining power obtained in flight must depend upon certain laws of action and re-action between relative weights; the weight of a bird, balanced, or finding an abutment, against the fixed inertia of a far greater weight of air, continuously brought into action in a given time. This condition is secured, not by extensive surface, but by great length of wing, which, in forward motion, takes a support upon a wide stratum of air, extending transversely to the line of direction.

The pelican, for example, has wings extending out 10 feet. If the limits of motion imparted to the substratum of air, acted upon by the incline of the wing, be assumed as 1 foot in thickness, and the velocity of flight as 30 miles per hour, or 2,640 feet per minute, the stratum of air passed over in this time will weigh nearly 1 ton, or one hundred times the weight of the body of the bird, thus giving such an enormous supporting power that the comparatively small weight of the bird has but little effect in deflecting the heavy length of the stratum downwards, and therefore the higher the velocity of flight the less the amount of "slip" or power wasted in compensation for descent.

As noticed at the commencement of this paper, large birds may be observed to skim close above smooth water without ruffling the surface, showing that during rapid flight the air does not give way beneath them, but approximates towards a solid support.

In all inclined surfaces, moving rapidly through the air, the whole sustaining power approaches toward the front edge; and, in order to exemplify the inutility of surface alone, without proportionate length of wing, take a plane 10 feet long by 2 broad, impelled with the narrow end forward, the first 12 or 15 inches will be as efficient at a high speed in supporting a weight as the entire following portion of the plane which may be cut off, thus reducing the effective wing-area of a pelican, arranged in this direction, to the totally inadequate equivalent of 2½ square feet.

One of the most perfect natural examples of easy and long-sustained flight is the wandering albatross. "A bird for endurance of flight probably unrivalled. Found over all parts of the Southern Ocean. It seldom rests on the water. During storms, even the most terrific, it is seen now dashing through the whirling clouds, and now serenely floating, without the least observable motion of its outstretched pinions." The wings of this bird extend 14 or 15 feet from end to end, and measure only 8½ inches across the broadest part. This conformation gives the bird such an extraordinary sustaining power, that it is said to sleep on the wing during stormy weather, when rest on the ocean is impossible. Rising high in the air, it skims slowly down, with absolutely motionless wings, till a near approach to the waves awakens it, when it rises again for another rest.

If the force expended in actually sustaining a long-winged bird upon a wide and unyielding stratum of air, during rapid flight, is but a small fraction of its strength, then nearly the whole is exerted in overcoming direct forward resistance. In the pelican referred to, the area of the body, at its greatest diameter, is about 100 square inches; that of the pinions, 80. But as the contour of many birds during flight approximates nearly to Newton's solid of least resistance, by reason of this form, acting like the sharp bows of a ship, the opposing force against the wind must be reduced down to one third or fourth part; this gives one-tenth of a horse-power, or about half the strength of a man, expended during a flight of 30 miles per hour. Judging from the action of the living bird when captured, it does not appear to be more powerful than here stated.

The transverse area of a carrier pigeon during flight (including the outstretched wings) a little exceeds the ratio of 12 square inches for each pound, and the wing surface, or sustaining area, 90 square inches per pound.

Experiments have been made to test the resisting power of conical bodies of various forms, in the following manner: A thin lath was placed horizontally, so as to move freely on a pivot set midway; at one end of the lath a circular card was attached, at the other end a sliding clip traversed, for holding paper cones, having their bases the exact size of the opposite disk. The instrument acting like a steelyard; and when held against the wind, the paper cones were adjusted at different distances from the center, according to their forms and angles, in order to balance the resistance of the air against the opposing flat surface. The resistance was found to be diminished nearly in the ratio that the height of the cone exceeded the diameter of its base.

It might be expected that the pull of the string of a flying kite should give some indication of the force of inclined surfaces acting against a current of air; but no correct data can be obtained in this way. The incline of a kite is far greater than ever appears in the case of the advancing wing surface of a bird. The tail is purposely made to give steadiness by a strong pull backwards from the action of the wind, which also exerts considerable force on the suspended cord, which for more than half its length hangs nearly perpendicularly. But the kite, as a means of obtaining unlimited lifting and tractive power, in certain cases where it might be usefully applied, seems to have been somewhat neglected. For its power of raising weights, the following quotation is taken from vol. XLI of the *Transactions of the Society of Arts*, relating to Captain Dansey's mode of communicating with a lee-shore. The kite was made of a sheet of holland exactly 9 feet square, extended by two spars placed diagonally, and as stretched spread a surface of 55 square feet: "The kite, in a strong breeze, extended 1,100 yards of line five-eighths of an inch in circumference, and would have extended more had it been at hand. It also extended 360 yards of line, 1¾ inches in circumference, weighing 60 pounds. The holland weighed 3½ pounds; the spars, one of which was armed at the head with iron spikes for the purpose of mooring it, 6¾ pounds; and the tail was five times its length, composed of 8 pounds of rope and 14 of elm plank, weighing together 22 pounds."

We have here the remarkable fact of 92¼ pounds carried by a surface of only 55 square feet.

As all such experiments bear a very close relation to the subject of this paper, it may be suggested that a form of kite should be employed for reconnoitering and exploring purposes, in lieu of balloons held by ropes. These would be torn to pieces in the very breeze that would render a kite most serviceable and safe. In the arrangement there should be a smaller and upper kite, capable of sustaining a weight of the apparatus. The lower kite should be as nearly as practicable in the form of a circular flat plane, distended with ribs, with a car attached beneath like a parachute. Four guy-ropes leading to the car would be required for altering the angle of the plane—vertically with respect to the horizon, and laterally relative to the direction of the wind. By these means the observer could regulate his altitude so as to command a view of a country in a radius of at least 20 miles; he could veer to a great extent from side to side, from the wind's course, or lower himself gently, with the choice of a suitable spot for descent. Should the cord break or the wind fail, the kite would, in either case, act as a parachute and as such might be purposely detached from the cord, which then being sustained from the upper kite, could be easily recovered. The direction of descent could be commanded by the guy-rope, these being hauled taut in the required direction for landing.

The author has good reasons for believing that there would be less risk associated with the employment of this apparatus than the reconnoitering balloons that have now frequently been made use of in warfare.*

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*The practical application of these suggestions appears to have been anticipated some years previously. In a small work, styled the "History of the Charvolant or Kite Carriage," published by Longman & Co., appears the following remarks:

The wings of all flying creatures, whether of birds, bats, butterflies, or other insects, have this one peculiarity of structure in common: The front, or leading edge, is rendered rigid by bone, cartilage, or a thickening of the membrane; and in most birds of perfect flight even the individual feathers are formed upon the same condition. In consequence of this, when the wing is waved in the air, it gives a persistent force in one direction, caused by the elastic re-action of the following portion of the edge. The fins and tails of fishes act upon the same principle: in the most rapid swimmers these organs are termed "lobated and pointed." The tail extends out very wide transversely to the body, so that a powerful impulse is obtained against a wide stratum of water, on the condition before explained. This action is imitated in Macintosh's screw-propeller, the blade of which is made of thin steel, so as to be elastic. While the vessel is stationary, the blades are in a line with the keel, but during rotation they bend on one side more or less, accord-

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"These buoyant sails, possessing immense power, will, as we have before remarked, serve as floating observatories. - - - Elevated in the air, a single sentinel, with a perspective, could watch and report the advance of the most powerful forces, while yet at a great distance. He could mark their line of march, the composition of their force, and their general strength, long before he could be seen by the enemy." Again, at page 53, we have an account of ascents actually made as follows: "Nor was less progress made in the experimental department, when large weights were required to be raised or transposed. While on this subject, we must not omit to observe that the first person who soared aloft in the air by this invention was a lady, whose courage would not be denied this test of its strength. An arm-chair was brought on the ground, then lowering the cordage of the kite by slackening the lower brace, the chair was firmly lashed to the main line, and the lady took her seat. The main-brace being hauled taut, the huge buoyant sail rose aloft with its fair burden, continuing to ascend to the height of 100 yards. On descending, she expressed herself much pleased with the even motion of the kite, and the delightful prospect she had enjoyed. Soon after this, another experiment of a similar nature took place, when the inventor's son successfully carried out a design not less safe than bold—that of scaling, by this powerful aerial machine, the brow of a cliff 200 feet in perpendicular height. Here, after safely landing, he again took his seat in a chair expressly prepared for the purpose, and, detaching the swivel-line, which kept it at its elevation, glided gently down the cordage to the hand of the director. The buoyant sail employed on this occasion was 30 feet in height, with a proportionate spread of canvas. The rise of the machine was most majestic, and nothing could surpass the steadiness with which it was maneuvered, the certainty with which it answered the action of the braces, and the ease with which its power was lessened or increased. - - - Subsequently to this, an experiment of a very bold and novel character was made upon an extensive down, where a wagon with a considerable load was drawn along, whilst this huge machine, at the same time, carried an observer aloft in the air, realizing almost the romance of flying."

It may be remarked that the brace-lines here referred to were conveyed down the main-line and managed below; but it is evident that the same lines could be managed with equal facility by the person seated in the car above; and if the main-line were attached to a water-drag instead of a wheeled car, the adventurer could cross rivers, lakes, or bays, with considerable latitude for steering and selecting the point of landing, by hauling on the port or starboard brace-lines as required. And from the uniformity of the resistance offered by the water drag, this experiment could not be attended with any greater amount of risk than a land flight by the same means.

ing to the speed and degree of propulsion required, and are thus self-compensating; and could practical difficulties be overcome would prove to be a form of propeller perfect in theory.

In the flying mechanism of beetles there is a difference of arrangement. When the elytra, or wing-cases, are opened, they are checked by a stop, which sets them at a fixed angle. It is probable that these serve as "aero-planes," for carrying the weight of the insect, while a delicate membrane that folds beneath acts more as a propelling than a supporting organ. A beetle can not fly with the elytra removed.

The wing of a bird or bat is both a supporting and propelling organ, and flight is performed in a rapid course, as follows: During the down-stroke it can be easily imagined how the bird is sustained; but in the up-stroke the weight is also equally well supported, for in raising the wing it is slightly inclined upwards against the rapidly passing air, and as this angle is somewhat in excess of the motion due to the raising of the wing, the bird is sustained as much during the up as the down stroke—in fact, though the wing may be rising, the bird is still pressing against the air with a force equal to the weight of its body. The faculty of turning up the wing may be easily seen when a large bird alights, for after gliding down its aerial gradient, on its approach to the ground it turns up the plane of its wing against the air; this checks its descent, and it lands gently.

It has before been shown how utterly inadequate the mere perpendicular impulse of a plane is found to be in supporting a weight when there is no horizontal motion at the time. There is no material weight of air to be acted upon, and it yields to the slightest force, however great the velocity of impulse may be. On the other hand, suppose that a large bird in full flight can make 40 miles per hour, or 3,520 feet per minute, and perform one stroke per second. Now, during every fractional portion of that stroke the wing is acting upon and obtaining an impulse from a fresh and undisturbed body of air, and if the vibration of the wing is limited to an arc of 2 feet, this by no means represents the small force of action that would be obtained when in a stationary position, for the impulse is secured upon a stratum of 58 feet in length of air at each stroke. So that the conditions of weight of air for obtaining support, equally well apply to weight of air and its re-action in producing forward impulse.

So necessary is the acquirement of this horizontal speed, even in commencing flight, that most heavy birds, when possible, rise against the wind, and even run at the top of their speed to make their wings available, as in the example of the eagle, mentioned at the commencement of this paper. It is stated that the Arabs on horseback can approach near enough to spear these birds, when on the plain, before they are able to rise. Their habit is to perch on an eminence where possible.

The tail of a bird is not necessary for flight. A pigeon can fly perfectly with this appendage cut short off; it probably performs an important function in rapid steering, for it is to be remarked that most birds that have either to pursue or evade pursuit are amply provided with this organ.

The foregoing reasoning is based upon facts, which tend to show that the flight of the largest and heaviest of all birds is really performed with but a small amount of force, and that man is endowed with sufficient muscular power to enable him also to take individual and extended flights, and that success is probably only involved in a question of suitable mechanical adaptations. But if the wings are to be modelled in imitation of natural examples, but very little consideration will serve to demonstrate its utter impracticability when applied in these forms. The following diagram, Fig. 1, would be about the proportions needed for a man of medium weight. The wings *a a* must extend out 60 feet from end to end and measure 4 feet across the broadest part. The man, *b*, should be in a horizontal position, incased in a strong frame-work, to which the wings are hinged at *c c*. The wings must be stiffened by elastic ribs extending back from the pinions. These must be trussed by a thin band of steel, *e e*, Fig. 2, for the purpose of diminishing the weight and thickness of the spar. At the front, where the pinions are hinged, there are two levers attached and drawn together by a spiral spring, *d*, Fig. 2, the tension of which is sufficient to balance the weight of the body and machine and cause the wings to be easily vibrated by the movement of the feet acting on the treadles. This spring serves the purpose of the pectoral muscles in birds. But with all such arrangements the apparatus must fail; *length of wing is indispensable!* and a spar 30 feet long must be strong, heavy, and cumbrous; to propel this along through the air at a high speed would require more power than any man could command.

Fig. 1.

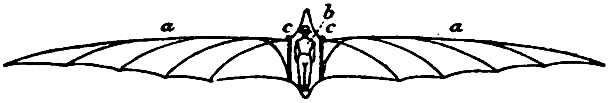
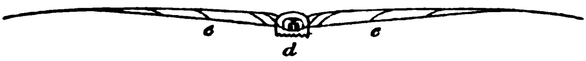


Fig. 2.



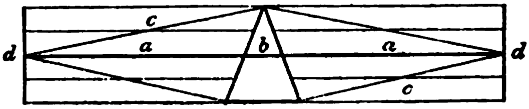
In repudiating all imitations of natural wings, it does not follow that the only channel is closed in which flying mechanism may prove successful. Though birds do fly upon definite mechanical principles and with a moderate exertion of force, yet the wing must necessarily be a vital organ and member of the living body. It must have a marvellous self-acting principle of repair in case the feathers are broken or torn; it must also fold up in a small compass and form a covering for the body.

These considerations bear no relation to artificial wings; so in designing a flying-machine, any deviations are admissible, provided the theoretical conditions involved in flight are borne in mind.

Having remarked how thin a stratum of air is displaced beneath the wings of a bird in rapid flight, it follows that in order to obtain the *necessary length* of plane for supporting heavy weights, the surfaces may be superposed, or placed in parallel rows, with an interval between them. A dozen pelicans may fly one above the other without mutual impediment, as if framed together; and it is thus shown how 2 hundred-weight may be supported in a transverse distance of only 10 feet.

In order to test this idea, six bands of stiff paper, 3 feet long and 3 inches wide, were stretched at a slight upward angle, in a light rectangular frame, with an interval of 3 inches between them, the arrangement resembling an open Venetian blind. When this was held against a breeze, the lifting power was very great, and even by running with it in a calm it required much force to keep it down. The success of this model led to the construction of one of a sufficient size to carry the weight of a man. Fig. 3 represents the arrangement: *a a* is a thin plank, tapered at the outer ends, and attached at the base to a triangle, *b*, made of similar plank, for the insertion of the body. The boards *a a* were trussed with thin bands of iron, *c c*, and at the ends were vertical rods, *d d*. Between these were stretched five bands of holland, 15 inches broad and 16 feet long, the total length of the web being 80 feet. This was taken out after dark into a wet piece of meadow land, one November evening, during a strong breeze wherein it became quite unmanageable. The wind acting upon the already tightly-stretched webs, their united pull caused the central boards to bend considerably, with a twisting, vibratory motion. During a lull, the head and shoulders were inserted in the triangle, with the chest resting on the base-board. A sudden gust caught up the experimenter, who was carried some distance from the ground, and the affair falling over sideways broke up the right-hand set of webs.

Fig. 3.



In all new machines we gain experience by repeated failures, which frequently form the stepping-stone to ultimate success. The rude contrivance just described (which was but the work of a few hours) had taught first that the webs, or aero-planes, must not be distended in a frame, as this must of necessity be strong and heavy, to withstand their combined tension; second, that the planes must be made so as either to furl or fold up, for the sake of portability.

In order to meet these conditions the following arrangement was afterwards tried: *a a*, Figs. 4 and 5, is the main spar, 16 feet long, half an inch thick at the base, and tapered both in breadth and thickness to the end; to this spar was fastened the panels *b b*, having a base-board for the support of the body. Under this and fastened to the end of the main spar is a thin steel tie-band, *e e*, with struts starting from the spar. This serves as a foundation of the superposed aero planes, and though very light, was found to be exceedingly strong, for when the ends of the spar were placed upon supports, the middle bore the weight of the body without any strain or deflection; and further, by separation at the base-board, the spars could be folded back with a hinge to half their length. Above this were arranged the aero planes, consisting of six webs of thin holland 15 inches broad; these were kept in parallel planes by vertical divisions 2 feet wide, of the same fabric, so that when distended by a current of air, each 2 feet of web pulled in opposition to its neighbor; and finally, at the ends (which were each sewn over laths) a pull due to only 2 feet had to be counteracted, instead of the strain arising from the entire length, as in the former experiment. The end-pull was sustained by vertical rods, sliding through loops on the transverse ones at the ends of the webs, the whole of which could fall flat on the spar till raised and distended by a breeze. The top was stretched by a lath, *f*, and the system kept vertical by stay cords taken from a bowsprit carried out in front, shown in Fig. 6. All the front edges of the aero-planes were stiffened by bands of crinoline steel. This series was for the supporting arrangement, being equivalent to a length of wing of 96 feet. Exterior to this, two propellers were to be attached, turning on spindles just above the back. They are kept drawn up by a light spring, and pulled

Fig. 4.

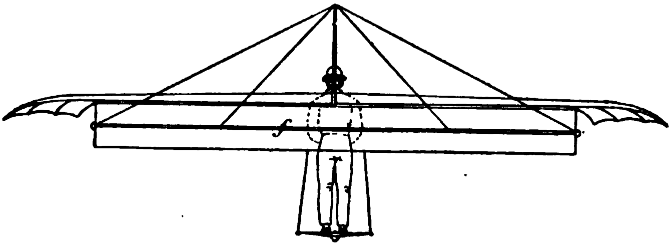


Fig. 5.

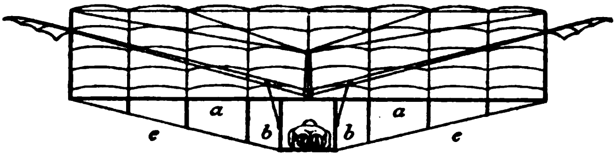
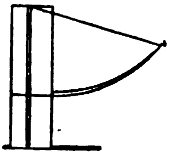


Fig. 6.



down by cords or chains running over pulleys in the panels *b b*, and fastened to the end of a swiveling cross-yoke, sliding on the base-board. By working this cross-piece with the feet, motion will be communicated to the propellers, and by giving a longer stroke with one foot than the other a greater extent of motion will be given to the corresponding propeller, thus enabling the machine to turn just as oars are worked in a rowing-boat. The propellers act on the same principle as the wing of a bird or bat; their ends being made of fabric, stretched by elastic ribs, a simple waving motion up and down will give a strong forward impulse. In order to stop, the legs are lowered beneath the base-board, and the experimenter must run against the wind.

An experiment recently made with this apparatus developed a cause of failure. The angle required for producing the requisite supporting power was found to be so small, that the crinoline steel would not keep the front edges in tension. Some of them were borne downward and more on one side than the other, by the operation of the wind, and this also produced a strong fluttering motion in the webs, destroying the integrity of their plane surfaces, and fatal to their proper action.

Another arrangement has since been constructed, having laths sewn in both edges of the webs, which are kept permanently distended by cross-stretchers. All these planes are hinged to a vertical central board, so as to fold back when the bottom ties are released; but the system is much heavier than the former one, and no experiments of any consequence have as yet been tried with it.

It may be remarked that although a principle is here defined, yet considerable difficulty is experienced in carrying the theory into practice. When the wind approaches to 15 or 20 miles per hour, the lifting power of these arrangements is all that is requisite, and by additional planes, can be increased to any extent; but the capricious nature of the ground-currents is a perpetual source of trouble.

Great weight does not appear to be of much consequence, *if carried in the body;* but the aero-planes and their attachments seem as if they were required to be very light, otherwise they are awkward to carry, and impede the movements in running and making a start. In a dead calm it is almost impracticable to get sufficient horizontal speed by *mere running* alone to raise the weight of the body. Once off the ground, the speed must be an increasing one, if continued by suitable propellers. The small amount of experience as yet gained appears to indicate that if the aero-planes could be raised in detail, like a superposed series of kites, they would first carry the weight of the machine itself, and next relieve that of the body.

Until the last few months no substantial attempt has been made to construct a flying-machine in accordance with the principle involved in this paper, which was written seven years ago. The author trusts that he has contributed something towards the elucidation of a new theory, and shown that the flight of a bird in its performance does not require that enormous amount of force usually supposed, and that in fact birds do not exert more power in flying than quadrupeds in running, but considerably less; for the wing movements of a large bird, travelling at a far higher speed in the air, are very much slower; and where weight is concerned, great velocity of action in the locomotive organs is associated with great force.

It is to be hoped that further experiments will confirm the correctness of these observations, and with a sound working theory upon which to base his operations man may yet command the air with the same facility that birds now do.

ON THE MOVEMENTS OF THE EARTH'S CRUST.

BY A. BLYTT.[*]

Translated by W. S. DALLAS, F. L. S.

This memoir is an attempt to further develop and establish ideas which I put forward five years ago. It contains an attempt to establish a chronology in geology. It sets forth what the English call "a working hypothesis," without claiming to be anything else. It was the distribution of plants which first introduced the author to this great question; but the problem of a chronology in geology can not be solved without the co-operation, it may perhaps be said, of all naturalists. It certainly can not finally be solved by any one man. In putting forth my hypothesis I must in the first place beg for indulgence for the many faults and imperfections with which such an attempt must be affected, and express a hope that in any case the hypothesis may be found worthy of being further tested.

Having endeavored in several memoirs on the distribution of plants, on peat-mosses, shore-lines, terraces, and morainic ridges, to show that climates undergo periodical changes, I published in the Transactions of the Society of Sciences for 1883 (No. 9) a memoir on alternation of strata and its possible significance for the chronology of geology and the theory of the modification of species. The essential contents of this paper, as regards the present question of geological chronology, were as follows:

Alternations of strata, under which term is understood an alternation of geological formations of different constitution, can be produced by local conditions of rapidly passing change, without the action of general and persistent causes. But there are also causes of the latter kind which effect an alternation of the strata. Two such periodically acting causes are traceable in the geological series of deposits—a shorter, somewhat regular one, and a longer, more irregular one. The former effects a change of climate, the strength of the marine currents alternately diminishing and increasing during thousands of years; the latter longer period effects a rise or fall of the sea in relation to the land, and an alternation of deep-sea formations with shore-formations or fresh-water deposits. The opinion has been expressed that these periods, which are traced in the series of deposits, might possibly stand in connection with the two cosmical periods revealed by astronomy—the precession of the equinoctial points, and variations in the eccentricity of the earth's orbit; although in the memoir referred to it is not attempted to show in what manner such a connection could be established. But if, with the aid of these two hypotheses, we construct an "artificial" series of strata, we find that one with no less than forty-six changes of deposit may be recognized, bed by bed, in the Tertiary formations of the Paris basin.

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[*] "On the probable cause of the Displacement of Shore-lines,—an attempt at a Geological Chronology." Read at the General Meetings of the Society of Science of Christiania, December 9, 1887, and June 1, 1888. Translated from the *Nyt Magazin for Naturvidenskaberne*, 1889; Bd. XXXI., pp, 240–297. (From the *London, Edinburgh, and Dublin, Philosophical Magazine*, May and June, 1889, vol. XXVII, pp. 405–429, and 487–519.)

The result may encourage us to test still further the correctness of the two suppositions. As regards the precession this has been attempted in my paper "On the probable cause of the periodical change in the strength of the marine currents."*

The contents of this memoir are essentially as follows: The precession of the equinoctial lines causes the summers in about 10,500 years to be longer, and in the following 10,500 years shorter, than the winters. The conditions are opposite in the northern and southern hemispheres. The difference between the number of winter and summer days increases with the eccentricity of the earth's orbit.

The cooling of continents under high latitudes, in the winter, produces a diminished pressure of air over the sea. This low pressure draws air from lower latitudes. For this reason, in the Atlantic, southwest winds prevail. Thus, in the winter, the southwest winds of the North Atlantic are on an average three times as strong as in the summer, in consequence of the great refrigeration of the mainland. In the semi-period when the winter falls in aphelion the average annual wind-force is consequently greater. Now it is the prevalent wind that produces the powerful marine currents, such as the warm current in the Atlantic Ocean. The strength of the marine currents is dependent upon the average wind-force for the last great time period. Now as this average wind-force is periodically variable in consequence of the precessions, the strength of marine currents and the temperature of the sea must also be subject to a periodical variability. For about 10,500 years the warm sea-current will increase, to diminish in the next similar period, and so on constantly through all time. When the winter falls in aphelion, the difference between the littoral and inland climates will increase. The propelling force of currents in the sea will increase and diminish by 1 to 5 per cent. upon their total annual value according as the winter falls in aphelion or perihelion, and according as the eccentricity of the earth's orbit is small or great.

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* *Vid. Selsk. Förh. Christiania*, December 14, 1883; *Archiv f. Math. og Naturv.*, IX. Christiania, 1884.

Such an alteration in the strength of the marine currents will produce an alteration of the climate, which however will not be very important, but which will nevertheless be great enough to leave its traces in the deposits. During colder and drier seasons the streams are fed in great part by spring water. This water has drained slowly through the beds and is charged with dissolved materials; but the small quantity of water and the feebler streams carry less clay, sand, and gravel. During rainy seasons, the rain carries down quantities of such materials, but it flows off rapidly, and as it for the most part runs only over the surface it has not time to dissolve so much. Although the springs flow more abundantly during rainy seasons, their water only mingles with the rain-water. The streams are therefore poorer in dissolved material, but they contain more water, and their more powerful current carries more clay, sand, and gravel into the basin. Hence the drier seasons will be richer in purely chemical deposits, which will be transported in the clearer water; the wet seasons in mechanical deposits. Strata of both kinds are formed, of course, at all times, but they are deposited at different places in accordance with the variation in the quantity of rain. Thus, I assume that when thick deposits of river-sand and clay alternate with each other, when soft clay and marl alternate with hard marl or limestone, when thick strata of loose sand alternate with sandstone, which is bound together by chemically produced cement (iron, silica, lime), when clay alternates with Septaria-beds, etc., then, in each case, the first-named deposit shows itself to belong to seasons with a warmer sea and a greater quantity of rain, which, as regards western Europe, will mean seasons with the winter in aphelion.

That this alternation of deposits implies a period of several thousand years' duration is shown by the fact that the fossils change rapidly through the strata. In the Tertiary formations there are only a few, often only four to five, such changes of deposits in each stage. The whole Oligocene period has only about thirty, the Miocene still fewer, and the Pliocene barely twenty such changes.

In this way, in my opinion, the precessions stamp themselves upon the strata, and this should therefore furnish a means of measuring time. The greater the eccentricity of the orbit, the more strongly marked will the periods be; when the orbit approaches the circular form, they are less recognizable.*

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* But the perihelion also shifts to and fro. The time between two aphelia in the winter solstice varied thus in post-glacial times by fully 4600 years. This must have some influence. The longer a period with winters in aphelion lasts the longer will the warm currents in the Atlantic increase in strength, and the greater will be the changes of climate. The mild period during which Bergenian sea-animals lived in the Christiania Fjord, and which has left its traces elsewhere in our hemisphere, was in my opinion, a consequence of such an unusually long period with the winter in

Referring for other things to the two memoirs cited and to my paper "On Variations of the Weather in the course of time" (*Letterstedtske Nordisk Tidskrift*, 1885, in English, in *Fork. Vid. Selsk. i Christiania*, 1886, No. 8) I will pass on to examine whether there is any probable ground for supposing that the other proposition is also correct, whether it is conceivable that under high latitudes the sea-level rises and sinks with the eccentricity of the earth's orbit.

Great part of the earth's surface consists of strata which still lie undisturbed in their original horizontal position. These parts are called "tables" by Suess. But in many places the crust of the earth is so traversed by clefts and fissures that it may be compared to a breccia. Fragments are often displaced relatively by thousands of feet. Strata which originally lay horizontally are folded, thicknesses of 7,000 to 8,000 feet are bent as if they were straws (Kjerulf, *Udsigt over Norges Geologi*, 1879, p. 76). Moreover, the folded strata are upheaved far above their original level. Even marine formations so recent as the Eocene are uplifted to heights of 21,000 feet above the sea (Suess, *Antlitz der Erde*, I, p. 564). Sometimes they stand vertically, or are inverted, so that older strata cover the younger ones. Through fissures eruptive masses are brought forth, and have covered thousands upon thousands of square kilometers. The distribution of land and sea also varies. It is indeed supposed that the great depths of the ocean and the great continents have essentially retained their original distribution from the most ancient times, but the shore-lines wander periodically to and fro; and these changes of the earth's surface have taken place from earliest times, and are still in action at the present day.

Geologists in general seek the explanation of these phenomena in the cooling and contraction of the body of the earth. The earth's crust folds, just as the skin of an apple wrinkles as the apple dries. The leading geologists of the present day adopt this theory, and A. Geikie in his "Text-book of Geology" (London, 1882, p. 287) says truly: "With modifications, the main cause of terrestrial movements is still sought in secular contraction."

According to this doctrine changes in the crust of the earth are due to the interior contracting more strongly than "the crust," so that the latter is too large for it. Its weight drags it down. By this means great horizontally acting pressure is produced in the crust, which must then become folded and cracked in places. The fragments sink down. By this means are formed what Suess has called "Einbrüche." When a part of the crust remains in position while all around it sinks, there is produced what Suess has called a "Horst." The old theory of forces

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aphelion. The winter solstice fell in aphelion (according to Croll) 61,300, 33,300, and 11,700 years ago. The middle of the Atlantic period with Bergenian sea-animals in the Christiania Fjord fell, from the testimony of the peat-mosses 33,000 to 34,000 years ago, therefore in accordance with the period of 28,000 years.

acting vertically from below is most decidedly rejected by Suess. He and Heim have shown, by their investigations of the Alps, that the foldings of the Alps are caused by lateral pressure, and that such lateral pressure is sufficient to lift great chains of mountains into the air. But Suess goes still further, for in a memoir, "Ueber die vermeintlichen säcularen Schwankungen einzelner Theile der Erdoberfläche" (in *Verh. K. K. Geol. Reichs.*, 1880, pp. 171 *et seq.*), he even denies any elevation by forces acting vertically from below;—neither mountain nor continent is elevated in this manner. He says (*l. c.* p. 180): "There are no vertical movements of the solid ground, with the exception of those which proceed directly from the formation of folds. We shall have to resolve to abandon the doctrine of secular oscillations of continents."

A. de Lapparent, who sharply criticises Suess's theory of "Horste" (*Bull. Soc. Géol. France*, sér. 3, tome XV, pp. 215 *et seq.*), nevertheless agrees with him that the cooling of the earth has formed great folds in the crust, and denies that any elevations are not caused by foldings. Thus he says (*l. c.* p. 217): "It is no longer necessary to oppose to the doctrine of absolute elevations produced by forces acting directly from below upwards, a protestation which has lost its object. For the partisans of vertical impulsions are nowadays more than scattered, and with the exception of a very few belated persons no one would now venture to ascribe to such an action an important part in the formation of mountains." As he makes no limitations, it must be assumed that he will not recognize any forces acting from below to elevate whole landmasses.

According to a statement of Suess's, in his *Antlitz der Erde* (1885, Bd. I, p. 741), he seems to find an essential reason for denying elevation by forces acting perpendicularly from below, in that we are quite ignorant of any force which could be capable of causing such an elevation.

The theories of Hutton and von Buch as to the action of such forces seem therefore to be rejected by geologists of the present day. Nevertheless there are still a few who hold similar opinions. Thus J. C. Russel (U. S. Geological Survey, Fourth Annual Report, Washington, 1884, pp. 452, 453,) says that the fractures in "the Great Basin" are not in consequence of any lateral pressure, but are caused by an extension in a horizontal direction: "The fractures are closely related to an extension of the strata by upheaval." It seems to me improbable that such a relation should be explicable by a folding. C. E. Dutton also (U. S. Geological Survey, Sixth Annual Report, 1885, p. 198,) at the same time that he recognizes that many chains are folded by lateral pressure, says, with regard to the mountain-masses in Western North America: "The mountains of the West have not been produced by horizontal compression, but by some unknown forces beneath which have pushed them up."*

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*The current doctrines with regard to refrigeration and compression are discussed by Pierce in a discourse before the American Academy on the 11th May, 1869 (see

It is not my intention to maintain that refrigeration has not at all contributed to give the surface of the earth the form which it now possesses. But I think that an auxiliary theory is required, which, while it will not entirely supersede the old theory, may yet serve to explain things which the old theory can not render comprehensible.

Henry H. Howorth has written two memoirs, namely, "Recent Elevations of the Earth's Surface in the Northern Circumpolar Regions" (*Journ. Roy. Geogr. Soc.*, 1873, vol. XLIII, p. 240) and "Recent Changes in the Southern Circumpolar Regions" (*op. cit.*, 1874, vol. XLIV, p. 252), in which he has brought together what was at that time known as to the displacement of shore-lines in the last section of geological time, and the principal result of his investigations is summed up in the following words: "The South Pole, as well as the North, is a focus of protrusion, the land around it is being gradually elevated." In the last section of geological time, *i. e.*, in the Post-glacial period, the land has in general sunk under lower and risen under higher latitudes.

Suess arrived at a similar result in his above-cited memoir (*Verh. K. K. Geol. Reichs.* 1880, pp. 174–175). He has likewise studied the displacement of coast-lines over the whole earth during the period nearest to the present time, and sums up the result as follows: "*Terraced land* [*i. e.* land which has recently risen in relation to the sea] *appears everywhere in the high northern latitudes*, so far as man has hitherto penetrated into these solitudes. It also extends far, although not everywhere equally far, down into the temperate latitudes, but generally decreasing in height. In other words, around the North Pole, and far down, the sum of the negative [*i. e.*, descending] movements of the coast-lines is greater than the positive; towards the south, however, these two sums approximate more and more. In *tropical seas*, in the regions of the coral formations, *the opposite condition occurs*, the sum of the positive movements preponderates. *Further towards the south*, beyond 25° to 35° south latitude, *the terraced land of the north begins again in South America, South Africa, South Australia, and New Zealand*, *i. e.*, the same preponderance of the negative movements, with the same oscillating* character as in the north." The exceptions (according to Suess) are few and of little importance.

Proc. Amer. Acad. Arts and Sci., 1873, vol. VIII, p. 106), as also by O. Fisher ("Physics of the Earth's Crust," 1881) and Dutton ("A Criticism upon the Contractional Hypothesis," in *Amer. Journ. Sci.*, 1874, ser. 3, vol. VIII, pp. 113 *et. seq.*). They all consider that contraction is not sufficient to explain the known phenomena; nay, the last-named even thinks that the phenomena are opposed to this. A. de Lapparent, on the other hand, in his memoir "Contraction et refroidissement du globe" (*Bull. Soc. Géol. France*, 1887, sér. 3, vol. XV, pp. 383 *et seq.*) seeks to prove that they are quite sufficient.

* With this word Suess alludes to the circumstances that the coast-lines and terraces occur at various levels one above the other. He thinks that each of these levels indicates an oscillation of the sea. I believe that *the greater part* of these levels are merely a consequence of climatic changes due to the precessions. (See *Fork. Vid. Selsk. Christ.* 1881, No. 4.)

Howorth and Suess have therefore both come to the same result. But their explanations are directly opposite. Howorth thinks that it is the land which has arisen under higher latitudes; that the earth, as it were, swells up towards the poles and contracts under the tropics. Suess, who will not admit any other elevations than those which are the consequences of foldings, is of the opinion that it is the sea which has flowed towards the lower latitudes. He indicates as a possible explanation changes in the length of the day and the centrifugal force. But this change should then only have acted upon the sea, and therefore, since the sea has flowed towards the equator, the day should have been considerably shorter in the last geological period. We shall see hereafter that there is no known cause which could have produced such a shortening of the sidereal day as would serve to explain what Suess wants to explain. The old theory of refrigeration is scarcely fitted to explain these conditions indicated by Howorth and Suess. Even Suess, who is a zealous adherent of the theory of contraction, is obliged here to seek for another explanation.

Another theory however has come forth in our day, a theory which, no doubt, is destined to play a great part in geology. It is derived originally from the celebrated philosopher J. Kant. In 1754 he wrote a memoir entitled "Untersuchung der Frage: ob die Erde eine Veränderung ihrer Achsendrehung erlitten habe?" In this it is shown that, by reason of the attraction of the moon and sun, the sea is constantly in a movement opposite to the daily revolution of the earth. The friction of the tidal waves against the bottom and coasts of the sea diminishes the force of the axial revolution and works constantly in the same direction, so that the sidereal days must for this reason always become longer and longer. The moon always turns the same side towards the earth because the earth's tidal action on the mass of the moon while still fluid, constantly rendered the axial revolution of the moon slower, until at last the moon was compelled to turn always the same side toward the earth.* In this way also, at some far distant period the earth will come to turn the same side always to the moon. This opinion of Kant's has been recognized as correct by the first physicists of the present day, by men such as Robert Mayer, Helmholtz, and W. Thomson.

There are certain peculiarities in the moon's movements which astronomers are inclined to explain by the assumption that the sidereal day gradually increases by reason of the friction of the tidal wave. But with regard to this we will merely refer the reader to Thomson and Tait's "Treatise on Natural Philosophy," and to a memoir by the first-named author, "On Geological Time" (*Trans. Geol. Soc. Glasgow*, 1868, vol. III. pp. 1 *et seq.*)

In their "Natural Philosophy," Thomson and Tait treat the problem of the earth's axial rotation. They state that there are various forces which may be efficient in altering it,—some make the sidereal day shorter, others make it longer. The latter are preponderant, and among them the tidal wave plays the greatest part, so that for this reason in the course of time the sidereal day becomes always longer and longer. Refrigeration is the most powerful force which contributes towards the *shortening* of the sidereal day, but its action is calculated by Thomson (*Trans. Geol. Soc. Glasgow*, *l. c.* p. 28) at only one six-thousandths of the tidal wave; and this last action cannot be annulled by any of the other forces, which act sometimes in one, sometimes in another, direction (transport of material from higher to lower latitudes, or *vice versa*, accumulation of ice at the poles, etc.), and which in course of time cease to act, the tidal wave acting always for millions of years in the same direction (Thomson, "Geological Dynamics," *Trans. Geol. Soc. Glasgow*, 1869, vol. III, part 2, p. 223).

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* Is it possible that the great abundance of old volcanoes in the moon may be explained by the great change which its axial rotation, and therefore probably also its compression, has undergone?

In this way, therefore, the sidereal day must in course of time always become longer and longer. Now what influence has this upon the earth? If this were fluid throughout, it is clear that it must at once change its form. According as the sidereal day became longer and the centrifugal force diminished, its compression must have decreased. But the old theory of a fiery fluid interior is now rejected by physicists, and Thomson assumes that the earth is on the whole a solid body. Now, will this solid body retain its form without reference to the length of the sidereal day, or will it yield and accommodate itself? The sea, as a matter of course, will at once yield, and, as the centrifugal force decreases, it will sink under lower, and rise under higher latitudes. We know that the earth's present form agrees, at all events in some degree, with the length of the sidereal day. It has at present a compression which about agrees with that which it should have from calculation with its present axial rotation. As it may now be rendered probable that the earth, since it acquired a solid surface, has lost so much of its axial rotation that the sidereal day has become several times longer, the circumstance that the compression suits that agreeing with the axial rotation seems to show that the solid earth has really changed its form. Jupiter and Saturn have a sidereal day respectively of $9^h\ 55^m$ and $10^h\ 15^m$, and a compression of one-seventeenth and one-tenth. In Mars, the sidereal day of which is about $24^h\ 37^m$, observations have not been able to prove definitely any compression. There would seem, therefore, to be a connection between compression and axial rotation. But it may indeed be objected that Jupiter and Saturn are still possibly melted masses.

W. Thomson and Tait seem to be of opinion that the earth will not change its form. They assume that it must have become solid not so many millions of years since, seeing that the compression nearly coincides with the axial rotation.

J. Croll ("Climate and Time," 1875, p. 335; see also *Amer. Jour. Sci.*, 1876, ser. 3, vol. XII, p. 457) thinks that the sidereal day lengthens so slowly that denudation will have time to adjust the form of the earth so as to coincide with the length of the sidereal day. Just as the sea sinks under low latitudes, the continents in the same latitudes will also become lower by denudation, but under higher latitudes the rising sea will protect the land instead of denuding it; and in this way the earth must then, by denudation alone, acquire a form always suitable to its axial rotation. But this is evidently erroneous. Imagine the earth formed of ellipsoidal layers with increasing solidity inwards. When the centrifugal force diminished, equilibrium would be disturbed throughout the whole mass, and in the interior tension would constantly increase. Nay, not even at the surface can denudation alter the compression. For we know from the recent investigations of the deep sea that in this deep sea, far from the continents, no products of weathering are present; only volcanic ashes and cosmical dust are deposited. Thus denudation is not even capable of obliterating the inequalities of the surface, still less the internal tension produced by the lengthening of the sidereal day. And as the day has become considerably longer, the sea ought to be collected towards the poles and the land under the equator, in case the solid earth had not changed its form.

Others think that the earth may actually change its form. The first who expressed this opinion, so far as I can find, is Herbert Spencer. In the *Philosophical Magazine* (1847, vol. XXX, p. 194) he published a small memoir, entitled "The Form of the Earth no proof of original Fluidity," in which he maintains that even the solid earth may change its form, according as the centrifugal force changes. When a body increases in size, the power of resistance to external forces increases only as the square of the dimensions, while the wasting and destructive forces (weight, centrifugal force) increase in the same proportion as the mass of the body, and therefore as the cube of the dimensions. As the size increases we therefore come to a point at which even the most solid body must yield to the forces. We must therefore assume, says Spencer, that the earth, by reason of its size, must yield and change its form, in case the centrifugal force, for example, changes; for the most solid matter known to us, exposed to the same forces which act upon the earth, would overstep the bounds of solidity before attaining a thousand-millionth part of the earth's size. This argument, in Professor Schiøtz's opinion, is not tenable. At any rate, I believe that Spencer is the first who expressed the opinion that even a solid earth can change its form. In the above-cited discourse of 1869,* Peirce says that the lengthening of the sidereal day may be supposed to have altered the form of the solid earth. And Principal Dawson, in his "Story of the Earth and Man" (ed. 9, 1887, p. 291), says that this alteration of form by reason of the lengthening of the sidereal day must have taken place at longer or shorter intervals. So long as the crust of the earth did not yield, the sea will have flowed towards the poles; but when the tension becomes so great that the solid crust bursts, the equatorial regions will sink in and the sea will flow again towards the equator.[*]

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* See *ante*, p. 329, foot-note.

In the *Philosophical Transactions* for 1879, parts I and II, Prof. G. Darwin has published a memoir the results of which are briefly as follows. He assumes that the earth possesses a small degree of plasticity, and calculates the internal friction which the tidal action of the moon and sun produce in such a body. He finds that both the sidereal day and the month have become much longer, that the distance of the moon has increased, that the obliquity of the ecliptic has diminished, and that a great part of the internal heat is developed by the internal friction. Forty-six million three hundred thousand years ago, according to his calculation, the sidereal day was $15^h\ 30^m$, and the moon's distance 46.8 terrestrial radii (against 60.4 at present). But 56,180,000 years ago the sidereal day was only $6^h\ 45^m$ long, the moon's distance only 9 terrestrial radii, and the month only 1.58 day (one-seventeenth of its present amount). The interior heat produced by friction in 57,000,000 years, if applied at once, would suffice to heat the whole earth 1700° Fahr.[†] He concludes that the compression has constantly diminished: "*the polar regions must have been ever rising, and the equatorial ones falling* though as the ocean followed these changes they might quite well have left no geological traces.[‡] The tides must have been very much more frequent and larger, and accordingly the rate of oceanic denudation much accelerated. The more rapid alternation of day and night [57,000,000 years ago, according to Darwin, the year had 1,300 days] would probably lead to more sudden and violent storms; and the increased rotation of the earth would augment the violence of the trade-winds, which, in their turn, would affect oceanic currents.[§]

Tresca (*Comptes Rendus*, 1864, p. 754; 1867, p. 802, etc.) has shown that ice, lead, and also cast-iron, even at ordinary temperatures, may be squeezed so strongly that their interior parts change their relative positions like particles in a fluid. Iron, in the solid state, by strong pressure, is squeezed into cavities and adapts its form to the surroundings. On cutting through such pressed pieces it has been found that the particles or crystals have arranged themselves by a flow-like movement suited to the form of the cavity into which the piece has been pressed.

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[*] Similar opinions are expressed by Dr. E. Reyer ("Die Bewegung im Festen," in *Jahrb. K. K. Geol. Reichs. Wien*, 1880, vol. XXX, pp. 543 *et seq.*). W. B. Taylor, in a memoir "On the Crumpling of the Earth's Crust" (*Amer. Journ. Sci.*, 1885, ser 3, vol. XXX, pp. 249 *et seq.*), expresses himself against the theory of the earth's contraction, and thinks that the lengthening of the sidereal day is the cause of the changes in the crust. A. Winchell, in a memoir on the "Sources of Trend and Crustal Surplusage" (*Amer. Journ. Sci. l. c.* p. 417), endeavors to show that the diminishing centrifugal force has produced foldings in a north and south direction. J. E. Todd, in a paper entitled "Geological Effects of a Varying Rotation of the Earth" (*Amer. Naturalist*, 1883, vol. XVII, pp. 15 *et seq.*), first enumerates the various forces which may act in accelerating and retarding the axial rotation. He assumes that the axial rotation decreases and increases abruptly, that it acts first upon the sea and afterwards upon the solid crust, and that for this reason the sea rises and sinks abruptly in relation to the land.

[†] This heat, produced by the internal friction, must contribute considerably to diminish the secular refrigeration. Lapparent has not taken account of this in the above-cited memoir on the contraction and cooling of the earth.

[‡] In a subsequent article, however, Darwin supposes that the coast-lines will shift in consequence of the lengthening of the sidereal day (*Nature*, Sept. 2, 1886, p. 422).

[§] These numerical values make no claim to represent the actual values; they are merely the maximum values, which according to Darwin are generally possible.

We must here also refer to the interesting investigations of Reusch upon pressed conglomerates. Under the strong pressure which has acted in the earth's crust, the pebbles in conglomerates are squeezed out into lance-shaped bodies, and these bodies have even become folded. (See Reusch, *Silurfossiler og pressede Konglomerater i Bergensskiferne, Univ. Progr. Christiania*, 1882, pp. 15, 117.)

By reason of the enormous pressure which prevails in the interior of the earth, it must be supposed that masses from a certain depth are more or less in a plastic state. A constant lengthening of the sidereal day will cause the equatorial parts to increase in weight. So long as the earth does not change its form, a constantly increasing weight will act upon the internal mass from lower towards higher latitudes. There is, as Darwin indicates (*Nature*, September 2, 1886, p. 422), reason to believe, that finally, when the tension has reached a certain amount, the earth will yield. A flow of plastic mass will be directed towards higher latitudes, and persist until the earth has approximated to the form suitable to the length of the sidereal day. When we consider the numerous testimonies as to changes in the solid crust of the earth, and the frequent elevations and depressions of the solid land relatively to the sea, we may well agree with Darwin, that this view may claim more probability than that of Thomson and Tait.

Wertheim has proved by experiment (according to Fock, *Lärobok i Fysiken*, Stockholm, 1861, pp. 202, 219) that there is really no definite limit of elasticity for any matter, but that they all, by the action even of quite feeble forces, undergo small persistent changes, especially if these forces have acted for a somewhat long time. When with feeble pressures we find no permanent change of form, this is because the force has not acted long enough. The action of the force, therefore, when it has a greater resistance to overcome, depends upon time. By "tension," says Schiøtz (*Lærebog i Fysik*, Christiania, 1881, p. 65), "lengthening constantly increases, although very slowly, after it first commences; therefore a weight which has acted for a short time will not produce persistent elongation such as it would be if it were allowed to act for a longer time. This applies not only to tension, but generally; and hence it comes about that wires slacken in course of time, and that beams bend little by little. A thread is worn out by less force when the pressure is long continued than when it is applied for a shorter time."

It seems to me that here we have a force which may be capable of effecting displacements in the solid earth. I believe that this is "the unknown force from below" which has elevated the mountains of western North America, and to which Dutton appeals. The sidereal day increases very slowly. The sea adjusts itself in accordance with the smallest change in the length of the day and rises slowly under high latitudes. But the solid earth offers resistance to change of form, and begins to give way only when the tension reaches a certain amount. When this period has arrived the crust also begins to rise under high latitudes. Under lower latitudes the movement takes place in the opposite direction. The solid earth probably is a little behind the sea in its movements, and while the sea moves evenly and uninterruptedly, the change of form in the solid earth must perhaps take place more spasmodically, with intervening periods of rest, during which new tension is set up.

"The elevation of mountains," says A. Geikie ('Text-book of Geology,' 1882, p. 917), is in most cases due to a long succession of such movements;" and (*l. c.* p. 919) "the elevation of mountains, like that of continents, has been occasional, and so to speak, paroxysmal." Upheavals of the crust take place repeatedly along the same fissure (see, *e. g.*, Brögger, *Bildungsgeschichte des Kristianiafjords*, 1886, p. 78). Something of the same kind occurs in volcanic eruptions. Volcanoes rest for a shorter or longer time between the different eruptions. Basaltic layers alternate with sedimentary deposits. Earthquakes are a consequence of a tension set up, to which the crust suddenly yields. All this indicates that the crust of the earth does not immediately accommodate itself to the forces, but that it yields only when the constantly increasing pressure has approximated to a certain amount. It seems moreover to follow from geological investigations that there are periods in the earth's history when changes have taken place on a larger scale than usual. In his "Text-book" above cited (pp. 197, 198) A. Geikie refers to the great eruptions ("fissure-eruptions") which have taken place in both the Old and the New World, in which melted masses burst forth from numerous fissures and overflowed thousands of square miles. The Vulcanism of the present day seems feeble in comparison with these gigantic eruptions.

We will now pass to the inquiry whether these changes in the form of the earth may stand in any relation of dependency to the periodical variations of the eccentricity of the earth's orbit. We start from the fact that Thomson and Tait are right when they say that the tidal wave is the most powerful of the forces which contribute to change the length of the day. But besides the tidal wave of the sea, the interior friction accepted by Darwin, ("the bodily tides") is also effective. Both, of course, are dependent upon the distance of the sun and moon; and we therefore examine whether the tidal action of these bodies upon the earth varies with the eccentricity of the earth's orbit. It appears from

Darwin's investigations that the lunar tides in very distant periods must have been much greater than now. I disregard this, as the time in question is so long ago, and because the profiles, which later on will combine in curves for the eccentricity of the earth's orbit, come down from a past geologically so near. When I perceived that the dependence of the tidal wave upon the eccentricity might be of geological importance, I applied to the observer, H. Geelmuyden, who with his usual kindness has given me the following answer:

"The action of the eccentrictity of the earth's orbit, e, upon the force which produces tide and ebb, and which, for the sake of brevity, I will call the tidal force, is as follows:—Let r be the sun's distance, then the sun's tidal force is

$$P=\frac{C}{r^3},$$

where C represents the sun's mass and r the earth's orbital radius. In the course of the year r varies; but the mean value of $\frac{1}{r^3}$ is found by a simple integration to be $\frac{1}{a^3(1-e^2)3/2}$, where a is the unchangeable mean distance. Consequently, the annual mean value of the sun's tidal force becomes

$$P=\frac{C}{a^3(1-e^2)\,3/2}=\frac{C}{a^3}(1+3/2\,e+\quad.\quad.\quad.).$$

"From this it follows that, when the eccentricity increases, the tidal force also increases; if the former increases Δe and the latter ΔP, then

$$\frac{\Delta P}{P}=\frac{3e\,.\,\Delta e}{1-e^2}=3e\,.\,\Delta e,$$

as $1-e^2$ in the denominator is of no significance. If past times be $3e=\frac{1}{20}$, and $\Delta e=-0.00043$ per thousand years, then $3e\,.\,\Delta e=-0.00002$, or the sun's tidal force decreases every for thousand years by $\frac{1}{50000}$ of its value. When the eccentricity has its greatest possible value, 0.0667 according to Leverrier, $e^2=0.00445$, $3/2\,e^2=0.00667$, then $P=1.00667\,\frac{C}{a^3}$; or the difference between maximum and minimum is $\frac{1}{150}$ of the value.

"The monthly mean value of the moon's tidal force will of course, in the same way, be dependent upon the eccentricity of the moon's orbit; but as this is not subject to any noticeable secular variation, it does not come under consideration. On the other hand, the moon's mean distance is dependent, although only to an extremely small extent, upon the eccentricity of the earth's orbit, namely so that the moon's tidal force becomes

$$P'=\frac{C'}{a^{3'}}(1-q\,.\,3/2\,e^2).$$

"Here therefore the eccentricity acts in the opposite direction, namely, so that the force diminishes as the eccentricity increases; but as the factor q, by which $3/2\,e^2$ is multiplied, is only about 3/400, while the magnitude outside the brackets $\frac{C'}{a'^3}=5/2.\,\frac{C}{a_3}$ (the lunar tides being in proportion to the solar tides most nearly as 5 : 2), its action upon the whole tidal wave is $\frac{3}{400}\cdot\frac{5}{2}=\frac{1}{53}$ of the former."

Thus we see that the tidal force rises and sinks with the eccentricity of the earth's orbit. It varies by about $\frac{1}{525}$, of its value from the highest to the lowest eccentricity. This force is the most important force for the alteration of the day, and it makes it longer. The most important force for shortening the day, according to Thomson, will be the refrigeration of the earth, but he has calculated its value at only $\frac{1}{6000}$ of the tidal force and (he has only taken into account the marine tidal wave). If, therefore, the tidal force diminishes and increases by $\frac{1}{525}$ of its value, this periodical variation can not compete with forces which act in the opposite direction; and we may therefore conclude that the sidereal day is constantly becoming longer, but that its increase is periodically stronger and weaker. It increases in length more and more rapidly so long as the eccentricity of the earth's orbit increases, more and more slowly so long as the ecccentricity diminishes. In other words, the centrifugal force diminishes and the equatorial regions increase in weight more and more rapidly under an increasing, and more and more slowly under a diminishing, eccentricity.

As has been stated, there prevails, even among physicists, a disagreement as to how far the earth will change its form in case the centrifugal force varies. Thomson is most inclined to believe that it will not; Darwin is of opinion that it will. And among other physicists whom I have consulted a similar divergence prevails upon this point. One thinks that a lengthening of the day even by several hours will be incapable of altering the form of the solid earth; another believes that the solid earth will probably change its form just as easily as the sea. And with regard to the rapidity with which the sidereal day lengthens, opinions are just as much divided. Darwin regards as possible, variations much greater than those which agree with the action of the tidal waves calculated by Thomson for recent times. It is therefore clear that this problem can hardly yet be finally solved, and that different hypotheses will be for the present admissible. We will therefore select that which is best fitted to explain the facts, assuming that the variation of the tidal wave with the eccentricity of the orbit may possibly be the cause of the periodical displacement of coast-lines. But we put forth this hypothesis with all possible reserve. Divergencies of opinion between the most esteemed physicists upon this matter, and the neat manner in which the hypothesis is supported by many facts, alone give us the courage to put forward conjectures which many will probably regard as not only bold, but even improbable.

The motive force of alterations in the form of the earth should therefore be periodically variable with the eccentricity of the orbit. The sea, which is fluid, adjusts itself at once in accordance with the smallest change in the length of the day. But the solid earth offers resistance; and the day lengthens slowly and imperceptibly. With such small forces, as we have already seen, it becomes a matter of time. Even small forces can produce an effect, if they only have time to work in. It is therefore probable that the solid earth will be behind the sea in its movements. Some time will elapse before the "crust" and the inner plastic mass begin to yield. The ground under a building often begins to give way only when the building has stood for some time. If then the solid body of the earth lags behind the sea in its movements, and the movements both of the sea and of the solid earth occur periodically more strongly and more feebly, because the motive force is stronger and weaker according as the eccentricity of the orbit increases or diminishes, it was conceivable that the coast-lines would come to be displaced up and down once for every time that eccentricity increases and diminishes. For there must be the greatest probability that the solid earth may yield at one place or another when the tension in the interior becomes strongest.

It is important now to examine whether the action of the tidal wave and variations in its strength are great enough to explain the displacement of the coast-lines. This is a mathematico-physical problem, and it is not for me to solve it. I put it as a question for the decision of competent men, and shall confine myself to the following remarks:

If the sidereal day has once been several times shorter, and the earth at the time was a solid body, the tension and pressure in its interior will increase with the length of the sidereal day, until finally the tension becomes so great that the earth begins to yield. It will then accommodate itself, if not in its entirety, at least partially, until the tension is equalized, at any rate in part. Perhaps then a state of repose will occur, during which a new tension will accumulate, which may introduce a new change of form. And these spasmodic changes of form in the body of the earth when strained to the limit of its power of resistance would occur precisely when the eccentricity had approached its highest value, and the tension increased most rapidly, or some time afterwards. Under such circumstances, possibly, the small variation which the tidal force undergoes with the eccentricity would turn the scale, and determine the time for the changes of the solid earth.

Thomson says (*Trans. Geol. Soc. Glasgow*, 1868) that it is still hopeless to attempt to solve the question of how rapidly the sidereal day lengthens, by means of tidal action. By way of trial he calculates (*l. c.* p. 26) the action of the existing tidal wave to be so great that the earth in one hundred years should be retarded one hundred and eighty seconds, with which corresponds a lengthening of the day of 0.01 second; and if we take this retarding power, for the sake of simplicity, as constant, the day, in one hundred thousand years (the time which is on the average occupied by an oscillation of the eccentricity) should become ten seconds longer. Moreover, Thomson reckons only the marine tidal wave. To this should now be added Darwin's "interior tide," his "bodily tides," which I know no means of calculating. For many millions of years, when the moon was nearer and the tidal action considerably stronger, the day also increased more rapidly. But nowadays its increase is undoubtedly much slower, and we can not expect great general changes of level in a short time from this cause.

To a lengthening of the day by ten seconds (according to Todd,*) corresponds a shortening of the equatorial radius by 5.6 meters and a double lengthening of the polar radius, therefore, by 11.2 meters. What value the lengthening of the day had in Tertiary times we do not know. It can not well have been remarkably greater than in recent times. And it seems, therefore, in any case to follow, as stated above, that the vertical displacement of coast-lines can scarcely have been in genera more than a few meters under any oscillation, in case our attempted explanation is correct. Therefore we must now see whether the displacement of coast-lines was so very considerable.

We must first examine how much is deposited in each precessional period and how great is the thickness of the stages. The thickness of the deposit depends, in the first place, upon the situation of the place, whether it lies near or far from the land or the mouths of rivers, and upon the nature of the deposit. Chemical deposits are commonly thinner than mechanical ones. As a mean number for each precessional period (twenty to twenty-one thousand years), I have obtained the following values for the different kinds of alternating deposits:

Marl and siliceous limestone, from 0.6 to 2.2 meters.
Clay and siliceous limestone, 1.3 meters.
Marl, gypsum, siliceous limestone, (marine,) 1.3 to 1.4 meters.
Ditto, fresh water, 2.8 to 2.9 meters.
Limestone and marl, 1.8 to 2.5 meters.
Marl, argillaceous limestone, (ironstone,) sandy marl, 2 meters.
Sand, calcareous sandstone, (marine,) 2 to 2.3 meters.
Ditto, fresh water, 3 meters.
Sand, clay, ferruginous sandstone, (marine,) 5 to 6 meters.
Clay, limestone, ironstone, sand, 5 to 7 meters.
Sand, marly clay, ferruginous sandstone, lignite, up to 30 to 60 meters.

In each stage, when there has only been one oscillation of the sea, there are usually four or five such alternating deposits, so the thickness of the stages is generally but small. I may cite the following examples. First, from the Paris basin: The Calcaire Grossier, which

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American Naturalist, 1883, vol. XVII. p. 18. (Or as stated, 1 minute of daily lengthening is equivalent to 110 feet of equatorial depression.)

represents twenty-five deposits and several (five to six) oscillations, is only 31.5 meters thick; Sables de Beauchamp, 13 to 14 meters; the Calcaire de St. Ouen, with ten alternating deposits, is only 6 to 7 meters; marine gypsum, 16 to 17 meters; palustrine gypsum, 20 meters; Sables d'Etampes, 11 to 12 meters.

In the Isle of Wight the beds are thicker, but also richer in mechanical deposits: Plastic clay, 26 meters; London clay, 61 meters; Lower Bagshot (sand, clay, lignite, and ferruginous sandstone, with seven alternating deposits), in all, 200 meters; Bracklesham, of the same kind as the preceding and without any alternation, 33.5 meters; Middle Bagshot, 91 meters; Upper Bagshot, (sand, without alternations), 37 meters; Lower Headon, 21 meters; Middle, 7 meters, and Upper Headon, 26 meters; Osborne Series, 19 meters; Bembridge limestone, 7.6 meters; Bembridge marl, 23 meters; and Hempstead Series, 52 meters.

From Belgium we have the following thicknesses: Montien (coarse limestone with foraminifera), 93 meters; Heersien, 32 meters; Landenien, about 60 meters; Yprésien, 140 meters; Bruxellien, 50 meters; Laekenien, 10 meters; Wemmelien, up to 80 meters (only determined by boring); Tongrien, 21 meters; Rupelien, 60 meters; Anversien, 3 to 4 meters (but near Utrecht, in an artesian well, 130 meters).

The thicknesses in the basin of Mayence are as follows: Alzeyer sand, 50 meters; *Septaria* clay, 50 meters; Elsheimer sands, 60 meters; *Cyrena* marls, 40 meters; *Cerithium* limestones, 25 meters; *Corbicula* limestones, 25 meters; *Litorinella* clay, 20 meters. In Italy, Seguenza gives the following thicknesses: Bartonien (in part conglomerates, and perhaps several oscillations), 300 meters; Tongrien, 50 meters; Langhien, Astien, and Saharien, each 200 meters; Zancleen, 300 meters. The Swiss Mollasse, which is a shore formation, is so thick that it forms whole mountains; but, according to Charles Mayer-Eymar, the Aquitanian has a much greater and, indeed, quite exceptional thickness near Bormida, in Tuscany. Here we find (probably inclined from the first) fresh-water and superiorly marine shore formations with manifold alternations of sandstone and shales, the thickness of which, although it has not been exactly measured, is believed to be 3,000 meters, and all supposed to be formed in the Aquitanian period. And the same stage, according to Gümbel, has a similar thickness in Bavaria. Etna, which is 12,000 feet high, has been built up by volcanic eruptions in the most recent geological period, and since the Mediterranean had acquired a fauna essentially the same as at the present day.

The formation of the Mediterranean, with its strong vulcanism, has been distinguished, according to Suess and Neumayr, by very considerable displacements of the earth's body. The Egean Sea and the Adriatic have been formed by depressions in the latest geological period. Under such circumstances very thick deposits may be formed near land in a short time. Eocene marine deposits are uplifted 21,000 feet above the sea in folded ranges (*e. g.*, in Upper Asia). But all these are only local disturbances. If we turn, on the other hand, to localities where the conditions have been more quietly developed, we find, as may be seen from the preceding statements, that the stages have only a small thickness. The deposits which form them are partly fresh-water formations, partly formations from shallow seas; there are no well-marked deep-sea formations among them. They are to a great extent—perhaps for the most part—formed in inland seas and bays, in basins which were separated by banks from the open sea. We may arrive at this conclusion from the circumstance that salt-water and fresh-water formations so frequently alternate in the Tertiary deposits; for it is only when stratified formations take place in basin-shaped depressions that fresh-water basins can be formed when the sea retires.

And if we have deep basins which are separated by banks from the open sea, a rising or sinking of the shore-line by some few meters will be sufficient to submerge or lay dry the banks. The deep basin will then alternately be salt and fresh. And a rising of the sea by a few meters will likewise suffice to cause the formation of thick salt-water deposits in the basin. If the bank then again rises a few meters, the basin will remain fresh, and thick fresh-water beds can be deposited above the marine beds. In this way the formation of alternating salt and fresh-water beds may continue, under small displacements of the coast-line, until the basin is filled up.

It would seem to be more difficult to reconcile the hypothesis with the very considerable elevations which particular countries have undergone in the period which has elapsed since the Glacial period. Thus near Christiania and Trondheim the highest trace of the sea from the Post-glacial time is situated 188 meters above the sea. But in other parts of our country the highest marine terraces are much lower, so that it would seem as if the elevation has not been everywhere equally great. It seems to have been weaker and weaker outwards from the center of the country. In southern Sweden and Denmark it has also been inconsiderable in the same period. Penck has shown ("Schwankungen des Meeresspiegels," in *Jahrb. Geogr. Ges. München*, Bd. VII.) that an inland ice exerts an attraction upon the sea, which, for this reason, stands higher on the coast of a country, when the land is covered with ice. The melting of the inland ice may therefore have caused the sea on our coasts to sink somewhat, but the difference between the situations of the highest marine traces in the different parts of Scandinavia is so great,* even in neighboring localities, that it could not be explained in this way; and the most probable explanation would be that the land has risen in different degrees at different places.† It is also a probable supposition that the crust has not everywhere the same power of resistance to the interior pressure, and especially that the plastic mass may press in under the more yielding parts of the surface. We have a striking example of this in the laccolites noticed in North America. Eruptive matter is here pressed up from below, and has lifted the bed into dome-shaped vaults, so that the elevations have been different in degree in different places, and greatest in the middle of the domes. We may imagine that similar forces, but on a much larger scale, have contributed to the elevation of Scandinavia;—that Scandinavia is, *sit venia verbo,* as it were a laccolite on a larger scale. We must in the next place remember that the changes of the earth's surface which have taken place in the Tertiary and Quaternary periods, however great they seem to be in our eyes, are inconsiderable in relation to the whole mass of the earth. Even small forces, where they act upon a great mass, may produce very considerable local effects, provided that the changes do not everywhere occur upon the same scale. If we consider that in this way the elevations are not everywhere equally great, then a depression of the equatorial belt of only a couple of meters will suffice to cause many such countries as Scandinavia to rise many meters, and there will still remain pressure which is not exhausted.

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* See E. von Drygalski, "Die Geoiddeformationen der Eiszeit," in *Zeitschr. d. Ges. f. Erdkunde in Berlin*, 1887, Bd. XXII, pp. 169 *et seq.*

† A similar unequal elevation has probably also taken place during earlier periods of elevation. In the Bergen conglomerate, the old shales are situated at a higher level, the farther one goes from the shore. (See Kjerulf, *Udsigt over det sydl. Norges Geologi*, Christiania, 1879, pp. 154-156, etc.; and Helland in *Arch. f. Math. og Naturv.* Christiania, 1881, Bd. VI. p. 222.)

Of course it is not said that whenever the eccentricity has attained a high value, Scandinavia will rise to an equally great amount. If the elevation has been great in a given period, it is probable that the next period of elevation will have more difficulty in upheaving the previously elevated land. The position of the weakest points will vary. The next time, perhaps, the elevation will chiefly affect other localities. If we consider the Tertiary formations in Europe, we see that the series of deposits is nowhere complete. It is only by combining all the deposits formed at different places that we can obtain a complete outline. In part, this is certainly due to the fact that the changes of form in the solid earth have not taken place simultaneously everywhere. The great eccentricities produced upheavals at different times in different places.

There is lastly a circumstance of great importance which may here be indicated, and which shows how quietly oscillations take place under normal conditions. Although according to our hypothesis, the radii of the higher latitudes constantly lengthen, while those of lower latitudes are shortened, yet through long geological periods coast-lines return repeatedly, during their displacements, to their old position. Thus A. de Lapparent (*Bull. Soc. Géol. France,* sér. 3, vol. xv. p. 400) says: "I have indicated, in the Cotentin, an agreement between the actual shores and those at which the sea stopped at various epochs of geological history. I have there shown shore-lines reproduced, almost without variations of altitude, in the Hettangian, Sinemurian, Liassian, Cenomanian, Danian, Parisian, Tongrian, Pliocene, and present epochs, . . . and that eight or nine times at least, since the Primary era, the coincidence of the shores has been reproduced at the same point;" and in the same work (p. 277) he says: "It is only by tens of meters that on the coast of the Cotentin we must reckon the differences between the successive levels of the seas, from the Trias down to the present day." Here we see that the variations of level have taken place with great regularity. The sea has risen, and later on the land has been elevated; and these alternate risings and sinkings have occurred with such regularity that the coast-line again and again, at long intervals, has returned about to its old place.

After this there seems really to be a possibility that our hypothesis is sufficient to explain the displacements of the shore-lines which have taken place. We have hitherto considered the conditions under high latitudes. Under lower latitudes all may sink. Here "Horste" may be formed such as Suess supposes, and as to the occurrence of these localities Lapparent's criticism is unsatisfactory. He has attacked Suess's theory of "Horste" in its entirety, but he has criticised it specially only for such localities (Colorado, Vosges, Black Forrest, and the central plateau of France) as lie under high latitudes. The localities named have (according to Lapparent) risen more than their environment, which also is quite in accordance with the opinion above developed. But under lower latitudes, when a general sinking takes place in the course of time, resistant parts will form true "Horste" in Suess's sense. And it scarcely goes against our hypothesis to assume, with Suess, that the Indian Ocean is formed by depression, and that Africa, Madagascar, India, &c., are "Horste," parts of the crust which have remained in position, or which have sunk less than the neighboring regions. In these countries, so far as their geology is known at present, there seem to be few marine formations of the Mesozoic and Cainozoic epochs.

I have said above that the different parts of the crust may be assumed to have different powers of resistance against the interior pressure. This may indeed be concluded from the fact that the surface is uneven, and that old (originally horizontal) formations have been upheaved unequally at different spots. In other words, there is an inequality of the surface, which has a deeper cause than the operation of eroding forces.

Changes of the earth's crust in reality happen in the most various degrees at different times. The greatest convulsions occur in the folded mountain-chains, and this has been the case in all geological periods. It is worthy of note that places where great foldings took place in ancient times seem to have been subsequently unaffected by processes of folding.* For upon the abraded summits of old folds there often lie other old formations in an undisturbed horizontal position. The most highly folded chains are also those in which plications have been continued to the latest time. Along both sides of the Pacific Ocean from Cape Horn to the Aleutian Islands, and opposite to this along the east coast of Asia as far as the Sunda Islands, strike mighty chains associated with series of volcanoes; and from the Himalaya through the Caucasus, Balkans, Pyrenees, and Atlas a similar series of vast chains stretches through localities which are often volcanic. These highest mountains of the earth are also the youngest; they are still the least affected by the tooth of time.*

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* If the earth's axis, as some astronomers (*e. g.*, Gylden) think, may shift its position in the course of time, calculations as to the pressure produced by the lengthening of the day will also change, and the situations of the parts of the crust exposed to the greatest pressure will also shift.

But these strongly folded localities are of small extent in comparison with the other parts of the earth's surface. On both sides of these folds there are great plateaux and plains, quite or nearly without any plications, and on the whole with undisturbed horizontal beds. These are Suess's "tables" (*Tafeln*). Africa, Western North America, (in the Eastern there are no younger plications than from Carboniferous times,) Brazil, Australia, Arabia, Persia, India, Siberia, and Russia, are such "tables," in which the crust is much less disturbed. And no doubt the same thing applies to the sea-basins, or at any rate to the greater part of them.

When the sidereal day lengthens, the sea at once adjusts itself to the new conditions. It sinks under the lower, and rises under the higher latitudes. According as the interior pressure upon the crust increases towards the poles, the opposite pressure upon the sea-bottom also increases in the same regions, because the sea rises. But the parts not covered by the sea are exposed alone to the increasing pressure from the interior without any exterior counterpressure being developed. Under lower latitudes the same thing takes place. According as the crust increases in weight the sea sinks, and the pressure upon the interior increases more rapidly in the continents, where nothing is removed, than in the sea, where the level of the water sinks. Therefore I think that the continents are weak points. The sea's movements weaken the effects of the diminishing centrifugal force for all parts covered by the sea, but the pressure acts with undiminished force everywhere on the solid land, both under low and under high latitudes. Whatever the cause may have been that originally determined the distribution of land and sea upon our globe, it seems to me that we may reasonably assume that the sea's mobility is a preservative force, which perhaps has contributed to make the continents and oceans, broadly speaking, retain their form from the most ancient times until now.

There is also reason to believe that the continents may yield more easily than the bottom of the deep sea, and that they may rise and sink more readily. And they are also separated from the depths of ocean by lines abounding in volcanoes, lines of weakness, where the connection between the parts of the crusts seems to be weaker than elsewhere.

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* The summary here given is founded upon Suess's interesting studies in his great work "Antlitz der Erde."

Processes of plication may also perhaps be a consequence of the movement of "tables" not being of the same kind on both sides.

But the boundaries between the deep ocean and the foot of the continents do not everywhere coincide with the existing shore. Along the coasts there are often shallow tracts in the sea. These are the foot of the land which the sea has flooded, and the great deep sea only commences farther out.

The Trias period has received its name because it shows a distinct triple division. It commences with fresh-water and littoral formations, upon which follow formations of deeper water, and then closes with fresh-water and shore formations. At its commencement the land was high relatively to the sea; as it went on the sea rose higher and higher; then the land again began to rise, and at the end of the period it was again high in relation to the sea. And these great changes in the situation of the coast-line were no doubt effected by means of many smaller oscillations.

But just as it is with the Trias, so is it also with other geological formations. They commence with littoral formations (there is often a conglomerate at the bottom); these are followed by deeper marine formations, and at the close we have again shore formations. The name of Trias would therefore really apply to all of them. The first person to call attention to this remarkable triple division of formations would seem to have been Eaton. It was subsequently discussed by J. S. Newberry in his memoir entitled "Circles of Deposition in American Sedimentary Rocks" (*Proc. Amer. Assoc.*, 1873, vol. XXII, p. 185), and Hull (*Trans. Geol. Soc. Glasgow*, 1868, III., pt. 1, p. 39); see also A. Geikie, "Text-book of Geology," p. 498, where further references to literature will be found. Principal Dawson called these tripartite periods "cycles," and in his "Story of the Earth and Man" he established the following cycles of this kind: (1) Cambrian; (2) Lower Silurian; (3) Upper Silurian; (4) Devonian; (5) Carboniferous; (6) Permian; (7) Trias; (8) Lower Jurassic; (9) Middle Jurassic; (10) Upper Jurassic; (11) Cretaceous; and (12) Tertiary.* It appears therefore that these cycles are periods of long duration; each of them has certainly lasted several hundred thousand years. And in the middle of each cycle the great overflows of the sea have attained their highest point. The cycles alternate with continental periods. During the elevation of the land the horizontal position of the strata was often disturbed, so that the deposits of the new cycle lie unconformably upon the older ones.

In this way the development has gone on, at any rate in the northern hemisphere. Mojsisowics, Suess, and others have pointed out that it has taken place simultaneously in the same direction in Europe, Asia, and North America. These great changes have taken place over the whole of the northern hemisphere, and on both sides of the oceans they have constantly had the same direction. And the same geologists have justly insisted that this law is one of the most remarkable results of geological investigations.

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* We shall see hereafter that this formation includes two cycles.

The development of organic life, as we now know, has gone on uninterruptedly from the earliest times. There has certainly never been any general destruction, never any completely new creation. The new has developed from the old through transitional forms and in the course of millions of years. If we knew all the deposits which have been formed it would be impossible to draw any boundaries between geological formations. One would imperceptibly pass over into the other. The boundaries between formations correspond with great gaps in the series of beds. In the time which intervened between the youngest bed in an older and the oldest in a younger cycle, the land in the northern hemisphere lay so high that no marine deposits were formed in the parts of the earth's crust which are accessible to our investigations. Nevertheless the development of living forms went on its even course. But when, after a long time, the land was again submerged, the life in the sea had changed, and beds with new fossils were deposited upon the old ones. And it is from the animal remains of marine deposits that the formations are determined. Hence, the sudden change of fossils where a new formation commences is not due to any catastrophe, but simply to a shorter or longer interruption in the formation of deposits in the parts of the earth which we are able to examine. There is no doubt that there are transition beds between formations, but they lie concealed from us at the bottom of the sea. It is only in certain strongly plicated chains that these beds are upheaved and can be examined. Thus in the Alps there are transitional beds between the Cretaceous and Tertiary, between the Permian and Trias, etc.*

If we should attempt to establish geological formations by the aid of the known remains of terrestrial animals and plants, the boundaries of these would not coincide with those which are defined by marine animals.† Thus, to mention an example, the appearance of Dicotyledons does not coincide with the boundary of any formation; but they first appear with a number of forms in the Upper Cretaceous period (Cenomanian).

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* "As long ago as 1846, Darwin, in his observations in South America, showed that certain assemblages of fossils presented a blending of characters which are of Jurassic and Cretaceous age, respectively. Since that date, the study of the fossil faunas of South Africa, India, Australia, New Zealand, and the Western Territories of North America has furnished an abundance of facts of the same kind, showing that no classification of geological periods can possibly be of world-wide application." (J. W. Judd, presidential address to the Geological Society, 1888, and *Nature*, March 1, 1888, p. 426). See also Mojsisowics, *Die Dolomitriffe Südtirols und Venetiens*, Vienna, 1879, p. 36; and von Hauer, *Die Geologie*, Vienna, 1875, p. 515.

† "The growth of our knowlege concerning the terrestrial faunas and floras of ancient geological periods has constantly forced upon the minds of many geologists the necessity of a duplicate classification of geological periods, based on the study of marine and terrestrial organisms respectively." (J. W. Judd, *loc. cit.* p. 427.)

From Tertiary times, with the exception of the deposits in the great mountain-chains, we know only formations of shallow seas. The Tertiary deposits which correspond to the deep-sea strata of older formations, and which were deposited farther from the land during that period without their formation being interfered with by the numerous minor oscillations of the coast-lines, still remain for the most part concealed from us in the sea.

Land-formations, fresh-water and littoral formations such as we have in abundance in our Tertiary basins, are greatly exposed to destruction, for they are more frequently elevated above the protecting sea. In the older cycles such formations are more rare, probably to a great extent because they have been destroyed by denudation. We may therefore conclude that the Tertiary formations would much more resemble those of the older cycles if our knowledge of them all were equal. Of the older cycles we often know especially the deep-water formations, of the youngest chiefly those of more shallow waters. At some far-distant period the exposed Tertiary formations will come to equal those which are now visible from older cycles.

Dawson (*l. c.* pp. 176–179) expresses the notion that the remarkable regularity with which such cycles recur may perhaps have a cosmical cause and be conditioned by one or another astronomical period. But he seems afterwards to reject this idea, because the Palæozoic cycles have deposits which are four or five times as thick as the Mesozoic (*l. c.* p. 195), and we might therefore believe that more time must have been occupied in their formation. But, on the other hand, he notes that in Palæozoic times changes in the organic world went on much more slowly in relation to the formation of deposits than subsequently, so that the fossils extend through greater thicknesses of strata than in the thinner, newer cycles. If I were to judge from these facts adduced by Dawson, I should come to a different conclusion; I should regard it as a probable supposition that the formation of deposits went on more rapidly in Palæozoic times than later on. If the moon at that time were nearer to us and the sidereal day shorter, as Darwin thinks, the tidal wave must both have been stronger and have acted more frequently than at present. The coasts would be destroyed much more rapidly, and the sea would have much more material to deposit. A cycle of this period would be thicker than the younger cycles, and the fossils would extend through a greater thickness of strata than in the latter. For I see at present no probable ground for the supposition that the development of new species would be accelerated in the same degree as the formation of deposits.

There is therefore reason to assume that it is owing to these great changes in the form of the earth, occurring at long intervals, that we can distinguish between geological formations. But such great changes in the distribution of land and sea must necessarily also bring with them considerable changes of climate, and at the same time also changes of living forms. I have already, in one of my memoirs, put forward the opinion that the glacial period had its origin in a change of the distribution of land and sea. If the land gained a great extension in the middle and higher latitudes, especially if there should be a formation of bridges across the sea such as the supposed bridge through the Faröes and Iceland from Scotland to Greenland, the warm sea-currents would be excluded from the higher latitudes. The northern seas would then become icy seas, and where the snow-fall is sufficiently inland ice would be formed. In a memoir entitled "Natürliche Warmwasserheizung als Princip der klimatischen Zustände der geologischen Formationen" (in *Abhandl. Senckenb. Gesellsch.* vol. XIII, p. 277 *et seq.*), J. Probst (like Sartorius von Waltershausen, in his *Untersuchungen über die Klimate der Gegenwart und Vergangenheit*, 1865) has with justice pointed out the great importance which warm sea-currents have, and have had, in rendering milder the climate of high latitudes.

It has been generally accepted among geologists that during the older formations animal and plant life was more uniform over the whole earth than at present. But this opinion must be changed according to recent investigations. Thus J. W. Judd says (*Nature*, March 1, 1888, pp. 424 *et seq.*), with regard to the oldest fossiliferous deposits (the Cambrian): "Even at that early period there were life-provinces with a distribution of organisms in space quite analogous to that which exists at the present day." Examples of geographical provinces are indicated by him in the Silurian, Trias, Jura, and Cretaceous; and he says further: "I believe that the study of fossils from remote parts of the earth's surface has abundantly substantiated Professor Huxley's suggestion that geographical provinces and zones may have been as distinctly marked in the Palæozoic epoch as at present."

Most deposits of ancient times belong to periods in which the land lay low in relation to the sea, and the difference between the geographical provinces is far less in the great depths of the sea than near the shores and on the solid ground. It has also hitherto been a general theory that the climate in old times was warmer and more uniform over the whole earth than now. The further we go back, it is said, the warmer it was, and this has been regarded as connected with the interior heat of the earth. The Glacial period was an interruption of the continuity of its gradual cooling. In periods of overflow, when the land lay low and the sea had great extension under high latitudes, warm marine currents had much easier access to the poles than during continental periods. As we now know most about the deposits formed during periods of overflow, and as most of the deposits of continental periods are either removed by denudation or concealed under the sea, it is still probable that the deposits of older cycles might show less strongly marked geographical provinces, and, as a rule, bear witness to warmer climates even under high latitudes. But the great changes in the distribution of land and sea compel us to assume that, hand in hand with them, occurred a periodical alternation of climate, which has been far greater and more radical than the change produced by the precessional periods.

Ramsay, Croll, J. Geikie, and others have thought that they found more or less certain traces of Glacial periods in the older formations (see, *e. g.*, J. Geikie, "The Great Ice Age," ed. 2, 1887, pp. 566 *et seq.*). Some of these traces seem to prove that, at any rate, there have been more Glacial periods than the Post-tertiary one. Nevertheless von Richthofen remarks (*Führer für Forschungsreisende*, p, 362) that these supposed traces of Glacial periods are perhaps only a phenomenon of abrasion, and that the action of the waves upon the shore could produce conglomerates with striated stones. As regards these supposed old Glacial periods, the most certain traces (see J. Geikie, *l. c.*) appear to be furnished by the Devonian Sandstone, "Old Red," in England and Scotland, by the commencement of the Carboniferous period (Scotland), by the Permian conglomerate (England), and by the Eocene (Switzerland). The most striking evidence (with striated stones) is from the periods when the land had great extension.

As regards these great overflows, it must be remembered that it is only in folded chains and in strongly elevated regions (*e.g.* in the Alps, Himalaya, Colorado, etc.) that sea-formed deposits of the later and latest geological periods occur at very considerable elevations above the sea. These great elevations, if we consider them in relation to the whole, can only be regarded as quite local phenomena. At the time when the deposits were formed they lay much lower, and when we now find an alternation of marine and fresh-water deposits in such formations we must not suppose that the sea rose and sank in relation to the land by thousands of feet at each oscillation. During the period of formation the shore-line need only have moved up and down a few meters. Afterward the whole system of strata was lifted high above its original level by locally acting "geotectonic" forces.

Therefore I assume that even the great overflows do not depend upon any very considerable displacement of coast-lines in a vertical direction. When there are large flat countries with basin-shaped depressions, a small elevation may suffice to produce great geographical changes.

Possibly also these overflows may be due to changes in the eccentricity of the orbit.

We will now test our hypothesis by a comparison between the astronomical periods and the geological series of deposits.

The curve of the eccentricity of the earth's orbit has been calculated from Leverrier's formulæ by J. Croll ("Climate and Time," 1875, p. 312) for a period of four millions of years; three millions of years backward and one million forward from the present time. The curve is also calculated according to the same formulæ by McFarland (*Amer. Journ. Sci.*, 1880, [3], vol. xx, pp. 105–111). His calculation extends from 3,250,000 years backward, to 1,250,000 years forward in time.* He has calculated with shorter intervals of time than Croll (Croll 50,000, McFarland 10,000 years), which however has had no particular influence in altering the form of the curves. McFarland has in the same place calculated the curve for the same period of time from new formulæ of Stockwell's. The two curves, taken in the gross, show a uniform course throughout their length, but as regards the first half Leverrier's curve is thrown somewhat backward. Stockwell's formulæ are considered to be more accurate than Leverrier's.

Both curves are given by McFarland. If we compare them together it appears—

(1) The curves coincide with only a small essential difference from the present day until one million years back.

(2) If we omit the portion between 7′ and 8′ of Leverrier's curve, Leverrier's and Stockwell's curves are in all essential points identical also as regards the older part, although the agreement is not so complete as for the last million of years. The reason of this is that the calculations are less certain with regard to the older periods; when the number of years enters as a factor in the formula, small errors in the values adopted for the planets' masses will be enlarged in proportion to the time, and the result becomes less certain.

(3) A very remarkable consequence proceeds from these calculations. *The curve repeats itself after the lapse of* 1,450,000 *years*, when it is calculated according to Stockwell's formulæ. In the period of 4,500,000 years for which McFarland has calculated it, it repeats itself in this way with remarkable regularity a little more than three times. In each of these cycles there are 16 arcs of the curve. Thus the arcs which in the accompanying plate are indicated by 1 to 16 correspond with 1′ to 16′ and 1″ to 16″. Mr. Geelmuyden, from calculations which he made at my request, has declared that the course of the curve will probably be sufficiently correct to be adopted with safety as the foundation for geological considerations, and that uncertainties in the curve caused by errors in the masses employed by Stockwell will probably not be of any importance.

(4) The mean value of the eccentricity is least at the limits of two cycles; it rises in the first and sinks in the last half of each cycle, and therefore attains its greatest value about the middle of each cycle.

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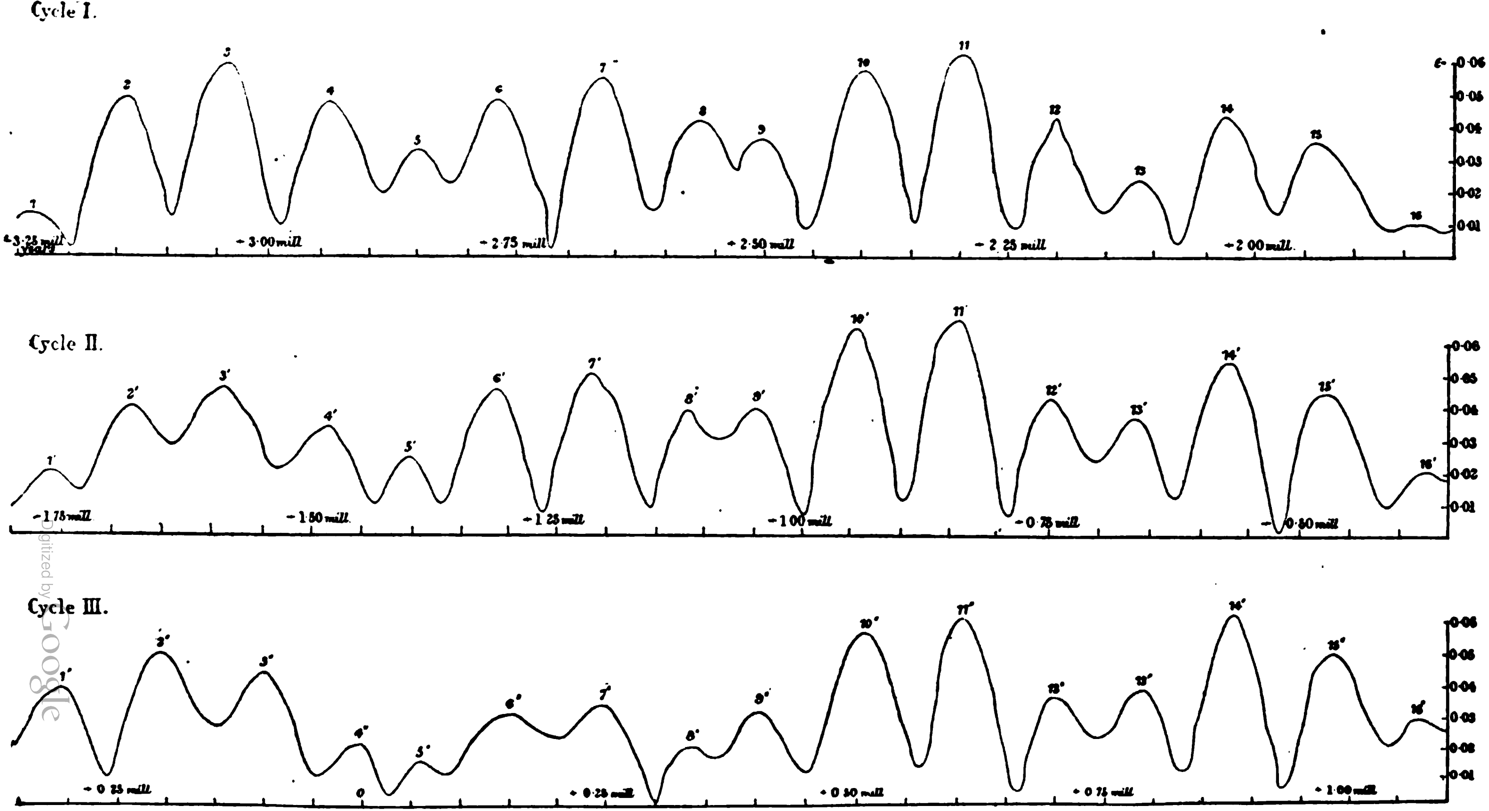
* [On the scale shown in the accompanying diagram, the interval of 2,000 years would occupy only $\frac{1}{100}$ of an inch on the base line. The epoch of the table (1850)—marked "0" in the third line of curves (cycle III, just under the arc 4″)—may therefore as well be assumed to be the present year.]

Eccentricity of the Earth's orbit, from 3,250,000 years past, to 1,100,000 years future. (Copied from R. W. McFarland.—*Am. Jour. Sci.*, 1880, XX, 107.)



Thus for the first and second of the calculated cycles and their subdivisions it is as follows:

Cycle I. ÷3,250,000—2,720,000 years, 0.0304.
÷2,720,000—2,150,000 years, 0.0332.
÷2,150,000—1,810,000 years, 0.0203.
Cycle II. ÷1,810,000—1,250,000 years, 0.0247.
÷1,250,000— 700,000 years, 0.0340.
÷ 700,000— 350,000 years, 0.0280.
Cycle III. ÷ 350,000 to the present time, 0.0291.

Now as, according to our hypothesis, the sea-level under high latitudes will rise and fall with the eccentricity, then it must not only rise and fall once for each arc of the curve, but the "mean sea-level" for longer periods must also rise and fall with the mean value of the eccentricity, and such cycles as cycles I and II must then correspond to two cycles in the geological sequence of deposits. The limits between the cycles of the curve must correspond to the periods of denudation which divide the geological cycles, and the middle must correspond to the periods of overflow.

The correctness of the two hypotheses put forward in my memoir on the Alternation of Strata may (as already indicated) be tested in one way by the comparison of geological profiles with the curves of the eccentricity of the earth's orbit. A first attempt was made at the time with the Upper Eocene and Oligocene beds of the Paris-basin.

Many difficulties, however, stood in the way of this work. First and foremost the calculation of the curve is less certain for distant periods. This difficulty is to a certain extent got rid of by the circumstance that, as the curves repeat themselves, it may be less essential.

Another difficulty is in the finding of long and accurately described profiles without gaps in the series of deposits. Survey-profiles are not sufficient. Geologists often only state that there are few, some, or many alterations of strata, without giving definite numbers.

A third difficulty is the distinguishing between the alternations of deposits which are due to precessions and those which have their cause in other more transitory and local conditions. In the case of shore-formations this difficulty is especially perceptible; but it has proved to be less than I supposed at first.

A fourth difficulty consists in the determination of the number of oscillations of coast-lines. The higher a place was situated, the more rarely was it overflowed; the lower it lay, the more rarely was it uplifted above the sea. And movements of the solid body of the earth, as might be supposed, have not been so uniform everywhere as those of the sea.

A fifth difficulty lies in the finding of perfectly typical profiles of the stages produced by the oscillations. When the sea rose and sank slowly the number of marine alternations of strata will be less, and of land and fresh-water formations greater, the higher the place lay, and the shorter the time which it remained submerged in the sea, during each oscillation. But this difficulty is of importance only when the continuous profiles are so short that they do not embrace several oscillations.

In the absence of longer, connected, and accurately traced profiles I have first endeavored to determine the number of oscillations of coast-lines as regards the Tertiary and Quaternary periods. Each of these oscillations, of which there have been about thirty-six from the commencement of the Tertiary period until now, has, in temporarily submerged localities, produced an alternation of marine beds with fresh water or terrestrial formations. To each more considerable oscillation corresponds a geological "stage." In these "stages" there is a certain number of alternations. By studying the literature of the Tertiary basins of Europe I have in this way formed a combined profile, which, as regards the alternations of strata, is not yet completed throughout, but which goes from the commencement of the Tertiary to the present time, and which I shall now proceed to describe.

The mode in which profiles can be compared with the curve to tes the correctness of the hypotheses is as follows: Each arc in the curve will correspond to an oscillation of the sea. It is supposed that under high latitudes the coast-lines move up and down with the curve. Such an oscillation I call a "geological stage." Each arc will therefore have its corresponding oscillation or "stage," and in each "stage" there will be as many alternations of strata as there are precessional periods in the corresponding arc. When the eccentricity only sinks inconsiderably between two or more arcs, the arcs run into one another, and form as it were, ranges with two or three small summits. We have then "stages" with more oscillations and more alternations of strata than the ordinary ones. We shall see examples of this in what follows. We can draw a line which indicates the boundary between marine and fresh-water formations. This line may be nearly or quite horizontal. Whether it is to be drawn high or low depends upon how much above the sea the place was situated where the deposits were formed at the time when the deposition took place. The higher it lay, the higher must the line be drawn. The place may have been so elevated that it never was submerged. Then the lines are situated higher than the curve, and all the deposits are fresh-water or terrestrial formations. But it may have lain so low that it never rose above the sea, and all the deposits are marine formations. But the line may cut the curve. Then marine formations alternate with land and fresh-water formations. The former correspond to those arcs of the curve which project above the line; the latter to those which lie below it. And when there are no gaps in the series of deposits, there will be as many alternations of deposits in the marine, fresh-water, and terrestrial formations as there are precessional periods in the corresponding arcs of the curve.

As a starting-point I will take the profile of the Paris basin*, which I will endeavor to join on to recent times. Afterwards I will refer to the lower and middle parts of the Eocene period.

The section of the Paris basin about Méry-sur-Oise (*Bull. Soc. Géol. Fr.* 1878, pp. 243 *et seq.*) shows the following oscillations and alternations of strata, and may, as regards the continuous portion, fit into Stockwell's curve, as appears from the arc numbers cited for each oscillation:

Sabies de Cuise, marine.
Calcaire grossier inférieur et moyen, marine, with seven alternations.
Calcaire grossier; Caillasses à Cerithium, two marine alternations, and between them a deposit with fresh-water shells.
Calcaire grossier; Caillasses à Lucina, marine, with five alternations.
Calcaire grossier; Caillasses à Cardium, marine, with eleven alternations. Gap in the series.
Sables de Beauchamp, fresh water and marine, about four alternations. Arcs 14–15.
Calcaire de St. Ouen, fresh water, four above which a marine deposit (summit of arc 16), then six fresh-water alternations. Arcs 15 to 2′.
Gypsum, marine, about eleven alternations. Arcs 2′ to 4′.
Gypse palustre, fresh water, about six alternations. Arc 5′.
Marine verte, brackish, two alternations. Arc 6′.
Calcaire de Brie, fresh water, one alternation. Between arcs 6′ and 7′.
Marne et Mollasse, sables de Fontenaye, marine, three alternations. Arc 7′.
Meulières de Montmorency, Calcaire de Beauce (p. p.), freshwater. Between arcs 7′ and 8′.

There is only one discrepancy: Arc 16, the summit of which should correspond to the marine deposit in the middle of the Calcaire de St. Ouen, does not go so high that we should expect an inundation of the sea. But the oscillation is at any rate also indicated in Stockwell's curve, and the marine formation consists of a single bed, and is so faintly marked that it has only recently heen recognized.

Another profile from another place in the Paris basin (la Frette, *Bull. Soc. Goél. Fr.* 1876, pp. 471 *et seq.*) has the same number of alternations as the above, and extends from 13′ to 2′. The marine bed at 16 is wanting in this profile, otherwise the same oscillations are indicated.

The profile at Méry-sur-Oise has in all seventy-one alternations of strata, of which twenty-five are in the Calcaire grossier. A great part of the calculated curve is therefore filled up by the occurrence of thirty-seven alternations, without a gap in the series of deposits. With arc 7′ the marine formations of the basin terminate. In Miocene times came the volcanic outbursts in Auvergne.

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* This section is given in my memoir on Alternations of Strata.

These oscillations of the coast-lines were not confined to the Paris-basin. The sequence of deposits in the basin of the Gironde, which seems to have been connected with the Paris-basin only through the Atlantic Ocean, is as follows (according to Vasseur, *Ann. Sci. Géol.* vol. XIII, pp. 398 *et seq.*):

The Tertiary formations commence with the Middle Eocene: Nummulitic sand and coarse limestone, marine. After this, elevation and erosion. Then again followed a depression: clay with *Ostrea cucullaris* (arcs 14 and 15), and another elevation: lacustrine limestone of Plassac, and simultaneously with this brackish-water limestone of Bégadan (16′ to 1′). Then a new depression: marine limestone of St. Estèphe and limestones and marls with *Anomia girondica* (2′ to 4′). Elevation and erosion: Mollasse (fresh water) of Fronsadais (6′?). Depression:—Calcaire à Astéries de Bourg (marine, 7′). Elevation: lacustrine limestone of l'Agenais, level 1 (between 7′ and 8′). This is contemporaneous with the Calcaire de Beauce of the Paris-basin. In the basin of the Gironde fresh oscillations took place, namely, the following, which are Miocene: Faluns de Bazas, marine, 8′; elevation: lacustrine limestone of l'Agenais, level 2 (between 8′ and 9′); depression: Faluns de Léognan et Merignac, marine (9′). The so-called Mollasse of Anjou, which is wanting in the basin of the Gironde, is, according to Tournouer (*Ann. Sci. Géol. l. c.* p. 62) younger than 9′, but older than the Faluns de Salles of the Gironde; both are marine, and probably indicate two oscillations, 10′ 11′. Then followed another Miocene oscillation, which has left its traces in the basin of the Loire, in the marine Faluns of la Dixmerie (arc 12′).

Thus the Miocene period in France had five oscillations. I have not however been able to obtain detailed profiles of all these series of deposits.

We now pass to England. In the Memoirs of the Geological Survey of Great Britain, 1856, we have accurate profiles of the Tertiary formations of the Isle of Wight (by Forbes and Bristow). The series of beds, from below upwards, has the following oscillations and alternations*:—

Plastic Clay (brackish?), four alternations.
London Clay, marine, at least eleven alternations.
Lower Bagshot, (in part?) fresh water, seven alternations.
Middle Bagshot (Bracklesham and Barton), the first fresh water, the second marine, and with five alternations.
Upper Bagshot, without alternations.

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* See Postscript to this article, *post*, p. 370.

This part of the series is in part older than the Calcaire grossier, and there are at least one,—probably two gaps in it. The following series, on the contrary, is continuous:

- Lower Headon, fresh water and brackish, seven to eight alternations (arc 13 and the first part of 14).
- Middle Headon, marine, one alternation, at 14.
- Upper Headon, fresh water and brackish, five alternations, between arcs 14 and 15.
- Osborne, fresh water, three alternations, between arcs 15 and 1′.
- Bembridge limestone, fresh water, three alternations, between arcs 1′ and 2′.
- Bembridge oyster-bed, marine, at least one alternation, at 2′
- Bembridge marl, fresh water, six alternations, arcs 2′ and 3′.
- Hempstead marl, fresh water and brackish, two alternations, 4′ or 5′?
- Hempstead Corbula-beds, marine, imperfect above by denudation, one alternation.

The profiles of the different stages are taken at different parts of the island which have lain at different levels. Bearing this in mind, the series may be fitted into the curve, and at any rate correspond with them pretty closely.

The number of alternations in this last continuous part of this series of deposits is about the same as in the contemporaneous deposits of the Paris-basin, although the beds are more than three times as thick (48 meters in the Paris-basin, 156 meters in the Isle of Wight).

With the marine deposits of Hempstead the marine formations of England are interrupted, and it is only in the Pliocene that we have indications of a new marine submergence. The basalts and volcanic eruptions of Ireland and the Hebrides are probably, at any rate in part, Miocene. Basaltic dikes extend in places across the whole of England; but the chief outbreaks were on the western side, and hence they can be traced through the Faröes to Iceland.

We will now see whether we can fill up the curve from 7′, where the continuous profile from the Paris-basin closes, up to recent times. The uppermost bed of the Paris-basin lies upon the boundary between Oligocene and Miocene. As we have already seen, the Miocene period in France had five oscillations. In Transylvania (according to Koch, in the *Földtani Közlöny*) there are five Miocene stages, namely: Koroder beds, Kettösmezö beds, Hidalmas beds, Mezöseger beds, and Feleker beds. All these stages are marine. Even if they are not throughout separated by fresh-water formations, as in the case of several, at any rate, of the French Faluns, they may nevertheless be regarded as corresponding to five oscillations. In the deeper seas the bottom will not always be upheaved above the sea under low eccentricities; but the oscillations will nevertheless operate in changing the fauna, and also frequently the constitution of the deposits.

The Miocene deposits of the Vienna basin are divided into three principal stages,—the first and second Mediterranean, and the Sarmatian. But if we study the detailed profiles more closely, there appear to have been here also five Miocene oscillations. Thus (according to Suess, *Sitzungsb. Wiener Akad.*, 1866) the first Mediterranean stage shows the following sequence of strata from below upwards:

Beds at Molt, with oyster-shells (broken), at the top with lignite, four alternations, arc 8′.—Supposed by Suess to be on the same horizon with the Faluns of Bazas.

Beds near Loibersdorf, Gauderndorf, and Eggenburg, marine, probably with eight alternations, at any rate in part younger than the beds at Molt (arcs 8′ ? and 9′).

"Schlier" with gypsum, at the top with land-plants.—Suess calls it "ein ersterbendes Meer," and seems inclined to regard it as a peculiar stage. Alternations, but scarcely more than two. The last part of arc 9′.

Beds at Grund, marine, with few (three to four) alternations, to judge from Suess' profiles.—The fauna forms a transition from the first to the second Mediterranean stage, and this deposit at Grund is with reason regarded by several Viennese geologists as representing a distinct stage. Arc 10′.

This was followed by the greatest submergence, the second Mediterranean stage (arc 11′), contemporaneous with the French Faluns de Salles: The sea rose quite up into the inner Alpine Vienna basin. I have been unable to make out the number of alternations in this stage. I have only seen sections of the littoral formations described.

Finally, the last Miocene oscillation, the Sarmatian stage, arc 12′. In some localities (*e. g.*, near Constantinople) this stage commences with fresh water covered by marine formations (see Suess, *Antlitz der Erde*, I. p. 419). According to a profile from Hungary (by Peters in *Sitzungsb. Wiener Akad.*, 1861) the stage has four alternations.

This stage is followed by the Pliocene Congeria-beds, which in the Vienna basin are represented only by brackish-water formations, according to Fuchs (*Jahrb. k. k. Geol. Reichs.*, 1875) with four alternations; arc 13′. And with these the marine formations of the Vienna basin, Hungary, and Transylvania come to a close. Volcanic outbursts commenced in these countries even in the Oligocene period; they became very frequent in the Miocene, and during this period the Alps rose to great altitudes.

In the basin of Mayence the marine Oligocene formations (Weinheimer marine sand and Septaria-clay) are followed first by a fresh-water formation; then the Miocene period commenced with a depression. But during volcanic eruptions the basin was upheaved and became more and more fresh-water. A continuous formation of beds took place. Over the Cerithium-limestone, the Corbicula-limestone and Littorinellaclay were deposited, in all with twenty or more alternations (according to Lepsius, *Das Mainzerbecken*). All these deposits are Miocene.

We now pass further forward in time. The Pliocene has four oscillations, 13′, 14′, 15′, and 16′. We have already mentioned the Congeria-beds of the Vienna basin. In England there are three oscillations: Coralline Crag (14′), Red Crag (15′), and Cromer Clay or Westleton Shingle (16′). Profiles of these are to be found in *Quart. Journ. Geol. Soc. Lond.*, 1871 (by Prestwich). The climate of Europe began to become colder in the Pliocene. Even the oldest deposit in the Pliocene of England contains stones which may have been grooved by ice, and at the close of the Pliocene there were already great glaciers; the Pliocene was followed by the Glacial epoch. We have seen how, during strong and extensive volcanic action, previously marine basins were during Oligocene, and especially Miocene, times uplifted above the sea not to be depressed afterwards (Paris, Vienna, Hungary, the Mayence basin, and we may add Switzerland), and we have seen that the Alps were upheaved in Miocene times. The Faröes and Iceland were built up, at any rate in great part, at the same time by basalts and lavas; perhaps, moreover, the submarine bank which connects Europe with Greenland was uplifted during the last portion of the Miocene period. In the Mediterranean, according to Neumayr (see Suess, *Antlitz der Erde*, I. p., 425) the coast-lines at the close of the Pliocene lay even lower than at the present day. No doubt all these elevations have had much influence upon climate. Changes in the length of the day are dependent upon variations of the eccentricity. Geographical changes follow upon the increase of the day, and climate changes with the distribution of land and sea.

The Coralline Crag in England (according to Prestwich) has a thickness of only 25 meters, and can not have many alternations. After this stage was formed the land rose, but was again partially depressed under the sea. During this depression was formed the Red Crag, with the Chillesford Clay. In the Coralline Crag two shore-lines were hollowed out one over the other and the new stage lies now on the old shore platforms. The Red Crag is thinner than the Coralline Crag and can not include many alternations.

In Belgium, also, we have two Pliocene stages, which correspond to the two English Crag-stages: the Scaldisien, étages supérieur et inférieur. To these two oscillations of the North Sea correspond two contemporaneous ones of the Mediterranean. Suess calls them the third and fourth Mediterranean stages. And even in the earliest part of the Pliocene the Mediterranean fauna indicates a somewhat colder climate (Suess, *l. c.* I. p. 431).

Italy possesses thick Pliocene formations. Seguenza describes deposits 500 to 600 meters in thickness from this period. I have been unable to obtain profiles of these deposits. They are in part conglomerates and shore formations, like the great Miocene Mollasse of Switzerland, and near the shore thick deposits can be formed in a short time.

The profiles of Roussillon (by Depéret, in *Ann. Sci. Géol.* vol. XVII. 1885) show four alternations in the stage contemporaneous with the Coralline Crag (arc 14′). The overlying stage in Roussillon is a fresh-water formation. The land had risen. The fresh-water stage has several alternations, probably six to eight, so far as I can see from the profiles given, which, however, are not quite accurately described (arcs 15′, 16′).

We now turn again to England. The fossils of the Red Crag show a colder climate than that of the Coralline Crag, and the Chillesford beds, which belong to the last portion of the Red Crag, have distinctly arctic shells. The Glacial epoch was advancing. After the Red Crag was formed, England again rose and became united by land with the continent. Extinct mammals wandered in its forests, which consisted of existing trees (spruce, pines, etc.) and show a temperate climate, milder than that of the Chillesford beds, and about as at present. "The forest bed of Cromer" was overlain by marine deposits—Westleton Shingle and Cromer Clay (arc 16′). In the latest terrestrial formation at Cromer Nathorst has found Arctic plants (*Salix polaris*, etc.), and the Cromer Clay indicates the vicinity of inland ice. With this the Pliocene closes.

As regards the Quaternary oscillations, we will take the English deposits as described by J. Geikie ("Great Ice Age," ed. 2, pp. 387 *et seq.*) as our guide.

The Quaternary period commences with the retrogression of the ice and with a considerable denudation. Then the sea again rose and covered a great part of the east of England. The inland ice again extended itself and formed a bottom-moraine, "the great chalky bowlder-clay" (arc 1″). After this glacial period an elevation of the land seems to have followed, and the ice retreated. But a new depression followed (Bridlington Crag) and a new glacial period (purple bowlder-clay, arc 2″). A fresh elevation seems to have followed, with a new interglacial period. Then came a new depression, which was very considerable, and which at Moel Tryfaen in Wales, at Macclesfield, and in Ireland has left marine shells at heights of 1,000 to 1,300 feet above the sea (nearly approaching that at which the old "seter" or beach-lines in Österdalen, Læsje, etc. occur). Like the preceding depression, this was also followed by a glacial period, the last (Hessle bowlder-clay, arc 3″). Finally the land rose and the ice melted. The Post-glacial period came with its four peat-beds (the last portion of 3″ and 4″). To arc 4″ corresponds a small oscillation of the sea immediately before the recent period. In Scania, Gäravallen, a raised beach-formation, rests upon peat; in Gotland, in the British Islands (Carse Clay, etc.), and even in North America, we may trace the same oscillation of the sea; it was no doubt too great to be capable of explanation by local conditions, compression of peat-beds by shifting sand-dunes, etc.

We have already seen that the land (according to Howorth and Suess) in many places under high latitudes rose considerably in the Post-glacial period, and that a corresponding depression took place in the warm coral-seas. The last oscillations therefore affected a great part of the earth. From this we may conclude that this was the case also with the oscillations of former times, and that they have their cause in general cosmical conditions. The small oscillation (arc 4″) forms an interruption in this great upheaval under high latitudes. A similar interruption of the depression, if our theory be correct, must be exhibited under the tropics; and in reality in the equatorial parts both of America and the Old World, there are numerous evidences of such a small post-glacial oscillation in coral-reefs, which have been upraised several meters and are now lying dry (see Suess, *Antlitz der Erde*, II. pp. 630 *et seq*). These coral-reefs may date from the same time when the northern peat-beds were submerged. The sunken peat-beds with the marine deposits formed during the depression have been again uplifted, and the raised coral-banks have probably again begun to sink (at Bombay there is a sunken forest), but the depression has not yet brought them down beneath the sea.

We may make one or two further observations upon the Glacial period and its formations. Contemporaneous with "the forest-bed of Cromer" (according to Heer) are the lignites of Dürnten in Switzerland. The fossils show this. They have nearly the same plant-remains, and the same extinct animals. The lignites rest upon and are covered by bottom-moraines, and are therefore "inter glacial." They have seven alternations of peat and forest-beds, and may be fitted into the curve between the arcs 15′ and 1″. From this the Alps must have had large glaciers even during the time of the Red Crag. And there is no improbability in this if we remember that *Leda arctica* and other Arctic animals were already living on the English coast at this period, and that the Chillesford beds indicate a much colder climate than the subsequent forest-bed of Cromer.

It is instructive to see how each rising of the sea in England during the Quaternary period had as its consequence the increase of the inland ice. This seems to agree with Croll's theory, that glacial periods are a consequence of great eccentricities. But the scanty traces of glacial periods in the older formations, and above all the distribution of glaciers at the present day, show that geographical conditions have the greatest influence. It is only when these are favorable that a high eccentricity can cause the glaciers to increase; if they are very favorable, there may be a glacial period even during a small eccentricity, as in Greenland at the present day. When the eccentricity increases, the precipitation during rainy periods also increases. If the sea is cold, the precipitation will fall as snow, and in this way the glaciers will grow as the eccentricity increases.

North Germany (according to Jentzsch) has also had three glacial periods with corresponding bottom-moraines (and oscillations?); and in the Alps there have been (according to Penck, *Vergletsch. d. deutsch. Alpen*) at least three glacial periods.

We have thus filled up the curve to the present time, and connected the profile of the Paris basin therewith. We will now trace the oscillations back to the close of the Cretaceous period in order, if possible, to see how many oscillations are included in the Geological period known as the Tertiary.

The Cretaceous period is separated from the Tertiary by a period of denudation, during which the land was high relatively to the sea. The oldest marine formation of the Tertiary period in Europe is considered to be the limestone of Mons, in Belgium. This indicates the first oscillation; but this submergence appears not to have left traces in the other Tertiary basins. The first marine inundation of the Paris-basin during Tertiary times formed the conglomerate of Rilly and Nemours. It was followed by an elevation of the land, and the marine conglomerate was covered by the fresh-water limestone of Rilly. This oscillation in the Paris-basin is perhaps represented in Belgium by the so-called "système Heersien," which is at the bottom a purely marine formation, but has remains of land-plants at the top. Then came a new oscillation, and now England also was partially submerged. Here was deposited the marine Thanet Sand, and upon this the Woolwich and Reading Series (=Plastic Clay), the latter partly a brackish and fresh-water formation, and which shows that the shore-line had again retreated. In Belgium the "système Landenien" was formed during this oscillation,—below purely marine, above brackish. In the Paris basin there was formed the marine sand of Bracheux, which was followed by a fresh-water formation with lignite (the Lignites de Soissonnais). Then followed a new depression, and again an upheaval. This has left no traces in the Paris-basin; but in England the London Clay was formed, and in Belgium the "système Yprésien." The London Clay commences with a shore formation of shingle or gravel (Oldhaven Beds), and the upper part of the stage shows that the sea again became shallower, in consequence of a new elevation of the land.* The "système Yprésien" in Belgium is divided into two sub-stages,—the older, a clay with Foraminifera,—the younger sandy, with numerous fossils, and therefore probably indicating a shallower sea. A new submergence formed, in Belgium, the marine "systéme Panisélien" (sand), and in the Paris-basin the marine sand of Cuise. With this the Lower Eocene closes. It has therefore in all probability, six oscillations.

The Middle Eocene is represented in France chiefly by the "Calcaire grossier." In this stage there are five to six sub-stages, and in several places breaks in the series of deposits. The Middle Eocene is, on the whole, marine, but with intercalated fresh-water beds, and it probably also represents six oscillations. In Transylvania it commences (according to Koch in *Földtani Közlöny*, 1883, pp. 118 *et seq.*) with alternations of clay and marl, upon which follow alternations of gypsum and marl ("lower gypsum horizon," first oscillation). Above it, marine deposits, the *Perforata-beds*—from below upwards—(α) an oyster-bed, (β) argillaceous marl (γ) calcareous marl ("lower striata horizon"), (δ) a shell-bed ("lower perforata-horizon"), (ε) clay ("upper striata horizon," second oscillation ?), (ζ) clay with a few hard marly beds and the same fossils as in β, (η) another oyster-bed, (θ) clay with oysters, (ι) calcareous marl ("upper perforata horizon," third oscillation ?); above this the *Ostrea-clay*, a thick clay with oysters and marly beds, and with a sandy calcareous bed in the middle (fourth oscillation). Over this again the *Lower Coarse limestone*, generally in two thick beds (fifth oscillation), covered by a thick bed of clay varied with layers of sand, probably a fresh-water formation, and covered by fresh-water limestone. Finally, the last (sixth) oscillation, the *Upper Gypsum horizon*, gypsum alternating with clay; and above it coarse limestone alternating with gypsum; in other places, clay with Foraminifera (marine),—the *Upper Coarse limestone.* I have cited all of these details in order to show that these beds, which are all contemporaneous with the "Calcaire grossier" of Paris, seem to indicate six oscillations.

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* This stage, as was shown above, contains at least 11 alternations, and therefore probably corresponds with at least two arcs of the curve.

Above the "Calcaire grossier" the Upper Eocene commences with the continuous series of the Paris basin, which has already been described.

The Lower and Middle Eocene therefore appear to include twelve oscillations, six of which pertain to each of the two divisions of the formation. By this the first cycle of the curve is filled up, so that the beginning of the cycle will about fall upon the boundary between Cretaceous and Eocene. In the Paris basin the Middle Eocene has twenty-five alternations of strata and perhaps one or two breaks. Six oscillations about correspond to twenty-five or thirty precessional periods.

At the commencement of the cycles the mean value of the eccentricity is low; it rises in the middle of the cycle and sinks again towards the conclusion. The position of the shore-lines must also depend upon the mean value of the eccentricity. But as it increases very slowly through very long periods, it will be very long before its action is to be seen on the solid earth. The middle of the cycles ought thus to correspond to the overflows of the sea, the beginning and close to the periods of denudation which separate the formations. Breaks in the series of beds may therefore be expected under high latitudes, especially at the limits between the cycles.

The boundary between Cretaceous and Eocene is indicated by what Suess (*Antlitz der Erde*, II, 7ter Abschn., p. 376) calls a negative phase; the sea had retreated in higher latitudes. During the Eocene it rose again, and the Eocene sea had a great extension; we find its formations even in the heart of Upper Asia. The limit between the Eocene and

Oligocene is again distinguished by a negative phase. In the latter part of the Oligocene period, and still more during the Miocene, the sea again rose; between the Miocene and Pliocene it retreated far, and at the beginning of the Quaternary epoch it rose again. Similar great oscillations are also to be traced in North America and in Patagonia. But marine Miocene deposits are wanting in the last-mentioned locality, where the Miocene fresh-water beds are associated with great quantities of volcanic products.

At the commencement of the Tertiary period, when the sea had retreated far under high latitudes, the climate of Europe was temperate rather than tropical (see Saporta, *Le Monde des Plantes avant l'apparition de l'homme*, 1879). According as the sea rose and the Eocene overflow advanced the climate became warmer, and at the close of the Eocene period the climate of Southern Europe was hot and dry. The abundant Tertiary flora of the Arctic lands is (according to Saporta and Gardner) rather Eocene than Miocene (as Heer supposed). At the boundary between Eocene and Oligocene the sea retreated, and the Arctic Tertiary flora began to migrate into Europe, supplanting the more southern plants. Then came the Miocene overflow, and with it a rich tropical or sub-tropical flora. But in proportion as the Miocene sea retreated, the European flora also, little by little, lost in richness and beauty and the tropical elements became more and more rare. During the Pliocene epoch the sea retreated still farther, and the climate became colder and colder until the Glacial period came in. But the last Quaternary overflow has again, after several oscillations, caused the ice to retreat, and our climate has again become temperate. There is thus clearly a relation of dependency between the climate and geographical conditions. Great seas under high latitudes produce warm climates, and *vice versa.*

Now we have seen that these great geographical changes were in all probability a consequence of the rising and sinking of the mean value of the eccentricity, and we must therefore believe that these great changes of the climate had a cosmical origin, and occurred at the same time over the whole earth. We still know too little of the geology of tropical countries; but there is ground for the belief that here also great changes have taken place in the distribution of land and sea, and that these changes must also have had an influence upon the climate of the warm countries.

It is further probable that the force of vulcanicity stands in relation to the changes in the eccentricity. Each of the great geological formations, from the Pre-Cambrian itself, has had its volcanoes (see A. Geikie, "Textbook," pp. 259–260); and we have already seen that the same author states that there have been periods in the earth's history when vulcanicity was much more powerful and widely distributed than at other times. We have seen how the upheaval of the land was accompanied by volcanic outbursts; and as regards the Tertiary period, at any rate, it appears that the great overflows of the sea were followed by periods during which the solid ground began to rise during violent and wide-spread volcanic eruptions.

For easy reference we will finally enumerate all the arcs in the curve, and name the geological stages supposed to correspond to them. To some extent we adopt the names given by Charles Mayer Eymar.*

LOWER TERTIARY; EOCENE—Cycle I.†

Lower Eocene. Arcs 1 to 6.

From 3,250,000 years to 2,720,000 years before the present time.

Arc 1. Etage Montien ?
2. Etage Heersien.
3. Etage Suessonien.
4. Etage Yprésien inférieur ? } Londinien.
5. Etage Yprésien supérieur ? } Londinien.
6. Etage Panisélien.

Middle Eocene.

From 2,720,000 to 2,150,000 years before the present time.

Arcs 7–12. Etage Parisien, with six oscillations.

Upper Eocene.

From 2,150,000 to 1,810,000 years before the present time.

Arcs 13–16. Etage Bartonien, with four oscillations.

UPPER TERTIARY.—Cycle II.‡

Oligocene.

From 1,810,000 to 1,160,000 years before the present time.

Arcs 1′–4′. Etage Ligurien, with four oscillations.
5′–7′. Etage Tongrien, with five oscillations.

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* See his valuable "Classification des Terrains Tertiares" (Zurich, 1884). He divides his stages into two sub-stages,—one with "mers amples," and one with "mers basses." Some of his stages however represent several oscillations. He thinks that the precession of the equinoxes is the cause of the changes in the level of the sea. The whole of the Tertiary and Quaternary periods must (according to him) have had a duration of only a little over 300,000 years. He founds his views upon Schmick's untenable hypothesis of the dependence of the sea-level upon the precessions.

† The upper line of curves in the diagram.

‡ The middle line of curves in the diagram.

Miocene.

From 1,160,000 to 700,000 years before the present time.

Arc 8′. Etage Aquitanien ?
9′. Etage Langhien.
10′. Etage Helvétien.
11′. Etage Tortonien.
11′. Etage Messinien.

Pliocene.

From 700,000 to 350,000 years before the present time.

Arc 13′. Etage Matérin.
14′. Etage Plaisancien
15′. Etage Astien.
16′. Etage Arnusien.

QUATERNARY.—CYCLE III.*

From 350,000 years ago, to the present time.

Arcs 1″–3″. Etage Saharien, with three oscillations.

The limits between the cycles of the curve are not drawn arbitrarily. The beginning and the close of the first two cycles are distinguished by their unusually low eccentricity. The last arc in one cycle and the first in the following one have together a duration of about 150,000 years; and in all this time the eccentricity was very low. In these two cycles, likewise, the highest mean eccentricity occurs in the middle of the cycle.

The Eocene period seems to have had sixteen oscillations, and should correspond to the first cycle; the Oligocene, Miocene, and Pliocene have likewise together sixteen oscillations, and correspond to the second cycle. The Lower Eocene corresponds to arcs 1 to 6, the Middle Eocene to 7 to 12, and the Upper Eocene to 13 to 16. In the same way the Oligocene corresponds to arcs 1′–7′, the Miocene to 8′–12′, and the Pliocene to 13′–16′. There is thus a certain analogy between the older and the younger Tertiary periods. We have here six divisions which nearly correspond to each other in the following manner:

Lower Eocene to the Oligocene: the former with six, the latter with seven oscillations.

Middle Eocene to the Miocene: the former with six, the latter with five oscillations.

Upper Eocene to the Pliocene: both with four oscillations.

The great overflows of the sea occur in the middle of the cycles, in the Middle Eocene, the Upper Oligocene, and the Miocene. In the middle of the cycles the mean value of the eccentricity was greatest. At the commencement and the last part of the cycles, when the mean value of the eccentricity was small, the sea retreated far, as between the Cretaceous and the Eocene, and in the Upper Eocene and Pliocene. The notion therefore presents itself with great probability that there is a connection between the cycles in the curve representing the eccentricity of the earth's orbit and what is called a geological epoch, or what has also been called a "cycle" or "circle of deposition." The two Tertiary cycles are as it were great stages, each composed of sixteen smaller ones. Just as each of these sixteen represents a small oscillation of the sea, so does each cycle represent a great oscillation; but this great oscillation has been accomplished by means of the sixteen small ones. In the same way the mean value of the eccentricity rises and falls in each cycle with sixteen oscillations; it is low at the commencement of the cycle, attains its greatest value in the middle of the cycle, and falls again towards the close. These agreements between the cycles of the curve and the formations, between the arcs of the curve and the stages, and between the number of the arcs' precessions and the alternations of the strata in the stages wherever these could be checked, appear to me to be so striking as to exclude the notion of an accidental coincidence, and distinctly point to a causal relation.

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* The lower line of curves in the diagram.

If we would test the correctness of our hypotheses by means of the older formations, the following points must be borne in mind: After investigating the laws of the variations of eccentricity, Geelmuyden told me that it is probable that a cycle of about 1,500,000 years must appear in the curve, but that without more extended investigation we can not conclude that this will continue unchanged for unlimited periods. Even in the calculated curve, the Cycle III is distinguished from the other two by a much lower eccentricity in the arcs 4″–9″.

If the polar compression in old times was greater, then the precessional period was also shorter. According to Geelmuyden it would be very nearly proportional to the square of the time of rotation. For example, to a rotation time of sixteen hours corresponds a (synodic) precessional period of 10,000 years, consequently only half the present period. The shorter the period the less marked (other things being equal) must the climatic period be, and the more indistinct the alternation of the strata.

Further, it must be remembered that in Palæozoic and Mesozoic times the moon was probably much nearer. In that case the lunar tide was much stronger, and stronger in proportion to the solar tide than at present. The day was shorter, and the stronger tidal wave acted more frequently. The shores were more rapidly destroyed. Deposition, no doubt, took place more rapidly. The sidereal day increased more quickly in length than at present. All these circumstances must have had an influence upon the form of the earth, upon the distribution of land and sea, upon the displacement of shore-lines, upon the changes of climate, upon the ocean-currents, upon the distribution of chemical and mechanical sediments and the alternations of strata, so that without taking these and perhaps other circumstances into consideration, we can not prove the applicability of the hypotheses to the Palæozoic and Mesozoic series.

In conclusion, I will briefly notice the chief points in my hypothesis.

The precession of the equinoxes and the periodical change in the eccentricity of the earth's orbit, are reflected in the series of strata and furnish the key to the calculation of the duration of geological epochs.

Precession causes winter and summer to be alternately longer and shorter. In the semi-period when the winter is longer than the summer, the difference between the inland and coast climate becomes more marked. The atmospheric current becomes stronger. As a consequence of this, the currents of the ocean increase in strength, and this again re-acts on the climate. The periodical change of the climate caused by precession is not very considerable, but still great enough to leave its mark in the alternations of strata, and in the formation of shore-lines, terraces, series of moraines, etc. One alternation of strata corresponds to each precessional period.

The eccentricity of the earth's orbit is periodically variable. Its mean value rises and falls in periods of about 1,500,000 years, with sixteen oscillations. Such a rise and fall I call a cycle, and each cycle, in the calculated curve, is composed of sixteen arcs.

The tidal wave, which is the most important agent in altering the sidereal day, and which makes it longer, rises and falls to a certain extent with the eccentricity. It so predominates over the other forces which alter the length of the sidereal day, that the day steadily lengthens on the average more rapidly in the middle of the cycles when the mean value of the eccentricity is greatest, and more slowly at the boundaries between them, when it is least, and, as regards the individual arcs, with increasing rapidity during rising, and decreasing rapidity during falling eccentricity.

The interior of the earth is plastic in consequence of the great pressure. The surface or "crust," opposes the greatest resistance to change of form. But as the sidereal day lengthens, and the equatorial parts of the earth increase in weight, a constantly increasing strain acts outward toward higher latitudes, and this strain increases until the resistance is overcome. It must also be remembered that forces which are too small to effect any sudden alteration in a solid body, may nevertheless produce a change of form when they act for a long time.

Hence the lengthening of the sidereal day does not act only upon the sea, but also upon the form of the solid earth. The earth constantly approaches more and more to the spherical form; but the solid earth, in its movements, lags behind the sea, which accommodates itself at once to the altered time of rotation.

As the motive power of these movements of the sea and the solid earth is periodically variable in accordance with the eccentricity of the orbit, these movements also take place periodically more rapidly and more slowly. And as the sea always adjusts itself to the forces before the solid earth, it is probable that the shore-lines oscillate up and down once for each rising and sinking of the eccentricity of the orbit. This applies both to the individual arcs of the curve and to the cycles. In such a cycle "the mean level of the sea" rises and falls once during sixteen oscillations.

According to Darwin the sidereal day has become several hours longer. It is therefore probable that so great a strain must have accumulated in the mass of the earth, that a slight increase of the strain would suffice to effect changes of form at the weakest points. It is also probable that these partial changes in the solid body of the earth must occur especially during great eccentricities, or some time after them, when the motive power increases most rapidly.

The change in the tidal wave with the eccentricity is supposed to be sufficiently great to explain the displacement of shore-lines. A vertical displacement of the shore-line by a few meters is sufficient to produce, in the deeper basins, an alternation of many meters of thick marine and fresh-water deposits. And as regards the changes of the solid mass of the earth, we must remember that the series of strata is not complete at any single place. In other words, the oscillations were not general to such an extent as to render them contemporaneous everywhere. It is only by partial changes of form, sometimes here, sometimes there, at those points which were weakest at each period, that the solid earth has approached the spherical form. To each arc of the curve therefore there corresponds only a partial—not a general—alteration of the form of the solid earth. And the oscillation of the shore-lines corresponding to the arcs therefore can not be demonstrated everywhere, but only in the basins where the forces at the time exerted their action. Hence we can only obtain a complete profile by combining the beds of all the Tertiary basins. Nor were the changes of the solid earth everywhere equally great, but they were greatest at the most yielding parts of the surface, so that very considerable local upheavals may be consequent upon small changes in the length of the sidereal day. This applies to the individual oscillations; but even the great overflows of the sea (of which one falls in each cycle) need not be due to any very great rise in the level of the sea, for great plains may be flooded and drained by a comparatively small vertical displacement of the shore-line. But these great changes in the distribution of land and sea were undoubtedly great enough to cause considerable alterations of climate. Great seas in high latitudes render their climate mild, and *vice versa*.

If now, keeping these principles in view, we compare the curve of the eccentricity with the geological series of strata, we find an agreement which indicates that the hypotheses are correct. The two cycles of the calculated curve correspond to two geological cycles. Each of these cycles has sixteen arcs, which correspond to sixteen smaller oscillations of the shore-lines, or sixteen geological stages. In each of these stages there are as many alternations of strata as there are precessions in the corresponding arc. And the "mean sea-level" rises with the mean eccentricity in the middle of the cycles, and falls at the boundary between them, and hand in hand with the mean sea-level the temperature in the higher latitudes also rises and falls.

The theory here discussed agrees with Lyell's great principle. Slow changes in the length of winter and summer and in the force of the tidal wave produce periodical changes of climate and displacements of shore-lines. The changes take place so slowly that the effects begin to appear distinctly only after the lapse of many thousands of years. There are two astronomical periods which are the cause of the great and fundamental changes of which geology bears testimony to us from long past days, and which will still continue, for millions of years, to effect similar changes in the geography of our globe, in its climate, and its animal and vegetable life.

POSTSCRIPT.

With reference to the profile of the Isle of Wight above cited (*ante*, p. 356), I must make a few remarks. Although with some doubt, I have referred the Headon beds to the Upper Eocene. But the difference between the faunas of the Grès de Beauchamp and the middle Headon is far too great for these beds to be synchronous.

The cause of the error is that I reckoned too many alternations of climate in the Isle of Wight beds. In these fluvio-marine deposits there is by no means the same regularity as in beds which are formed in basins with less sedimentation. The river eroded its borders and shifted its bed, banks were formed and carried away, according as the direction of the stream varied and the channel changed. Hence lenticular intercalations were often formed in the beds, and as precipitous cliffs of the Isle of Wight break down, the minor details of the profiles change in appearance. But with all this irregularity there are certain beds which appear far more constantly, and which we can recognize in the different profiles even although their condition is somewhat altered. By the aid of these constant beds we find order in the variations, and it appears that the great features of the profiles are maintained unaltered; and it is these great features that we must follow when we wish to determine the number of climatic alternations. In the Paris Basin, where sedimentation was much less, chemically deposited beds play a much more prominent part. In the Isle of Wight the stages are of much greater thickness. The Oligocene deposits of the Isle of Wight are 156 meters in thickness, and more than three times as thick as the contemporaneous beds in the Paris Basin, which have a thickness of only 48 meters.

In the dry periods the deposition of clay and mud was much less, the water of the river was purer, and chemically formed beds had time to be deposited. Instead of clay and marl,—limestone, Septaria beds, ironstone, etc., were formed. These beds were undoubtedly formed much more slowly than the sand, clay, and marl deposits which alternate with them. They are analogous to the forest beds in peat-mosses. Forest beds often separate peat deposits with different species of plants. This shows that the forest beds indicate long dry periods, during which the formation of peat ceased and the flora became changed;* when the quantity of rain again increased and the formation of peat commenced anew, the forest trees which grew around the mosses were changed, and the forest beds thus make divisions between different sub-stages or zones in the peat.

Among the beds deposited in water (whether fresh or salt-water formations) it is chiefly the above mentioned chemically formed beds that are formed in dry periods. And just as in the peat mosses forest beds often separate peat deposits with a different flora, so limestone and Septaria beds also frequently intervene between clay, marl, and sand deposits with a more or less different fauna, so that these chemically produced deposits often form boundaries between geological stages and sub-stages. This is the case, for example, in the Fluvio-marine series of the Isle of Wight, the main features of which we shall now pass on to describe with the aid of Forbes's detailed and classical statements. We shall then see that we have fewer climatic changes than I previously supposed, and that the series of beds in the Isle of Wight coincides as admirably with the curve of eccentricities as the Parisian deposits, although somewhat later on in time than was hitherto supposed; thus the agreement with the palæontological results becomes complete.

We begin from below, with the Upper Eocene *Barton Clay*. Judging from the fossils this is synchronous with the Grès de Beauchamp in the Paris Basin. It has five Septaria beds, and corresponds to arc 14 of the curve, which has the same number of precessional periods. The Barton Clay is covered by the *Headon Sands* (previously referred to the Upper Bagshot), which have no alternations, and which were probably formed in a comparatively short time.

A great gap now follows in the series in the Isle of Wight. In the Paris Basin the fresh-water Calcaire de St. Ouen was formed at this time. This is only 6 to 7 meters thick, but it has ten alternations, which should represent 200,000 years according to my calculation. It might seem that this was a long time for the formation of a stage of so little thickness; but while this stage was deposited the marine fauna was changed to such an extent that a great geological boundary has been drawn through this point, the boundary between the Eocene and Oligocene. The first marine Oligocene bed in the Isle of Wight (the Marine Headon) has a fauna of which only 30 to 50 per cent. of the species

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* See "Theori om Indvandringen af Norges Flora under vekslende regnfulde og tørre Tider," in *Nyt Mag. for Naturv.*, 1876, XXI, (pp. 52, 53 of separate copies).

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occur in the Barton Beds. The 6 to 7 meters of the fresh-water limestone in the Paris Basin probably represents more than half the time which elapsed between the formation of the Marine Barton and Headon.

Then the sea rose again, and the Oligocene period commenced. The oldest Oligocene stage in the Isle of Wight is the Lower Headon; it is a fresh and brackish water formation, showing one oscillation of the shore-line. I have given it seven to eight alternations. The stage contains five limestones, separated by deposits of sand and clay, and besides these, two horizons with ferruginous concretions. Reckoning these, it has seven periods. Marine fossils (*Cytherea*, *Mytilus*) sometimes occur in the middle of the stage; fresh-water and brackish forms above and below. The Lower Headon thus represents one oscillation of the shore-line (or a little more) with seven climatic changes.

The next stage or oscillation is the *Middle* and *Upper Headon*. These have together six alternations of strata, four limestones, and two beds with iron concretions separated by clays and sands. The Middle Headon is brackish at the base, but soon becomes a purely marine formation, with an abundant fossil fauna. The Upper Headon contains fresh and brackish water animals.

Above the Headon come the *Osborne Beds*, a nearly pure fresh-water formation. It has eight to ten alternations: Two Septaria beds, two ironstone bands, and six horizons with concretions of argillaceous limestone, separated by clay and marl. Ten alternations represent two oscillations and two arcs of the curve.

Over the Osborne comes the *Bembridge Stage*. The Bembridge beds consist of:—first, a fresh-water limestone, which has three well-marked alternations of compact limestone with clay and marl; these three alternations recur in profiles from the most different localities; over this the marine Bembridge Oyster-bed, and immediately above this a Septaria bed, of which Forbes says that it is "very remarkable and constant." Above this come the Lower Bembridge Marls with brackish and fresh water animals, but without alternations; upon this a Septaria bed, "sometimes siliceous, sometimes calcareous," which forms the boundary between the two substages, the Lower and Upper Bembridge Marls. In these Upper Marls, which likewise contain brackish and fresh-water shells and even lignites, I have assumed four climatic alternations: There are two pyritous bands and a marly bed, and at the top, at the limit of the overlying Hamstead* stage, a bed with ferruginous concretions capped with marl. But the two pyritous bands and the first of the above-mentioned marls constitute no palæontological boundary, and are far from being so prominent as the Septaria bed. I therefore regard it as the most probable assumption that the whole of the Bembridge Marls indicate only three alternations of climate, and thus for the whole stage we have six climatic periods.

Finally, we come to the *Hamstead* Beds*. These at the lowest part consist of brackish and fresh water marls, in which, besides a pyritiferous horizon of little importance, and which forms no palæontological horizon, we find indications of two dry periods. One of these, the so-called "White Band," a more or less hardened ferruginous bed rich in fossils, forms the boundary between the two substages, the lower and Middle Hamstead Marls; and higher up there is a bed of ironstone concretions, which nearly coincides with the limit between the substages of the Middle and Upper Hamstead Marls. The uppermost part of the Hamstead stage is formed by the marine Corbula bed, in which there is a bed with Septaria. The stage therefore represents one oscillation with three climatic alternations; but it is not completely preserved, the top having been removed by denudation.

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* The name is also often written "Hempstead," but this is incorrect.

If we now sum up the above statements, we obtain the following numbers of oscillations of shore-lines and climatic alternations:

Barton, one oscillation, with five climatic alternations.
Headon Hill Sand, without alternations.
Break in the series.
Lower Headon, one oscillation (or a little more), seven alternations.
Middle and Upper Headon, one oscillation, with six alternations.
Osborne, with eight to ten alternations, corresponding to two oscillations.
Bembridge, one oscillation, with six alternations.
Hamstead, one (incomplete) oscillation, with three alternations.

Besides the Eocene Barton there are three well-marked marine Oligocene horizons in this series: The Middle Headon, Bembridge Oyster-bed, and Hamstead Corbula beds. The Middle Headon is regarded by palæontologists as synchronous with the marine gypsum in the Parish basin. I have fitted the Paris beds, so that the marine gypsum coincides with the arc 3′, and the Fontainebleau Sands with the arc 7′. If we now arrange the equivalent beds in the Isle of Wight in the same arcs, we see that the Isle of Wight profile fits perfectly into the curve of eccentricity, as follows:

Lower Headon to the arc 2′ and perhaps the last part of 1′, with seven alterations and seven precessions.
Middle and Upper Headon, with five alterations, to the arc 3′, with five precessions.
Osborne, with ten alterations, to the arcs 4′ and 5′, with ten precessions.
Bembridge, with six oscillations, to the arc 6′, with five or six precessions.
Hamstead, with three alterations, to the first part of arc 7′, with three precessions.

It thus appears that the three marine horizons coincide with the three highest eccentricities, the summits of the arcs 3′, 6′, and 7′, while the lower arcs and parts of arcs correspond to brackish and fresh-water beds The most unmixed fresh-water formation, the Osborne, coincides with the two lowest arcs, 4′ and 5′.

For the sake of comparison, we will again carefully go through the profile of the Paris Basin, and compare this with Stockwell's curve, commencing from the bottom. The beds are numbered in the same way as in the original description of Dollfus and Vasseur (*Bull. Soc. Géol. Fr.*, 1878, sér. 3, tom. VI, pp. 243, *et seq.*).

Sables de Beauchamp et Mortefontaine, etc., beds 89 to 111. Arc 14 and first half of 15. In this series we have, first five marine sandstones alternating with sand; then a limestone and a calcareous marl, with intercalated sand and marl. Thus in all six or seven alterations.

Calcaire de St. Ouen, beds 112 to 142. A fresh-water formation which is divided by a marine deposit (128) into two subdivisions. In the lower part (from the summit of arc 15 to the summit of arc 16) there are four horizons of hard limestone and siliceous limestone with intercalated marls. Then comes the marine bed (at the summit of 16). It must be remarked that the corresponding arc in Leverrier's curve reaches higher up. In the upper division of fresh-water limestones we have six alterations of hard limestone and siliceous limestone with marl and clay. This division therefore finishes a little to the left of the summit of arc 2′.

Sablas de Monceaux, beds 143 to 145. Marine sand with three Septaria layers. The rest of arc 2′.

Marnes à Pholadomya, beds 146 to 154. Marine, with two alterations of siliceous limestone and marl. The first part of arc 3′.

Gypsum No. 3, beds 155 to 158. Marine marl and gypsum, one alternation, and *Marne à Luciana*, bed 159. The rest of arc 3′.

(The beds 146 to 159 thus have together three alternations and correspond to the arc 3′.)

Gypsum No. 2, beds 160 to 196, arc 4′. Marine, at any rate for a great part. But it must be remarked that no fossils have been cited from the last part of this series. Gypsum alternating with marls about five times. The most important gypsiferous horizons are the beds 161, 171 to 176, 178 to 188, 191 and 194.

Gypsum No. 1, bed 197, 8 meters thick, with fresh-water animals. One alternation. Between arcs 4′ and 5′.

Marne bleue, beds 198 to 204, and *Marne blanche*, beds 205 to 209, fresh-water marls alternating with marly limestones and ferruginous marls about four or five times. Arc 5′ and the first third of arc 6′.

Marne verte, beds 210 to 217, a brackish-water formation with two alternations of clay with marl and siliceous limestone. The upper part of arc 6′.

Calcaire de Brie, beds 218 to 220, a fresh-water limestone. Perhaps we have here indications of several climatic alternations, for limestones occur alternating with marl three or four times, though certainly in very thin beds. Its place is in the hollow between arcs 6′ and 7′.

Marne et Molasse Marine, beds 221 to 231. Clay alternating with marly limestone and sandstone three or four times. The upper part of arc 7′.

Sables de Fontenaye, bed 232. Marine sand with a few layers of clay, but without marked alternations. The latter part of arc 7′.

Calcaire de Beauce (p. p.) Fresh water, between arcs 7′ and 8′

From this we get a complete agreement with the palæontological results, as shown by the following comparison of the equivalent formations in both basins:

Paris.	Isle of Wight.
Grès de Beauchamp, etc	Barton Clay. Headon Hill Sand.
Calcaire de St. Ouen	Wanting.
Sables de Monceaux	Lower Headon.
Marne à Pholadomya, Gypse No. 3, Marne à Lucina.	Middle and Upper Headon.
Gypse No. 2–1. Marne bleue	Osborne.
Marne blanche	Bembridge Limestone.
Marne verte	Bembridge Oyster-bed.
Calcaire de Brie	Bembridge and Hamstead Marls.
Marne et Mollasse marine	Hamstead Corbula-beds.

It will be seen that the number of alterations of strata is about the same in the synchronous formations in the Paris and Hampshire Basins. This shows that this alternation of strata was due to a general cause; and that this cause is the precession of the equinoxes, seems highly probable.

As moreover the curve of the eccentricity of the earth's orbit appears at the same time to be a curve of the variations of the sea-level, we may also conclude with probability that for one reason or another the sea rose and fell with the eccentricity.

TIME-KEEPING IN GREECE AND ROME.*

By F. A. SEELY, *of the U. S. Patent Office.*

In my room in the Patent Office there hangs a Connecticut clock of ordinary pattern and quite imperfectly regulated. Its variation of perhaps half a minute in a day, however, gives me no concern, since being connected by wire with the transmitting clock at the Naval Observatory, it is every day, at noon, set to accurate time. At the moment of 12 o'clock there comes a stroke on a little bell and simultaneously the three hands, hour, minute, and second—whether they may have gained or lost during the preceding twenty-four hours, fly to their vertical position. Immediately after I hear a chorus of factory whistles, sounded in obedience to the same signal, dismissing the workmen to their midday meal. At the same moment and controlled by the same impulse, the ball, visible on its lofty staff from all the ships in New York Harbor, drops, and the seamen compare their chronometers for their coming voyage. The same signal is sent to railway offices and governs the clocks on thousands of miles of track and determines the starting and stopping and speed of their trains. It goes to the cities of the Gulf and of the Pacific as well as to those of the Atlantic coast—noted everywhere as an important element in the safe, speedy, and accurate conduct of commerce; and so the work of the regulating clock of the Observatory, sent out by means which note the minutest fraction of a second of time, is playing its important part in the economy of our century. I can not follow it out in detail; every one will do so to some extent in his own mind. But if we were to divide human history into eras according to the minuteness with which the passage of time is observed in the ordinary affairs of life, we should find ourselves to have arrived, and very lately, in what might be called the era of seconds.

At the opposite extreme is the period when the passage of day and night reveals itself to the dullest intellect. Perhaps no savage people have ever been so dull as not to have noted more than this. We can hardly conceive a state in which the brutal hunter did not take note of the declining sun and observe that the close of the day was approaching

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* Read before the Anthropological Society of Washington, April 5, 1887. (From the *American Anthropologist* for January, 1888, vol. I, pp. 25–50.)

ing. The lengthening of his own shadow was an always present phenomenon, and men must have observed shadows almost as soon as they became capable of observing anything. But this kind of observation went on for ages without any attempt to sub-divide the day, and none but the great natural periods marked off by sunrise and sunset were recognized.

Between this period, marked by the observation of the natural day only, and that in which we live, there have been many steps of progress, the very dates of which may in some cases be quite distinctly observed. We find an era where noon begins to be noted, and the natural day is equally divided by its observation. Then we find an era in which either the entire day or its great natural fractions are again divided into smaller fractions of rather indefinite length, as is now done by some savages and as was done in the earlier history of Greece and Rome. Next to this comes the era in which definite artificial fractions of the day are observed, which may be called the era of hours. It was many centuries after this before men in the ordinary transactions of life counted their time by minutes, but the time when this began is quite distinctly marked.

I would not say that these eras are contemporaneous in all nations, nor could I assert that they correspond closely with any recognized stages in civilization and culture; in fact, the observation of hours of the day does not appear to obtain until civilization is reached. This is true however,—men measure most carefully that which they value most, and the value of time is enhanced just in proportion to the multiplicity of the demands upon it which the existing state of society involves. The man who has engagements at the bank, the custom-house, his own warehouse or factory, and in a court-room, and a dozen or more individuals to meet, each of whom, perhaps, has similar pressing engagements, and then must reach an express train at 4:30 in order to dine at 6, fifty miles away, must allot his time with the greatest care and measure it with the utmost minuteness. To the savage, the sun rises and sets, and rises again;—one day is as another; nothing presses but hunger, and that he endures till fortune brings food. He needs no clock to tell him it is dinner-time, for it is always dinner-time when there is food. When people travelled leisurely by stage-coach, walking up the hills to rest the horses, stopping at the wayside inns to dine, and well content at the close of the day if 50 or 60 miles had been covered, seconds of time and even minutes were of little account; but when trains are run on a complex schedule, and for a whole season in advance it is set down at just what place each train must be at each moment of every day, and the safety of lives and property depends on exact adherence to the prescribed order, then the station clocks must be invariable and synchronous and the conductor's watch true to the second. Civilization is marked at every step of its progress by the multiplication of the varied relations between men, and since the importance of time is enhanced by the same multiplication, it may fairly be asked whether the accuracy with which time is observed in ordinary life, may not after all afford one of the most perfect indications of the social condition of a people.

The material is not gathered for a full discussion of a question like this, and I shall not occupy myself with it, but as incidental to and suggested by the topic I have chosen, some light seems to be thrown on it by the attempt to place in their true correlation facts of history not hitherto brought together. I have proposed to myself only a study of the growth of the common clock, noting the various steps in its development with reference to their period in history, and to the social conditions which inspired or demanded them, as well as to the state of science and mechanic arts which made their consummation possible. The subject is too large for a single paper, and I have therefore taken for present consideration that part which relates to time keeping among the ancient peoples from whom we chiefly derive our civilization and to a period of history which, by a sort of coincidence, practically terminates with the beginning our of era. My guide in this inquiry will be the principles in eurematics that inventions always spring from prior inventions or known expedients, and that they come in response to recognized wants. It need not be repeated that these principles find copious illustrations in the progress of every art; but the truth can not be too strongly enforced that the progress of no art can be intelligently studied or thoroughly comprehended without keeping them in mind.

The few barren and isolated facts that have been preserved to us regarding time-keeping prior to about six hundred years ago are not enough in themselves, however carefully collated, to constitute an intelligible or consecutive history. But I need not say that no event is in fact isolated from all others in cause and effect; and if we can not have direct light we may look to the concurrent events of history for side lights upon our meager facts which will perhaps throw them into stronger relief than the direct narration of unphilosophical historians. Hence, if I shall seem to any one to lean too much upon the synchronisms and sequences of history, it is not that I do not realize the possible fallaciousness of an argument which has no other foundation; but in the progress of inventions such sequences are to be sought for. Invention responds to want, and the want may originate in some crisis or event having no apparent affinity in character with the want it engendered or the invention that sprang to meet it. And these are not mere accidents; they are the natural course of what I venture to call the fixed laws of eurematics. At the same time these laws do not necessarily always call for original invention, since importation of an invention already known elsewhere may equally supply the want, and historical crises are as likely to lead to importation, where it is possible, as to invention. It is with these principles in view, and always looking for such side light as contemporary events can give, that I have attempted to frame the consecutive history of time-keeping, of which this paper is a part.

There are three primitive forms of time-keeping instruments—the sun-dial, the clepsydra or water clock, and the graduated candle. The last plays no part in the evolution of the modern time-keeper, and I shall pass it by without further notice, notwithstanding some interesting historical associations connected with it. But the sun-dial was at the beginning the only time-keeper, and man's ideas, developing into wants, led to its greater perfection till these wants passed far beyond what, with its limitations, it could supply. Its contribution to the present state of the art was not large, mechanically considered, but it was enough to create the demand for something better, and without this contribution the art could not have been. The rude utensil which the Greeks called a clepsydra had no resemblance to the perfected time-piece of this century, but nothing in history is surer than that out of it, by slow accretions, science and art, by turns mistress and handmaid, have produced the masterpiece of both.

This history is, therefore, the history of a human want and of a mechanical structure developed in response to it. But wants grow, and this has grown; and in tracing it we do not find it always in the same likeness. Sometimes the want of the moment is satisfied, and then it appears in a novel and unexpected form, altered in its whole complexion by that which has just appeased it. And as we recognize this Protean character, we need not suppose that the Babylonian astrologer who made some improvement in a sun-dial had a single idea or purpose in common with those of a railway manager who last week connected his regulator by wire with the Observatory. We trace our want in the development of institutions, in the creation of new demands upon time, in the growing complexity of human relations, in political crises, and we may determine its character or intensity by the means used to supply it and the generality of their adoption. The story of the growth of the instrument is inseparable from that of the growth of civilization.

Writers on the history of the clock (and they are not few) have generally begun by a reference to the sun-dial as a Babylonian or Chaldean invention. We can trace it no further, and have no means of determining when the invention was made. We learn from the Old Testament Scriptures that it was known at Jerusalem as early as seven centuries before our era, and the manner of its mention indicates that in that city it was a novelty. King Ahaz, by whose name this dial is called, had introduced other novelties into his capital on his return from Damascus, whither he had gone to make his submission to Tiglath-Pileser II, King of Assyria; and it is not unreasonable to suppose that the dial had the same origin. However this may be, it was a graduated instrument, having degree marks of some kind which showed the daily course of the sun. We may infer that it was at least of a Babylonian pattern, and it points to a remote period when a graduated dial indicating the time of day by a shadow passing over it was known to Oriental peoples.

Presumably it was their invention. The suggestion that they derived it from Egypt is a guess only, based on the supposed earlier growth of Egyptian science. To such a guess might be opposed the fact that in all the Egyptian monuments yet explored there is no hint of such an instrument.

The Assyrian monuments are equally silent; and the same speculation which attempts to account for the absence of all representation of a sun-dial in the sculptures which have revealed to us so much of the domestic life of the Assyrian people applies to Egypt also. We may believe that it was not a device generally known or commonly used. Very likely the knowledge of it was confined to the priests and magi, who were not only ministers of the religion of each country, but the masters of its science. This device constituted a part of their mystery and was religiously kept from the public knowledge. In support of this conjecture it may be said that the Phœnicians, who penetrated every land, dealt in every merchantable commodity, and from their active commercial habits were the very persons who would have found the use of a time-piece most valuable, do not appear to have known of any such instrumentality; but the inner temples of Thebes and Babylon were not open to those hardy mariners, and the exhumations of Cyprus reveal no more to us than those of Nimroud and Memphis.

It is scarcely profitable to grope in the darkness for the origin of the sun-dial; but certain facts are apparent and may be briefly indicated. In Egypt and Assyria observation of the heavenly bodies was a part of the religious cult. The regulation of the calendar belonged to the ministers of religion. For the regulation of the calendar, which of course involved the determination of the length of the year, the recurrence of the solstices must be noted; and these could only be noted by observation of the day when the shadow cast by the sun at noon was at its maximum or minimum. The observation of shadows for the determination of noon led (it could scarcely be avoided) to their further observation during the entire period of the sun above the horizon, and, at last, to marking the surface on which the shadow was cast by permanent lines dividing the day into some kind of regular parts. All this might be done as a matter of scientific observation without conscious need of a time-piece.

The sun-dial took many forms, and more than one of these may have been known to the Babylonians. The art of dialing involved mathematical problems of considerable complexity, and the study of this art very likely contributed to the knowledge of mathematics that the world possessed at that early period. The consideration of these forms is not germane to my present purpose, which is for the moment only to show that long before the appearance of the sun-dial in Greece the instrument had been apparently perfected by the wise men of the East.

Historians have agreed in fixing the period of the introduction of the sun-dial into Greece in the latter part of the sixth century B. C. Herodotus says it was derived from the Babylonians, from whom he also declares the Greeks to have derived the twelve parts (δυώδεκα μέρεα) of the day. Others however ascribe its invention to Anaximander, who is said to have set it up in Lacedæmon. It is evident that he need not have invented it, but might have brought it from some country where its use was already known. It is significant that Anaximander and Anaximenes (to whom some writers ascribe the honor of the invention), were both fellow-citizens and pupils of Thales of Miletus, and that the date of this introduction synchronizes with the extensive and intimate acquaintance between Egypt and Greece, which, commencing in the reign of Psammetichus, reached its culmination under Amasis, the fourth king of that dynasty, and in which the people of Miletus bore the most prominent part. Under this last king, whom they assisted in throwing off the yoke of Assyria, Greeks swarmed in the Egyptian court, filled her armies, manned her fleets. They passed to and fro continually; Greek philosophers pursued their studies in Egyptian schools; and who shall say how many of the secrets of art and science found their way at that time from the land of the Pharaohs to the spirited and versatile people just emerging from barbarism across the Mediterranean? Surely, if under such conditions anything of Egyptian origin or likely to have been in Egyptian possession is found to have made its appearance among the Greeks, we need not speculate as to how it got there.

It does not appear that the sun-dial was introduced to the Greeks in any perfected form. On the contrary, it was at first a mere staff or pillar (γνώμον), destitute of any graduated dial which could indicate the passage of an hour or any definite fraction of a day. The length of the shadow, measured in feet, determined the time for certain regular daily duties, as a shadow 6 feet long indicated the hour for bathing and one 12 feet long that for supper. More accurate and convenient forms were perhaps known to philosophers; but if so, they did not come into common use. This simple device was sufficient for the simple habits of the people. The twelve parts of the day of which Herodotus speaks had no meaning to the Athenians, who had no word meaning specifically an hour; and as late as the time of Alexander, the old system seems to have been followed. This kind of observation, it may be remarked, was perfectly feasible in the shadow of an Egyptian obelisk, which may partly account for the absence of the instrument from other monuments of that country. As a matter of history, an obelisk at Rome was actually used for a sun-dial in the time of Augustus.

We learn from this history at what period and in what stage of progress the Greeks first had the idea of measuring time. If we associate it with the period of Solon, the Athenian law-giver who died about 570 B. C., we may form some idea of the condition of the people of Athens from the character of his legislation and the miseries he attempted to mitigate. The Greeks had written language and they had literature,—Homer, Hesiod, Sappho. They had a system of weights and measures, and a coinage. They were prolific in political ideas. But the period just previous to Solon was marked by the tyranny of the oligarchs, the severity of whose legislation gave the term "Draconian" its significance, by widespread poverty, by slavery, by the decline of agriculture and industry, and by the unceasing war of factions. Athens was emerging from such conditions as these, under the reign of Pisistratus, at the time when the Milesian philosopher is said to have introduced the sun-dial. We may conceive that the conditions were not favorable to the general adoption of any novelty of this character, but it is noticeable that this period was followed immediately by one of democratic ascendency under the constitution of Cleisthenes, in which the naval power and commercial importance of Athens were vastly augmented, and which continued without interruption until his invincible phalanxes laid all Greece at the feet of Philip of Macedon.

It was during this era of maritime vigor, of commercial prosperity, and of dominating influence at home and abroad, that Athens achieved that splendor in art which has made her a beacon-light for all subsequent peoples and ages; and in this period, time-keeping in common life had its first development. But the sun-dial is an instrument of limited capacity; however perfected, it was valueless in the hours of night and in the days of cloud and storm that even sunny Greece does not always escape. But more than this, it was incapable of in-door use; and in the outgrowth of institutions under democratic order and among a litigious and voluble people a new and singular want had arisen demanding some means of checking time which, from its limitations, the sun-dial could not supply. With her other arts, that of oratory had developed in Athens; but every orator was not a Pericles, and whatever may have been the merits or defects of their performances the inordinate length of these was too great a tax on the tribunals. It therefore became necessary to limit and apportion the time of public speakers in the courts, and to do this equitably some practical means of indicating time was necessary. Hence arose the demand for another instrumentality whose origin and history are now to be traced.

It is proper to pause for a moment here to note a distinction between two kinds of instruments used to measure time. A continuous instrument like a clock, which marks off the hours of the day and night as they pass successively away, is what is called in common language a time-keeper; but there is a class of instruments which do not keep the record of continuous time, but are used only for the checking of brief periods; such an instrument is the glass by which the seaman observes his log or the cook boils her eggs. To such instruments, for the want of a better term, I give the name time-checks, to distinguish them from time-keepers. Their use is quite distinct from that of observing the time of day, and yet it is apparent at once, that by careful attendance, as by turning the hour-glass at the moment when its last sand has run out, the time-check may be made to perform the office of a time-keeper.

The allusions of ancient writers and of some modern ones to devices of these two classes are sometimes mis-leading and confusing because this distinction has not been kept in view. It is particularly important in the study of the clepsydra, which is originally a time-check only, while the sun-dial is a true time-keeper.

The clepsydra or water clock, in its simplest form, is traced by historians no further than Greece, about 430 B. C., in the time of Aristophanes, whose familiar references to it show its use for certain purposes to have been common.

I confess I have been far from satisfied with stopping at this half-way house in seeking for the origin of this instrument. I have sought further, and what I have found, if conclusive of nothing, is at least suggestive.

If, taking our lives in our hands, we could step on board a Malay proa, we should see floating in a bucket of water a cocoanut shell having a small perforation, through which the water by slow degrees finds its way into the interior. This orifice is so proportioned that the shell will fill and sink in an hour, when the man on watch calls the time and sets it afloat again. This device of a barbarous, unprogressive people, so thoroughly rude in itself, I conceive to be the rudest that search of any length can bring to light. It is in all aspects rudimentary. One can scarcely conceive of anything back of it but the play of children, and as a starting point for this history, it is much more satisfactory than what is disclosed in the polished ages of Greece. There is nothing in its structure, if we were to consider that only, to prevent it from being a survival of an age long antecedent to the use of metal. The protolithic age might have originated it if can conceive that protolithic man could have had use for it.

Leaving our piratical friends, to whom we are so much indebted, and passing to their not remote neighbors in Northern India, we find the rude cocoanut shell developed into a copper bowl. Its operation is the same; but the attendant who stands by and watches the moment of its sinking, now strikes the hour on the resonant metal. It is easy to see—in fact it would be difficult to doubt—that this has been an improvement on an apparatus like that of the Malay and the natural result of improvements in other arts, eminently that of metal-working. It is more enduring, more perfectly accomplishes its purpose, and is in the precise direction that improvement on the ruder appliance might be expected to pursue.

Passing from Southern Asia to a people geographically remote, I next observe the water clock in use up to this day in China. We find the metal vessel with its minute perforation as before, but it has undergone a radical change in respect to its manner of use. It is now filled and the water flows from it in drops. Obviously enough the flight of time might be indicated by merely observing when the vessel has emptied itself, and then re-filling it, which, as will presently appear, was exactly the simplest Greek and Roman clepsydra and differs in no mechanical respect from the ordinary sand-glass.

But in the days when the Chinese were a progressive people and developed inventions for which Europe had many centuries to wait, this water-clock advanced far beyond the crude thing we have been considering. It would seem that the problem was to increase its usefulness by sub-dividing the unreasonably long intervals required for the complete emptying of the vessel. If this was done by marking graduations on the inside of the vessel and so noting the decline of the level the difference in its rate could not fail quickly to make itself manifest. The solution of this problem, not obvious at first, was found in so arranging the vessel that it should discharge into another, where the indication would be read in the rise of the surface, and contriving to hold the water in the upper vessel at a constant level. This was done by employing a third source, from which there was a constant flow into the first equal to its discharge. As the head in the middle vessel is thus maintained constant, the rise in the lowest is made uniform. Another radical improvement enhancing the practical utility of the device was the arrangement of a float on the surface of the water in the lowest vessel. Upon this was an indicator or hand, which in its rise travelled over an adjacent scale, and so gave a time indication visible at a distance.

To show what progress this structure implies in the development of the mechanical clock it is worth while to glance a moment at the essential elements of such an instrument. Reduced to its lowest terms a clock consists of three elements only. These are a motor, or source of power, represented in our clocks by a spring or weight; an escapement, or a means by which the stored power in the motor is let off at a measured rate; and a dial, which is but the means by which the rate at which the power is let off is made visible to the eye. In this Chinese water-clock we discover all these elements. Water, acted on by gravity, is a familiar form of motor; the small perforation through which it slowly trickles drop by drop is a true escapement, doing in its place just what our complicated mechanisms are doing in theirs; and, rude as it may appear, it is one which mechanicians of our time are not ready to dispense with. The visual indication is given by the rise of the float, causing the pointer to pass over the scale. Going backward from this Chinese clock we perceive, but less distinctly, the same elements in the Indian and Malay devices, in which the operation is reversed. In these the weight of the vessel, held up by the resistance of the water in which it floats, is the power; the perforation admitting the water by slow degrees is the escapement, and the only indicator is the visible sinking of the vessel itself.

The three devices described correspond in the degree of their perfection with the conditions of art and culture among the peoples to which they belong; and, as these conditions appear to have been unchanged for a long period, we hazard little in assuming that they date from a remote epoch. A description of the Hindoo instrument appears in a Sanscrit work on astronomy in which it is adopted for astronomical observations, and Chinese writers do not hesitate to ascribe the invention to Hwang-ti, who flourished, according to their chronology, more than twenty-five centuries before our era, and its later improvement by the introduction of the float to Duke Chau, fourteen centuries later.

In describing these three devices in the order in which I have placed them I do not mean to be understood as intimating that they have followed the same order in respect to the time of their development nor that they have been transmitted from one people to another in the same order. I have, for convenience, proceeded from the lowest form to the highest; but it may well be true that the lower was an adaptation from the higher, fitting it for coarser needs, and so being in a certain sense an improvement. Consideration of the lines of commerce might in fact lead to the suspicion that the Malay got his notions from the Chinese, since they must for many centuries have sailed the same waters and been in frequent contact.

But we may come further west. Writers on this subject, while attributing to the Chaldeans the invention of the sun-dial, do not generally accredit them with the knowledge of any other instrument for measuring time. But if we may take as an authority Sextus Empiricus, who wrote near the end of the second century of our era, they had, as he tells quite minutely, the same device, and used it in their astronomical observations. "They divided," says this author, "the zodiac into twelve equal parts, as they supposed, by allowing water to run out of a small orifice during the whole revolution of a star, and dividing the fluid into twelve equal parts, the time answering for each part being taken for that of the passage of a sign over the horizon." I see no reason for doubting this. In fact the division of the zodiac into twelve signs seems to require a means of measuring the passage of time at night, and this fact and the story just quoted tally with the conclusion that an instrument of the common generic character borne by all the forms I have described was known among widely distinct peoples of Asia before the dawn of European civilization.

Such an invention is not likely to be lost by political changes while supremacy in the exact sciences is maintained. We know that down to the Medo-Persian conquerors of Babylon each successive dominant race adopted, as has often happened in history, the dress, the manners, and the arts of the conquered; and we need not doubt that this instrument was in use in the Persian Empire when its sword first crossed that of the Greeks.

No record exists of the introduction of the clepsydra into Greece. We might infer from the absence of all reference to it by Herodotus that up to the period when his history ends, 478 B. C., it was not known. Fifty or sixty years later, when Aristophanes was writing his comedies, it was absolutely familiar in Athens. The interval named seems short in accounting for so radical a change in the habits of a people as is implied by the general introduction of such an appliance; and yet, if we ask ourselves as to the condition of the electric telegraph or the sewing-machine fifty years ago or of the telephone ten years ago, it need not startle us to conceive that a versatile people like the Greeks were capable of as swift changes in their habits of life, as these inventions have induced in ours. That this epoch saw more than one change in Athens, in the aspect of the city, in the habits of the people, and above all in their advance in culture and refinement and the arts of peace, we may be sure when we remember that it includes all the years of Pericles' administration. It includes also the abandonment by Sparta, always unprogressive, of the leadership of the Greek commonwealths, and with this abandonment the removal of the re-actionary influences hitherto a clog to the enterprise and prosperity of Athens and of all Greece.

In the absence of data on this subject it seems not unreasonable to believe that the knowledge of the clepsydra, which was widely spread among Oriental peoples, was introduced into Athens from the East during—or at the termination of—the second Persian war; and if we choose to surround its introduction with the halo of romance, it is not hard to conceive that these useful devices of civilization were gathered up among the spoils of Platæa or washed ashore with the wrecks of Salamis. A more commonplace and not less likely conjecture would be that the instrument was already becoming known in the Greek colonies of Asia, and perhaps even in Athens herself, through intercourse with the Persians and other Oriental peoples. It came into common use in obedience to the want, not of a time-keeper, which was already supplied, but of a time-check,—a want created by the conditions of Athenian society which I have already described, and which the only known time-keeper could not satisfy.

If the increasing burden and tediousness of litigation led to the enactment of a statute restricting and apportioning the time of speakers in the courts, and providing this means for its regulation, it is easy to see that the use of such means must become at once familiar. I have found no trace of such enactments, but that strict ordinances existed there is no doubt. We know that the time of speakers was carefully proportioned to the importance of the case; and trials of importance enough to have the time apportioned were known as *προς ὕδωρ*, while those of trifling importance, in which perhaps no lawyer appeared, were known as *ἄνευ ὕδατος*, two terms which may be freely rendered *wet* and *dry*, the dry case being as it happens most quickly disposed of. In a case of great moment to the State, involving a charge of faithlessness in an embassy, each party was allowed 10 amphoræ, or about

50 gallons of water. Nothing however seems to be known of the actual length of time indicated by this quantity of water. A passage in Aristotle gives some idea of the form of the clepsydra as commonly used. It was a spherical bottle with its minute opening at the bottom and a short neck at the top, into which the water was poured. The running out of the water at the bottom could be stopped by closing this neck. In using the word bottle I do not mean to imply that this clepsydra was of glass. Glass vessels of a suitable size could not be made at that period.

The familiar association of this device with the courts is shown in many ways. Aristophanes throughout his comedies is in the habit of using the word clepsydra as a synonym for court of justice, and in a humorous passage in *The Wasps* the impossibility of conducting a trial without it is quite forcibly set forth, by the introduction, to supply its place, of a vessel intended for less refined purposes. In fact, ὕδωρ became a synonym for time. We find Demosthenes charging his opponent with talking ἐν τῷ ἐμῷ ὕδατι, "*in my water;*" and on another occasion he shows the value he attached to the time allotted to him by turning to the officer, when interrupted, with a peremptory σὺ δὲ ἐπίλαβε τὸ ὕδωρ, "*You there! Stop the water!*"

I shall again have to refer to this use of the clepsydra when I come to the Roman period of this history, and will not follow it further now; nor shall I consider its use as a time-keeper, which, if ever general in Greece, was not until a very late period, belonging rather to the Roman chapter also. The story that Plato had a clepsydra which indicated the hours of night is of little moment, although it is frequently taken as indicating some kind of a striking apparatus; but the language of the author who is the only authority for the statement contains no allusion to an audible signal, nor in fact any intelligible allusion except to a larger clepsydra than usual.

In fact, all the improvements by which this instrument was converted into a time-keeper belong to so late a period of Greek history that it is more convenient to consider them further on.

Where Greek colonies were founded, and where Greek influence predominated Greek acts and culture flourished also. Under the Ptolemies, Alexandria became a second home of art and science, not inferior to Athens herself. To a greater or less extent the same must have been true of the great cities which dotted the northern coast of the Mediterranean, such as Tarentum, Agrigentum, and Syracuse. With kindred people, similar culture and needs, and with unceasing commercial intercourse, there is no reason to doubt that whatever was in common use in the mother cities found its way to them also. It was in Alexandria that in the shape of what is appropriately termed the water-clock the clepsydra attained its highest development, in the inventions of Ctesibius, who is placed by some writers in the third century B. C. and by others with more probability in the second. I reserve these inventions also for the latest epoch in this history, to which they seem more properly to belong, and will now pass to Rome.

There is no reason to believe that the Etruscan people, with all their proficiency in certain arts and a vigorous and extensive maritime commerce, possessed any artificial means of indicating time. If they had, it could hardly have failed to come into use among the Romans, whose relations with them for centuries were close, even if generally hostile. But it was not till a late period, long after Etruria had been crushed under the successive assaults of her northern and southern enemies, that any device of this character was known to the people of Rome.

Indeed, the condition of society and of the arts in Rome at that era was not such as to require any reckoning of the time of day beyond the observation of sunrise and sunset. In the twelve tables, which date from the middle of the fifth century B. C., noon also is mentioned. But the facts that history has preserved to us show that the Romans of that time were a thoroughly rude and almost barbarous people. It was not till two centuries later than this, in the year of Rome 485 (268 B. C.), that silver coinage was first struck. Pliny says that barbers were first introduced about the same time, and that till then the Romans had gone unshorn. Cicero says the arts which had reached some degree of perfection in Etruria were even allowed to retrograde. He says the Romans had some knowledge of arithmetic and land surveying, but they could not improve their calendar, and were not even in condition to erect a common sun-dial. As to the state of commerce and agriculture, we are told that in the fourth century of Rome, private enterprise was so inadequate to the provisioning of the city, that state commissioners were placed in charge of it.

It would seem that Rome was at that period a capital, populous indeed, but without arts or sciences, without industries and without cultivation. War was the only trade and plunder the only source of public or private revenue. For the civil purposes of such a people the natural divisions of time were all that were necessary. They marked the periods for toil and repose, and that was enough.

These were a ruder people than those of Athens in the time of Solon; but if they had less of culture they had less of tyranny and less of intestine warfare to contend with at home than had the Greeks, and they were always reaching out, widening their domain, absorbing neighboring peoples, and making each in its turn add to the strength and glory of their capital. Whatever the art and science of the subdued nations could contribute to the prosperity of Rome, came by the enforced levy of the conqueror.

The time system of early Rome was, like everything else, of the rudest character. Growing out of their military habits and adapted to them, it divided the day and night each into four watches, the periods of which must have been roughly determined by observation of the courses of the sun and stars. In the city, according to Pliny, noon began to be accurately observed some years after the publication of the law of the twelve tables. The *accensus* watched for the moment when, from the Senate House, he first caught sight of the sun between the Rostra and the Græco-Stasis, when he proclaimed publicly the hour of noon. From the same point he watched the declining sun and proclaimed its disappearance.

Authorities differ as to the date of the introduction of the sun-dial into Rome. Pliny attributes it to the consul L. Papirius Cursor, who set it up at the temple of Quirinus. This has been supposed to be a trophy from the Samnite war, but, as the Samnites were a ruder people even than the Romans, that seems scarcely credible. Varro, as reported by Pliny, gives a clearer story, that the first public sun-dial erected in Rome was fixed upon a column near the Rostra in the time of the first Punic war by the Consul Valerius Messala, and adds that it was brought from the capture of Catina. The date given by Varro, 491 A. U. C., corresponds to 262 B. C., and is about thirty years later than that ascribed by Pliny to the dial of Cursor. As a source for this instrument, Sicily with her Greek arts and refinements, is much more probable than the rude Samnite people, and with real appreciation of Pliny's frankness, we may accept the story he quotes from Varro in preference to his own.

What were the social conditions in Rome at this period, the middle of the third century before our era? It needs scarcely more than a glance at a chronological table to see that it was a period of swift advance from the primitive rudeness that has been described. In the year 283 B. C. Etruria and her allies, hitherto perpetual foes to Rome, were totally defeated at the Vadimonian Lake, and about 265 B. C. Etruscan independence disappeared forever, simultaneously with the subjugation of all Italy. The whole peninsula her own, Rome reaches out beyond. The Græco-Egyptian monarchy, then at the very height of its power and magnificence under Ptolemy Philadelphus, seeks her alliance. The Greek cities across the Adriatic court her favor. She pushes her conquering arms across into Sicily, which, in 241 B. C., becomes a Roman province, followed a little later by Corsica and Sardinia. No longer *prima inter pares* among the warring tribes and nations of Italy, she has sprung as if at a single bound into her position as one of the great powers of the world.

The absorption of Magna-Græcia and Sicily brought under her dominion for the first time a cultured people and populous cities, filled with and habituated to Grecian art and the appliances of refinement and luxury, and the sun-dial of Catina is but one instance of what was borne away to embellish the Imperial City. Doubtless the fame and wealth of the capital offered strong inducements to the skilled artisans of dismantled Tarentum, while the captives of Agrigentum may in their turn have contributed in no small degree to her industrial population.

The colonists planted by thousands far and wide over the conquered territory of Italy formed a sturdy rural population,—a strong reliance in peace and war. And the great highways built for the march of the legions, and hitherto scarcely resounding but to their armed tread, now became the arteries of a steady and growing traffic. The needs of a circulating medium in her domestic and foreign trade were ill supplied by the copper coins she had struck hitherto, and the products of various foreign mints that had come to her with her other acquisitions; and in 258 B. C., she began to coin silver of her own. Carthagenian jealousy of her aggressive rivalry led to the necessity of maintaining a fleet, and (after some disasters) to maritime supremacy.

"The ten years preceding the first Punic war," says Dr. Thomas Arnold, "were probably a time of the greatest physical prosperity which the mass of the Roman people had ever seen," and it is in this very decade, with enlarging industries, with a growing commerce, with multiplying complications in public and private business, that Rome stepped from the spring time of her history into her vigorous summer, and with this step time-keeping began.

The Catanian sun-dial was no mere gnomon such as had been introduced into Greece three centuries earlier. Greek science and genius had been at work on it, and it was an improved instrument, constructed for a particular latitude, and that 5° south of Rome. But there was no science yet in Rome to detect its imperfections, and, in spite of them, for ninety-nine years it served as the regulator of time for the city. Scarcely credible as it may seem, it was not therefore till about a century and a half before the Christian era that Rome possessed her first accurate time-keeper in the form of a sun-dial constructed especially for her own latitude, which was set up at the instance of the Censor Marcius Phillippus. Meanwhile dials of imperfect construction had become common in the city; so common indeed, that as new inventions nowadays afford material for the American paragrapher, they became the happy source of quips and epigrams. Thus Plautus, in what I admit is rather a liberal version:

> When I was young, no time-piece Rome supplied,
> But every fellow had his own—inside;
> A trusty horologe, that—rain or shine—
> Ne'er failed to warn him of the hour—to dine.
> *Then* sturdy Romans sauntered through the Forum,
> Fat, hale, content; for trouble ne'er came o'er them.
> But *now* these cursed dials show their faces
> All over Rome, in streets and public places;
> And men, to know the hour, the cold stone question,
> That has no heart, no stomach, no digestion.
> They watch the creeping shadows—daily thinner—
> Shadows themselves, impatient for their dinner.
> Give me the good old time-piece, if you please,
> Confound the villain that invented these!

As formerly, in Greece, the clepsydra came to supply the deficiencies of the sun-dial, so history repeated itself in Rome. Pliny ascribes its introduction to Scipio Nasica in the year of Rome 595 (158 B. C.). Of the form of this clepsydra we have no knowledge, but it was no longer a mere time-check, such as was used in the Athenian courts, but a true time-keeper, capable of indicating continuously the hours both of day and night. There were many adopted for this purpose, as will presently be shown. In Pompey's third consulship (52 B. C.), he introduced the custom of apportioning the time of orators in the courts by the clepsydra, after the Greek fashion. The decline of Roman oratory has been attributed to this restriction, which, after all, seems to have left the speaker a fair amount of time. Pliny says: "I spoke for almost five hours, for to the twelve clepsydræ of the largest size which I received, four were added." Some read *twenty* in place of *twelve*, which seems to be the preferable reading, and out of it we get some idea of the time consumed by one discharge of the vessel. If twenty-four clepsydras is "almost five hours," it appears likely that the discharge was at the rate of five to the hour; and this helps us to better understand Martial's epigram to a tedious lawyer who had been permitted to exhaust the clepsydra seven times. It makes something less than an hour and a half; but the orator's mouth was as dry as his discourse, and he drank copiously, whereupon the witty poet suggests that he can satisfy his thirst and his audience at once by drinking out of the clepsydra.

In Rome at this period the use of the clepsydra, in the form both of a time-check and time-keeper, was quite general,—not as the house clock is common to-day—but generally known, and serving to regulate the hours of business and pleasure. Men of means had them in their houses, and slaves were kept whose special duties were to watch them and report the hour. Idlers meeting in the market-place or forum accosted each other with "*Hora quota est*," by way of opening conversation, as they now comment on the weather or compare watches. Generals took the water-clock with them to the field and relieved the watch by it during the hours of night. An allusion by Cæsar has been the source of a curious misconception, that he found this instrument in use among the Britons at the time of his invasion. Evidently referring to the phenomenon now so familiar of the Arctic night he says some had reported that at Mona the night at the winter solstice lasted for a month. "Our inquiries," he continues, "did not confirm this, but by careful measurements *ex aqua* we saw that the nights were shorter than on the continent." To draw from this the conclusion that the early Britons had water-clocks is about as if we were to infer from the Signal Service observations at Point Barrow that the Eskimos of that region were found in possession of the thermometer.

Greece too had by this time fallen under Roman rule, and the clepsydra as a time-keeper was well known in Athens. The most eminent instance of it probably for all time, was in the Tower of the Winds, which, fifty years before our era, was erected in the market-place in that city. A running stream kept at a constant level the water in an upper vessel, the discharge from which raised a float in a lower one, like that in the Chinese water-clock before described. This was the public timepiece of Athens, and its indications could always be compared with those of the sun-dials on the frieze of the octagonal building by which it was inclosed. At the top of the roof was a weather-vane in the form of a Triton, who pointed with his trident towards the prevailing wind. This institution served for Athens the combined purpose of a naval observatory and a weather bureau.

With time-keeping so generally observed, and with a fair degree of accuracy secured by means of mechanical contrivances, this history closes, but in reciting it I have omitted or only incidentally touched upon the growth of the idea of dividing the day into hours and the mechanical elaboration of what—in its perfected form—is properly termed the water-clock. These elements, in the complete history, are too important to be omitted.

Since we are only concerning ourselves with time-keeping in common life, we need not go back to Egypt or Babylon, where there is no evidence that it was known except to the initiated few. Whatever ideas are conveyed to us by the twelve divisions of the day known to the Babylonians, or by the graduated dial set up by the Hebrew king in his palace, it is evident that if the Greek philosophers derived from their Eastern contemporaries any notions of common or domestic time-keeping, these failed to take root in their soil until Greece, by her own progress, had prepared it to receive them.

The divisions of the day known to Homer were three: *ἠώς*, for the period from sunrise till noon; *μέσον ἦμαρ*, for mid-day; and *δείλη*, for afternoon till sunset. These divisions were employed in Greece to the latest period and long after others more exact were in use. Even with our nice observance of time we have similar general expressions for parts of the day, such as morning, mid-day, afternoon, and many others often having only local use.

If the Babylonian "twelve parts" of the day were made known to the Greeks, as Herodotus tells us, it was a knowledge for which they had no use at that period. With the introduction of the gnomon they began to observe time more closely, but they had no names for its arbitrary divisions.

When the shadow was 6 feet long it was time to bathe; when twice that length it was time to sup. It is not even certain, to my mind, that they clearly appreciated the varying length of the day. There is no possibility of setting a summer and winter day side by side and comparing them, and the difference between them can only be determined by some means of measuring time quite distinct from observation of the sun or shadows. The great difference between the days of winter and summer in our latitude, which is nearly that of Athens, seems to us to be plainly discernable; but if we could divest ourselves of our acquired knowledge and of our means for keeping time, and put ourselves in the place of the Greek of 600 B. C., we should probably fail to observe the fact except very dimly.

Accurate division begins with the observation of noon, and we have seen pretty clearly when this began in Greece. The next step in subdivision consists in dividing the day into quarters by dividing equally the periods before and after noon. This division was at least known to the Greeks, but I see no evidence that it was in common use; nor in fact does it appear that they in daily life made use of close sub-divisions, until Roman influences prevailed and the Roman divisions of the day were adopted.

In Rome the division of both the day and night into four watches resulted naturally from the military character of her people and remained in use down to the latest period. These divisions of the day corresponded with what were afterwards the third, six, and ninth hours, and it was customary for one of the subordinate officers of the prætor to proclaim them. They had also a three-part division corresponding to that of the Greeks.

Artificial means of measuring time came to the Romans so much later than to the Greeks that great improvements had been wrought in them. Science had gone so far in Egypt and Sicily that sun-dials were constructed for particular latitudes; but it is not clear that, as at first introduced, they were graduated. The same sub-division of the day into four watches that has just been noticed might obviously give the first suggestion of such graduation by bisecting the angle between the noon-mark and those of sunrise and sunset. As a closer sub-division was required the Romans appear to have taken one already known in Egypt and better adapted to the latitude of Thebes and Memphis than to that of Italy. This was the division of the day and night into twelfths (which varied in their length as the seasons changed) and is commonly known as the Roman system. Before intimate relations began between Rome and Egypt, Greece had already been annexed and the same system was introduced there, as also in Palestine, and wherever the Roman eagles penetrated. This division adapted itself perfectly to the older one already in use in Rome and its adoption was natural. The only change in the sun-dial that it involved was a further sub-division of the spacing. Being an improvement that cost nothing and could be adopted without any radical changes in the habits of daily life, it was one to commend itself to the people, who were slow to change; and when a few years later, in the middle of the second century B. C., Hipparchus proposed the division into equinoctial hours, the same as used now, the proposition met no welcome. This accurate and convenient system did not adapt itself to the established notions of the times, and the Roman hours secured a firmer and firmer grip, resulting, as I am inclined to believe, in one of the most remarkable instances of retardation of invention that history records. It was not until Europe had emancipated herself from slavery to this most awkward of time systems that modern time-keeping became possible. For many centuries invention was as it were thrown off the scent by the necessity of converting the regular and uniform motions which could be given to mechanism into means for displaying the ever-varying hours of the Roman system.

The word "hora," proposed by Hipparchus to express these divisions of the day, was adopted in its new sense by Greeks and Romans simultaneously and has ever since held its place in all the languages of Europe. In fact it was used in two senses; in its significance of the varying Roman hour it could not be employed to define exact intervals of time; when employed for that purpose it expressed exactly what we express by it now,—the twenty-fourth part of a civil day. The passage in Pliny I have quoted is not intelligible unless the word "hour" is employed in this sense.

Enough was said in the early part of this paper to show the line in which the clepsydra developed, the water-clock at Canton and that in the Tower of the Winds at Athens being examples of it in a fairly perfected state as a time-keeper. Invention had succeeded in giving to the rising pointer a regular motion, and adapting it well to its purpose. Other advances were made in it, and of these it remains to speak. Improvement, handicapped by the clumsy Roman hours, found in this fact a stimulus to ingenuity. To adapt it to indicate these hours one rude scheme was to reduce the capacity of the vessel from which the water flowed by coating it with wax in the winter time. The orifice remaining unchanged it emptied more quickly. The wax was gradually removed as the days lengthened. Of course the same instrument could not serve for both day and night. Less clumsy means for regulating the flow, as by adjusting the size of the orifice, were afterwards invented. One of these involved the passage of the water through a hollow cone or funnel, in which was an interior cone capable of adjustment for each day in the year; another, invented by Ctesibius, left the water-flow, and consequently the rise and fall of the float—constant, but included an automatic device by which the graduated scale over which the marker travelled was changed daily.

This difficulty in adapting the clepsydra to keep Roman time is precisely the same that the early Dutch navigators met with on their introduction of the clock into Japan, where the division of the day is into ten hours of varying length. The plan they adopted is a clumsy one, but of the same character as that of Ctesibius, since they did not attempt to alter the rate of the clock, but attached movable indications to the dial so that they might be changed with the season. One of these clocks is in the possession of the Bureau of Education, a gift from the Japanese Government after the Centennial Exposition of 1876.

But improvements in the clepsydra such as have been described, notwithstanding the ingenuity and mechanical skill they displayed, are of little consequence to us, since they were not towards the accomplishment of the final result but away from it. The actual steps towards the modern clock appear to be these: First, the employment of the ordinary rack and pinion device. If we are right in attributing the invention of gear-wheels to Archimedes, this application could not have been made earlier than the middle of the third century B. C. (287 to 212). It is attributed to Ctesibius, who, for many reasons as I have said already, is placed a century later than this. A series of teeth, commonly called a rack, was attached to the side of the rod, which was supported by the float, and had heretofore served only as an index. Fixed on a horizontal shaft above the vessel was a small toothed wheel, with which the toothed rack engaged, and which was, therefore, caused to turn by the rise of the float. On this shaft was a pointer attached like the hour-hand of a clock and travelling over a similar dial. To make this hand complete a circuit in twelve or twenty-four hours, is obviously only a question of the proportion of parts. The next step forward dispensed with the rack and pinion, and really was in the line of greater simplicity. In place of the toothed wheel a grooved pully was used, over which passed a cord from the float, being kept tight by a weight at the other end. The hand remained on the wheel shaft as before, and with the gradual rise of the float, traversed the dial.

We have reached the point where we may say "*presto, change*," and behold, a clock springs into view, for it is instantly apparent that with this structure it is no longer the water that advances the hand; water is not the motor now. The weight is the motor, and its fall is retarded by the float, which only permits its descent as fast as the rise of water in the vessel permits its own rise. We have an actual weight clock, with what we must be content to regard as a water escapement; it is far enough from our perfected time-piece, but in respect to its essential elements it differs in but one, and henceforth the problem of the clock is only that of escapements. But we need not expect it to be solved at once. It will be centuries before the actual problem will be recognized, so great is the obscurity with which the Roman time system has beclouded the subject.

There is a long and mournful perspective before us. The golden age of Roman literature is here, but she has yet to see the greatest extent of her empire and the summit of her own magnificence. A long line of Cæsars will come, base and noble alternating. Her decline will follow her glory; her palaces are to be plundered by barbarous northern invaders; her empire is to be shattered; out of her vast domain new peoples and nations and empires scarcely less mighty than her own are to spring, while she herself sinks to the paltry dimensions of a village. Her polished speech shall die from men's lips, but the rude dialects of her provinces, mingling with the uncouth tongues of illiterate Franks and Goths, shall develop into new languages, in time to become as perfect vehicles of thought as their original. New forms of government and of social order shall spring from her laws and institutions and philosophies; and from the hills of credulous and despised Judea is to burst a new religion, before whose bright beams the perpetual fires of Vesta shall pale and the whole train of Olympian gods vanish like the mist. But amongst these unconceived changes, and through the storms that shall sweep away—and the cataclysms that shall engulf—all the objects of her pride and glory and reverence, there shall still endure what she cared least for (constant in all their inconstancy), the Roman hours.

The problem of improving the time-keeper is one with which cloistered scholars and mechanicians will not cease to contend, but the barrier that Rome has set up will continue to baffle their ingenuity; and when thirteen centuries shall have passed since Hipparchus in vain urged the advantages of the equinoctial system and Ctesibius strove to solve the riddle of Roman time by some practical mechanism, we shall still find *Bernardo Monachus* recording how the monks of Cluny perplexed their pious souls with the old, old question, and how the good sacristan must needs to go out into the night to learn—from the stars—if it were time to call the brethren to prayer.

BOTANICAL BIOLOGY.*

By W. T. THISELTON-DYER, F. R. S.

It is not so very long ago, that at English universities, at least, the pursuit of botany was regarded rather as an elegant accomplishment than as a serious occupation. This is the more remarkable, because at every critical point in the history of botanical science, the names of our countrymen will be found to occupy an honorable place in the field of progress and discovery. In the seventeenth century, Hooke and Grew laid the foundation of the cell-theory, while Millington, by discovering the function of stamens, completed the theory of the flower. In the following century, Morison first raised ferns from spores, Lindsay detected the fern prothallus, Ray laid the foundations of a natural classification, Hales discovered root-pressure, and Priestley the absorption of carbon dioxide and the evolution of oxygen by plants. In the early part of the present one, we have Knight's discovery of the true cause of geotropism, Daubeny's of the effect upon the processes of plant-life of rays of light of different refrangibility, and finally, the first description of the cell-nucleus by R. Brown. I need not attempt to carry the list through the last half century. I have singled out these discoveries as striking landmarks, the starting-points of important developments of the subject. It is enough for my purpose to show that we have always had an important school of botany in England, which has contributed at least its share to the general development of the science.

I think at the moment however, we have little cause for anxiety. The academic chairs throughout the three kingdoms are filled for the most part with young, enthusiastic, and well-trained men. Botany is everywhere conceded its due position as the twin branch with zoology—of biological science. We owe to the enlightened administration of the Oxford University Press the possession of a journal which allows of the prompt and adequate publication of the results of laboratory research. The excellent work which is being done in every part of the botanical field has received the warm sympathy of our colleagues abroad. I need only recall to your recollection, as a striking evidence of this, the remarkable gathering of foreign botanists which will ever make the meeting of this association at Manchester a memorable event to all of us. The reflection rises sadly to the mind that it can never be repeated. Not many months, as you know, had passed before the two most prominent figures in that happy assemblage had been removed from us by the inexorable hand of death.

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* Presidential address before the Biological Section of the British Association, A. S., at Bath, September, 1888. (*Report of the British Association*, vol. LVIII, pp. 686–701).

In Asa Gray we miss a figure which we could never admit belonged wholly to the other side of the Atlantic. In technical botany we recognized him as altogether in harmony with the methods of work and standard of excellence of our own most distinguished taxonomists. But apart from this, he had the power of grasping large and far-reaching ideas, which I do not doubt would have brought him distinction in any branch of science. We owe to him the classical discussion of the facts of plant distribution in the northern hemisphere, which is one of the corner-stones of modern geographical botany. He was one of the earliest of distinguished naturalists who gave his adhesion to the theory of Mr. Darwin. A man of simple and sincere piety, the doctrine of descent never presented any difficulty to him. He will remain in our memories as a figure endowed with a sweetness and elevation of character which may be compared even with that of Mr. Darwin himself.

In De Bary we seem to have suffered no less a personal loss than in the case of Gray. Though, before last year, I do not know that he had ever been in England, so many of our botanists had worked under him that his influence was widely felt amongst us. And it may be said that this was almost equally so in every part of the civilized world. His position as a teacher was in this respect probably unique, and the traditions of his methods of work must permanently affect the progress of botany, and indeed have an even wider effect. This is not the occasion to dwell on each of his scientific achievements. It is sufficient to say that we owe to him the foundations of a rational vegetable pathology. He first grasped the true conditions of parasitism in plants, and not content with working out the complex phases of the life-history of the invading organism, he never lost sight of the conditions which permitted or inhibited its invasion. He treated the problem, whether on the side of the host or of the parisite, as a whole—as a biological problem in fact, in the widest sense. It is this thorough grasp of the conditions of the problem that gives such a peculiar value to his last published book, the "Lectures on Bacteria," an admirable translation of which we owe to Professor Balfour. To this I shall have again to refer. I must content myself with saying now, that in this and all his work there is that note of highest excellence which consists in lifting detail to the level of the widest generality. To a weak man this is a pitfall, in which a firm grasp of fact is lost in rash speculation. But when, as in De Bary's case, a true scientific insight is inspired by something akin to genius, the most fruitful conceptions are the result. Yet De Bary never sacrificed exactness to brilliancy, and to the inflexible love of truth which pervaded both his work and his personal intercourse we may trace the secret of the extraordinary influence which he exerted over his pupils.

As the head of one of the great national establishments of the country devoted to the cultivation of systematic botany, I need hardly apologize for devoting a few words to the present position of that branch of the science. Of its fundamental importance I have myself no manner of doubt. But as my judgment may seem in such a matter not wholly free from bias, I may fortify myself with an opinion which can hardly be minimized in that way. The distinguished chemist Prof. Lothar Meyer, perhaps the most brilliant worker in the field of theoretical chemistry, finds himself, like the systematic botanist, obliged to defend the position of descriptive science. And he draws his strongest argument from biology. "The physiology of plants and animals," he tells us, "requires systematic botany and zoology, together with the anatomy of the two kingdoms; each speculative science requires a rich and well-ordered material, if it is not to lose itself in empty and fruitless fantasies." No one of course supposes that the accumulation of plant specimens in herbaria is the mere outcome of a passion for accumulating. But to do good systematic work requires high qualities of exactitude, patience, and judgment. As I attempted to show on another occasion, the world is hardly sensible of the influence which the study of the subject has had on its affairs. The school of Jeremy Bentham has left an indelible mark on the social and legislative progress of our own time. Mills tells us that "the proper arrangement of a code of laws depends on the same scientific conditions as the classifications in natural history; nor could there," he adds, "be a better preparatory discipline for that important function than the principles of a natural arrangement, not only in the abstract, but in their actual application to the class of phenomena for which they were first elaborated, and which are still the best school for learning their use." He further tells us that of this, Jeremy Bentham was perfectly aware, and that his "Fragment on Government" contains clear and just views on the meaning of a natural arrangement which reflect directly the influence of Linnæus and Jussieu. Mill himself possessed a competent knowledge of systematic botany, and therefore was well able to judge of its intellectual value. For my part, I do not doubt that precisely the same qualifications of mind which made Jeremy Bentham a great jurist, enabled his nephew to attain the eminence he reached as a botanist. As a mere matter of mental gymnastic, taxonomic science will hold is own with any pursuit. And of course what I say of botany is no less true of other branches of natural history. Mr. Darwin devoted eight or nine years to the systematic study of the *Cirripedia*. "No one," he himself tells us, "has a right to examine the question of species who has not minutely described many." And Mr. Huxley has pointed out, in the admirable memoir of Mr. Darwin which he has prepared for the Royal Society, that "the acquirement of an intimate and practical knowledge of the process of species-making" - - - was "of no less importance to the author of the 'Origin of Species' than was the bearing of the Cirripede work upon the principles of a natural classification."

At present the outlook for systematic botany is somewhat discouraging. France, Germany, and Austria, no longer possess anything like a school on the subject, though they still supply able and distinguished workers. That these are however few, may be judged from the fact that it is difficult to fill the place of the lamented Eichler in the direction of the botanic garden and herbarium at Berlin. Outside our own country, Switzerland is the most important seat of general systematic study, to which three generations of De Candolles have devoted themselves. The most active centers of work at the moment are, however, to be found in our own country, in the United States, and in Russia. And the reason is in each case no doubt the same. The enormous area of the earth's surface over which each country holds sway brings to them a vast amount of material which peremptorily demands discussion.

No country however affords such admirable facilities for work in sytematic botany as are now to be found in London. The Linnean Society possesses the herbarium of Linnæus; the Botanical Department of the British Museum is rich in the collections of the older botanists; while at Kew we have a constantly-increasing assemblage of material, either the results of travel and expeditions, or the contributions of correspondents in different parts of the Empire. A very large proportion of this has been worked up. But I am painfully impressed with the fact that the total of our available workers bears but a small proportion to the labor ready to their hands.

This is the more a matter of concern, because for the few official posts which are open to botanists at home or abroad, a practical knowledge of systematic botany is really indispensable. For suitable candidates for these, one naturally looks to the universities. And so far, I am sorry to say, in great measure one looks in vain. It would be no doubt a great impulse to what is undoubtedly an important branch of national scientific work if fellowships could occasionally be given to men who showed some aptitude for it. But these should not be mere prizes for under-graduate study, but should exact some guaranty that during the tenure of the fellowship the holder would seriously devote himself to some definite piece of work. At present, undoubtedly, the younger generation of botanists show a disposition to turn aside to those fields in which more brilliant and more immediate results can be attained. Their neglect of systematic botany brings to some extent its own Nemesis. A first principle of systematic botany is that a name should denote a definite and ascertainable species of plant. But in physiological literature you will find that the importance of this is often overlooked. Names are employed which are either not to be found in the books, or they are altogether mis-applied. But if proper precautions are taken to ascertain the accurate botanical name of a plant, no botanist throughout the civilized world is at a loss to identify it.

But precision in nomenclature is only the necessary apparatus of the subject. The data of systematic botany, when properly discussed, lend themselves to very important generalizations. Perhaps those which are yielded by the study of geographical distribution are of the most general interest. The mantle of vegetation which covers the surface of the earth, if only we could rightly unravel its texture, would tell us a good deal about geological history. The study of geographical distribution, properly handled, affords an independent line of attack upon the problem of the past distribution of land and sea. It would probably never afford sufficient data for a complete independent solution of the problem; but it must always be extremely useful as a check upon other methods. Here however we are embarrassed by the enormous amount of work which has yet to be accomplished. And unfortunately this is not of a kind which can be indefinitely postponed. The old terrestrial order is fast passing away before our eyes. Everywhere the primitive vegetation is disappearing as more and more of the earth's surface is brought into cultivation, or at any rate denuded of its forests.

A good deal, however, has been done. We owe to the indomitable industry of Mr. Bentham and of Sir Ferdinand Mueller a comprehensive flora of Australia, the first large area of the earth's surface of which the vegetation has been completely worked out. Sir Joseph Hooker, in his retirement, has pushed on within sight of completion the enormous work of describing so much of the vast Indo-Malayan flora as is comprised within the British possessions. To the Dutch botanists we owe a tolerably complete account of the Malayan flora proper. But New Guinea still remains botanically a *terra incognita*, and till within the last year or two the flora of China has been an absolute blank to us. A committee of the British Association) has, with the aid of a small grant of money, taken in hand the task of gathering up the scanty data which are available in herbaria and elsewhere. This has stimulated European residents in China to collect more material, and the fine collections which are now being rapidly poured in upon us, will—if they do not overwhelm us by their very magnitude—go a long way in supplying data for a tentative discussion of the relations of the Chinese flora to that of the rest of Asia. I do not doubt that this will in turn explain a good deal that is anomalous in the distribution of plants in India. The work of the committee has been practically limited to central and eastern China. From the west, in Yunnan, the French botanists have received even more surprising collections, and these supplement our own work in the most fortunate manner. I have only to add, for Asia, Boissier's "Flora Orientalis," which practically includes the Mediterranean basin. But I must not omit the invaluable report of Brigade-Surgeon Aitchison on the collections made by him during the Afghan delimitation expedition. This has given an important insight into the vegetation of a region which had never previously been adequately examined. Nor must I forget the recent publication of the masterly report by Prof. Bayley Balfour on the plants collected by himself and Schweinfurth in Socotra, an island with which the ancient Egyptians traded, but the singularly anomalous flora of which was almost wholly unknown up to our time.

The flora of Africa has been at present but imperfectly worked up, but the materials have been so far discussed as to afford a tolerably correct theory of its relations. The harvest from Mr. Johnston's expedition to Kilimanjaro was not as rich as might have been hoped. Still, it was sufficient to confirm the conclusions at which Sir Joseph Hooker had arrived, on very slender data, as to the relations of the high-level vegetation of Africa generally. The flora of Madagascar is perhaps, at the moment, the most interesting problem which Africa presents to the botanist. As the rich collections, for which we are indebted to Mr. Baron and others, are gradually worked out, it can hardly be doubted that it will be necessary to modify in some respects the views which are generally received as to the relation of the island to the African continent. My colleague, Mr. Baker, communicated to the York meeting of the association the results which, up to that time, he had arrived at, and these, subsequent material has not led him to modify. The flora as a whole presents a large proportion of endemic genera and species, pointing to isolation from a very ancient date. The tropical element is however closely allied to that of tropical Africa and of the Mascarene Islands, and there is a small infusion of Asiatic types which do not extend to Africa. The high-level flora, on the other hand, exhibits an even closer affinity with that temperate flora, the ruins of which are scattered over the mountainous regions of Central Africa, and which survives in its greatest concentration at the Cape.

The American botanists at Harvard are still systematically carrying on the work of Torrey and Gray in the elaboration of the flora of Northern America. The Russians are, on their part, continually adding to our knowledge of the flora of Northern and Central Asia. The whole flora of the north temperate zone can only be regarded substantially as one. The identity diminishes southwards, and increases in the case of the Arctic and Alpine regions. A collection of plants brought us from high levels in Corea, by Mr. James, might (as regards a large proportion of the species) have been gathered on one of our own Scotch hills.

We owe to the munificence of two Englishmen of science the organization of an extensive examination of the flora and fauna of Central America and the publication of the results. The work when completed can hardly be less expensive than that of the results of the *Challenger* voyage, which has severely taxed the liberality of the English Government. The problems which geographical distribution in this region presents will doubtless be found to be of a singularly complicated nature, and it is impossible to overestimate the debt of gratitude which biologists of all countries must owe to Messrs. Godman and Salvin when their arduous undertaking is completed. I am happy to say that the botanical portion, which has been elaborated at Kew, is all but finished.

In South America, I must content myself with referring to the great "Flora Brasiliensis," commenced by Martius half a century ago, and still slowly progressing under the editorship of Professor Urban, at Berlin. Little discussion has yet been attempted of the mass of material which is enshrined in the mighty array of volumes already published. But the travels of Mr. Ball in South America have led him to the detection of some very interesting problems. The enormous pluvial denudation of the ancient portions of the continent has led to the gradual blending of the flora of different levels with sufficient slowness to permit of adaptive changes in the process. The tropical flora of Brazil therefore presents an admixture of modified temperate types which gives to the whole a peculiar character not met with to the same degree in the tropics of the Old World. On the other hand, the comparatively recent elevation of the southern portion of the continent accounts in Mr. Ball's eyes for the singular poverty of its flora, which we may regard indeed as still in progress of development.

The botany of the *Challenger* expedition, which was also elaborated at Kew, brought for the first time into one view all the available facts as to the floras of the older oceanic islands. To this was added a discussion of the origin of the more recent floras of the islands of the Western Pacific, based upon material carefully collected by Professor Moseley, and supplemented by the notes and specimens accumulated with much judgment by Dr. Guppy. For the first time we were enabled to get some idea how a tropical island was furnished with plants, and to discriminate the littoral element due to the action of oceanic currents from the interior forest almost wholly due to frugivorous birds. The recent examination of Christmas Island by the English Admiralty has shown the process of island flora-making in another stage. The plants collected by Mr. Lister prove, as might be expected, to be closely allied to those of Java. But the effect of isolation has begun to tell; and I learn from my colleague, Professor Oliver, that the plants from Christmas Island can not be for the most part exactly matched with their congeners from Java, but yet do not differ sufficiently to be specifically distinguished. We have here therefore it appears to me, a manifest case of nascent species.

The central problem of systematic botany I have not as yet touched upon: this is to perfect a natural classification. Such a classification, to be perfect, must be the ultimate generalization of every scrap of knowledge which we can bring to bear upon the study of plant affinity. In the higher plants, experience has shown that we can obtain results which are sufficiently accurate for the present, without carrying our structural analysis very far. Yet even here, the correct relations of the Gymnosperms would never have been ascertained without patient and minute microscopic study of the reproductive processes. Upon these, indeed, the correct classification of the Vascular Cryptogams wholly depends, and generally, as we descend in the scale, external morphology becomes more and more insecure as a guide, and a thorough knowledge of the minute structure and life history of each organism becomes indispensable to anything like a correct determination of its taxonomic position. The marvellous theory of the true nature of lichens would never have been ascertained by the ordinary methods of examination which were held to be sufficient by lichenologists.

The final form of every natural classification—for I have no doubt that the general principles I have laid down are equally true in the field of zoology—must be to approximate to the order of descent. For the theory of descent became an irresistible induction as soon as the idea of a natural classification had been firmly grasped.

In regard to flowering plants we owe, as I have said, the first step in a natural classification to our own great naturalist, John Ray, who divided them into Monocotyledons and Dicotyledons. The celebrated classification of Linnæus was avowedly purely artificial. It was a temporary expedient, the provisional character of which no one realized more thoroughly than himself. He in fact himself gave us one of the earliest outlines of a truly natural system. Such a system is based on affinity, and we know of no other explanation of affinity than that which is implied in the word,—namely, common parentage. No one finds any difficulty in admitting that where a number of individual organisms closely resemble one another, they must have been derived from the same stock. I allow that in cases where external form is widely different, the conclusion to one who is not a naturalist is by no means so obvious. But in such cases it rests on the profound and constant resemblance of internal points of structure. Anyone who studies the matter with a perfectly open mind finds it impossible to draw a line. If genetic relationship or heredity is admitted to be the explanation of affinity in the most obvious case, the stages are imperceptible when the evidence is fairly examined, by which the same conclusion is seen to be inevitable, even in cases where at the first glance it seems least likely.

This leads me to touch on the great theory which we owe to Mr. Darwin. That theory, I need hardly say, was not merely a theory of descent. This had suggested itself to naturalists in the way I have indicated,—long before. What Mr. Darwin did was to show how by perfectly natural causes the separation of living organisms into races which at once resemble and yet differ from one another so profoundly, came about. Heredity explains the resemblance; Mr. Darwin's great discovery was that variation worked upon by natural selection explained the difference. That explanation seems to me to gather strength every day, and to continually reveal itself as a more and more efficient solvent of the problems which present themselves to the student of natural history. At the same time, I am far from claiming for it the authority of a scientific creed or even the degree of certainty which is possessed by some of the laws of astronomy. I only affirm that as a theory it has proved itself a potent and invaluable instrument of research. It is an immensely valuable induction; but it has not yet reached such a position of certitude as has been attained by the law of gravitation; and I have myself, in the field of botany, felt bound to protest against conclusions being drawn deductively from it without being subjected to the test of experimental verification. This attitude of mine, which I believe I share with most naturalists, must not however be mistaken for one of doubt. Of doubt as to the validity of Mr. Darwin's views I have none: I shall continue to have none till I come across facts which suggest doubt. But that is a different position from one of absolute certitude.

It is therefore without any dissatisfaction that I observe that many competent persons have—while accepting Mr. Darwin's theory—set themselves to criticize various parts of it. But I must confess that I am disposed to share the opinion expressed by Mr. Huxley, that these criticisms really rest on a want of a thorough comprehension.

Mr. Romanes has put forward a view which deserves the attention due to the speculations of a man of singular subtlety and dialectic skill. He has startled us with the paradox that Mr. Darwin did not after all, put forth—as I conceive it was his own impression he did—a theory of the origin of species, but only of adaptations. And inasmuch as Mr. Romanes is of opinion that specific differenees are not even generally adaptive, while those of genera are, it follows that Mr. Darwin only really accounted for the origin of the latter, while for an explanation of the former we must look to Mr. Romanes himself. For my part however, I am altogether unable to accept the premises, and therefore fail to reach the conclusion. Specific differences, as we find them in plants, are for the most part indubitably adaptive, while the distinctive characters of genera and of higher groups are rarely so. Let anyone take the numerous species of some well characterized English genus—for example, *Ranunculus;* he will find that one species is distinguished by having creeping stems, one by a tuberous root, one by floating leaves, another by drawn-out submerged ones, and so on. But each possesses those common characters which enable the botanist almost at a glance, notwithstanding the adaptive disguise, to refer them to the common genus *Ranunculus*. It seems to me quite easy to see, in fact, why specific characters should be usually adaptive, and generic not so. Species of any large genus must, from the nature of things, find themselves exposed to any but uniform conditions. They must acquire therefore as the very condition of their existence, those adaptive characters which the necessities of their life demand. But this rarely affects those marks of affinity which still indicate their original common origin. Probably these were themselves once adaptive, but they have long been overlaid at Kew, owe to the kindness of Dr. Schweinfurth, a collection of specimens of plants from Egyptian tombs, which are said to be as much as four thousand years old. They are still perfectly identifiable, and as one of my predecessors in this chair has pointed out, they differ in no respect from their living representatives in Egypt at this day. The explanation which Lamarck gave of this fact "may well," says Sir Charles Lyell, "lay claim to our admiration." He attributed it, in effect, to the persistence of the physical geography, temperature, and other natural conditions. The explanation seems to me adequate. The plants and animals, we may fairly assume, were four thousand years ago, as accurately adjusted to the conditions in which they then existed, as the fact of their persistence in the country shows that they must be now. Any deviation from the type that existed then would either therefore be disadvantageous or indifferent. In the former case it would be speedily eliminated, in the latter it would be swamped by cross-breeding. But we know that if seeds of these plants were introduced into our gardens we should soon detect varieties amongst their progeny. Long observation upon plants under cultivation has always disposed me to think that a change of external conditions actually stimulated variation, and so gave natural selection wider play and a better chance of re-establishing the adaptation of the organism to them. Weismann explains the remarkable fact that organisms may for thousands of years reproduce themselves unchanged by the principle of the persistence of the germ-plasm. Yet it seems hard to believe that the germ-plasm, while enshrined in the individual whose race it is to perpetuate, and nourished at its expense, can be wholly indifferent to all its fortunes. It may be so, but in that case it would be very unlike other living elements of organized beings.

I am bound however to confess that I am not wholly satisfied with the data for the discussion of this question which practical horticulture supplies. That the contents of our gardens do exhibit the results of variation in a most astonishing degree no one will dispute. But for scientific purposes, any exact account of the treatment under which these variations have occurred is unfortunately usually wanting. A great deal of the most striking variation is undoubtedly due to wide crossing, and these cases must of course be eliminated when the object is to test the independent variation of the germ-plasm. Hoffmann, whose experiments I have already referred to, doubts whether plants do as a matter of fact vary more under cultivation than in their native home and under natural conditions. It would be very interesting if this could be tested by the concerted efforts of two cultivators, say, for example, in Egypt and in England. Let some annual plant be selected, native of the former country, and let its seed be transmitted to the latter. Then let each cultivator select any variations that arise in regard to some given character; set to work, in fact, exactly as any gardener would who wanted to "improve" the plant, but on a preconcerted plan.

A comparison of the success which each obtained would be a measure of the effect of the change of the environment on variability. If it proved that as Hoffmann supposed, the change of conditions did not affect the what we may call the rate of variation, then, as Mr. Darwin remarks in writing to Professor Semper, "the astonishing variations of almost all cultivated plants must be due to selection and breeding from the varying individuals. This idea," he continues, "crossed my mind many years ago, but I was afraid to publish it, as I thought that people would say, 'How he does exaggerate the importance of selection.'" From an independent consideration of the subject I also find my mind somewhat shaken about it. Yet I feel disposed to say with Mr. Darwin, "I still *must* believe that changed conditions give the impulse to variability, but that they act in *most cases* in a very different manner."

Whatever conclusions we arrive at on these points, every one will agree that one result of the Darwinian theory has been to give a great impulse to the study of organisms, if I may say so, as "going concerns." Interesting as are the problems which the structure, the functions, the affinity, or the geographical distribution of a plant may afford, the living plant in itself is even more interesting still.

Every organ will bear interrogation to trace the meaning and origin of its form and the part it plays in the plant's economy. That there is here an immense field for investigation there can be no doubt. Mr. Darwin himself set us the example in a series of masterly investigations. But the field is well-nigh inexhaustible. The extraordinary variety of form which plants exhibit has led to the notion that much of it may have arisen from indifferent variation. No doubt, as Mr. Darwin has pointed out, when one of a group of structures held together by some morphological or physiological *nexus* varies, the rest will vary correlatively. One variation then may, if advantageous, become adaptive, while the rest will be indifferent. But it appears to me that such a principle should be applied with the greatest caution; and from what I have myself heard fall from Mr. Darwin, I am led to believe that in the later years of his life he was disposed to think that every detail of plant structure had some adaptive significance, if only the clue could be found to it. As regards the forms of flowers, an enormous body of information has been collected, but the vegetative organs have not yielded their secret to anything like the same extent. My own impression is that they will be found to be adaptive in innumerable ways which at present are not even suspected. At Kew we have probably a larger number of species assembled together than are to be found anywhere on the earth's surface. Here then is ample material for observation and comparison. But the adaptive significance will doubtless often be found by no means to lie on the surface. Who, for example, could possibly have guessed by inspection the purpose of the glandular bodies on the leaves of *Acacia sphærocephala* and on the pulvinus of *Cecropia peltata* which Belt in the one case and Fritz Müller in the other have shown to serve as food for ants? So far from this explanation being far-fetched, Belt found that the former "tree is actually unable to exist without its guard," which it could not secure without some attraction in the shape of food. One fact which strongly impresses me with a belief in the adaptive significance of vegetative characters is the fact that in almost identical forms they are constantly adopted by plants of widely different affinity. If such forms were without significance one would expect them to be infinitely varied. If however they are really adaptive, it is intelligible that different plants should independently avail themselves of identical appliances and expedients.

Although this country is splendidly equipped with appliances for the study of systematic botany, our universities and colleges fall far behind a standard which would be considered even tolerable on the Continent, in the means of studying morphological and physiological botany, or of making researches in these subjects. There is not at the moment anywhere in London an adequate botanical laboratory, and though at most of the universities, matters are not quite so bad, still I am not aware of any one where it is possible to do more than give the routine instruction, or to allow the students, when they have passed through this, to work for themselves. It is not easy to see why this should be, because on the animal side the accommodation and appliances for teaching comparative anatomy and physiology are always adequate and often palatial. Still less explicable to me is the tendency on the part of those who have charge of medical education to eliminate botanical study from the medical curriculum, since historically the animal histologists owe everything to botanists. In the seventeenth century, as I have already mentioned, Hooke first brought the microscope to the investigation of organic structure, and the tissue he examined was cork. Somewhat later, Grew, in his "Anatomy of Plants," gave the first germ of the cell-theory. During the eighteenth century the anatomists were not merely on a hopelessly wrong tack themselves, but they were bent on dragging botanists into it also. It was not until 1837, a little more than fifty years ago, that Henle saw that the structure of epithelium was practically the same as that of the *parenchyma plantarum* which Grew had described one hundred and fifty years before. Two years later Schwann published his immortal theory, which comprised the ultimate facts of plants and animal anatomy under one view. But it was to a botanist, Von Mohl, that in 1846, the biological world owed the first clear description of protoplasm, and to another botanist, Cohn (1851), the identification of this with the sarcode of zoologists.

Now the historic order in discovery is not without its significance. The path which the first investigators found most accessible is doubtless that which beginners will also find easiest to tread. I do not myself believe that any better access can be obtained to the structure and functions of living tissues than by the study of plants. However, I am not without hopes that the serious study of botany in the laboratory will be in time better cared for. I do not hesitate to claim for it a position of the greatest importance in ordinary scientific education. All the essential phenomena of living organisms can be readily demonstrated upon plants. The necessary appliances are not so costly, and the work of the class room is free from many difficulties with which the student of the animal side of biology has to contend.

Those however who have seriously devoted themselves to the pursuit of either morphological or physiological botany need not now be wholly at a loss. The splendid laboratory on Plymouth Sound, the erection of which we owe to the energy and enthusiasm of Prof. Ray Lankester, is open to botanists as well as to zoologists, and affords every opportunity for the investigation of marine plants, in which little of late years has been done in this country. At Kew we owe to private munificence a commodious laboratory in which much excellent work has already been done. And this association has made a small grant in aid of the establishment of a laboratory in the Royal Botanic Garden at Peradeniya, in Ceylon. It may be hoped that this will afford facilities for work of the same kind as has yielded Dr. Treub such a rich harvest of results in the Buitenzorg Botanic Garden in Java.

Physiological botany, as I have already pointed out, is a field in which this country in the past has accomplished great things. It has not of late however obtained an amount of attention in any way proportionate to that devoted to animal physiology. In the interests of physiological science generally, this is much to be deplored; and I believe that no one was more firmly convinced of this than Mr. Darwin. Only a short time before his death, in writing to Mr. Romanes on a book that he had recently been reading, he said that the author had made " a gigantic oversight in never considering plants; these would simplify the problem for him." This goes to the root of the matter. There is, in my judgment, no fundamental biological problem which is not exhibited in a simpler form by plants than by animals. It is possible, however, that the distaste which seems to exist amongst our biologists for physiological botany, may be due in some measure to the extremely physical point of view from which it has been customary to treat it on the Continent. It is owing in great measure to the method of Mr. Darwin's own admirable researches that in this country we have been led to a more excellent way. The work which has been lately done in England seems to me full of the highest promise. Mr. Francis Darwin and Mr. Gardiner have each in different directions shown the entirely new point of view which may be obtained by treating plant phenomena as the outcome of the functional activity of protoplasm. I have not the least doubt that by pursuing this path English research will not merely place vegetable physiology, which has hitherto been too much under the influence of Lamarckism, on a more rational basis, but that it will also sensibly re-act, as it has done often before, on animal physiology.

There is no part of the field of physiological botany which has yielded results of more interest and importance than that which relates to the action of ferments and fermentation; and I could hardly give you a better illustration of the purely biological method of treating it. I believe that these results, wonderful and fascinating as they are, afford but a faint indication of the range of those that are still to be accomplished. The subject is one of extreme intricacy, and it is not easy to speak about it briefly. To begin with, it embodies two distinct groups of phenomena, which have in reality very little which is essential in common.

What are usually called ferments are perhaps the most remarkable of all chemical bodies, for they have the power of effecting very profound changes in the chemical constitution of other substances, although they may be present in very minute quantity; but—and this is their most singular and characteristic property—they themselves remain unchanged in the process. It may be said without hesitation that the whole nutrition of both animals and plants depends on the action of ferments. Organisms are incapable of using solid nutrient matter for the repair and extension of their tissues; this must first be brought into soluble form before it can be made available, and this change is generally brought about by the action of a ferment. Animal physiology has long been familiar with the part played by ferments, and it may be said that no small part of the animal economy is made up of organs required either for the manufacture of ferments or for the exposure of ingested food to their action. It may seem strange at first sight to speak of analogous processes taking place in plants. But it must be remembered that plant nutrition includes two very distinct stages. Certain parts of plants build up, as everyone knows, from external inorganic materials, substances which are available for the construction of new tissues. It might be supposed that these are used up as fast as they are formed. But it is not so; the life of the plant is not a continuous balance of income and expenditure. On the contrary, besides the general maintenance of its structure, the plant has to provide from time to time for enormous resources to meet such exhausting demands as the renewal of foliage, the production of flowers, and the subsequent maturing of fruit.

In such cases the plant has to draw on accumulated store of solid food which has rapidly to be converted into the soluble form in which alone it is capable of passing through the tissues to the seat of consumption. And I do not doubt for my part that in such cases ferments are brought into play of the same kind and in the same way as in the animal economy. Take such a simple case as a potato-tuber. This is a mass of cellular tissue, the cells of which are loaded with starch. We may either dig up the tuber and eat the starch ourselves, or we may leave it in the ground, in which case it will be consumed in providing material for the growth of a potato-plant for next year. But the processes by which the insoluble starch is made available for nutrition are, I can not doubt, closely similar in either case.

When we inquire further about these mysterious and all-important bodies, the answer we can give is extremely inadequate. It is very difficult to obtain them in amount sufficient for analysis, or in a state of purity. We know however that they are closely allied to albuminoids, and contain nitrogen in varying proportion. Papain, which is a vegetable ferment derived from the fruit of the papaw, and capable of digesting most animal albuminoids, is said to have the same ultimate composition as the pancreatic ferment and as peptones, bodies closely allied to proteids; the properties of all three bodies are however very different. It seems clear nevertheless that ferments must be closely allied to proteids, and like these bodies, they are no doubt directly derived from protoplasm.

I need not remind you that, unlike other constituents of plant tissues, protoplasm, as a condition of its vitality, is in a constant state of molecular activity. The maintenance of this activity involves the supply of energy, and this is partly derived from the waste of its own substance. This "self-decomposition" of the protoplasm liberates energy, and in doing so gives rise to a number of more stable bodies than protoplasm. Some of these are used up again in nutrition; others are thrown aside, and are never drawn again into the inner circle of vital processes. In the animal organism, where the strictest economy of bulk is a paramount necessity, they are promptly got rid of by the process of excretion. In the vegetable economy these residual products usually remain. And it is for this reason, I may point out, that the study of the chemistry of plant nutrition appears to me of such immense importance. The record of chemical change is so much more carefully preserved; and the probability of our being able to trace the course it has followed is consequently far more likely to be attended with success.

This preservation in the plant of the residual by-products of protoplasmic activity no doubt accounts for the circumstance which otherwise is extremely perplexing,—the profusion of substances which we meet with in the vegetable kingdom to which it is hard to attribute any useful purpose. It seems probable that ferments, in a great many cases, belong to the same category. I imagine that it is in some degree accidental that some of them have been made use of, and thus the plant has been able to temporarily lock up accumulaitons of food to be drawn upon in future phases of its life with the certainty that they would be available. Without the ferments, the key of the storehouse would be lost irretrievably.

Plants moreover are now known to possess ferments, and the number will doubtless increase, to which it is difficult to attribute any useful function. Papain, to which I have already alluded, abounds in the papaw, but it is not easy to assign to it any definite function; still less is it easy, on teleological grounds, to account for the rennet ferment contained in the fruits of an Indian plant, *Withania coagulans*, and admirably investigated by Mr. Sheridan Lea.

Having dwelt so far on the action of ferments, we may now turn to fermentation, and that other kind of change in organic matter called "putrefaction," which is known to be closely allied to fermentation. Ferments and fermentation, as I have already remarked have very little to do with one another; and it would save confusion and emphasize the the fact if we ceased to speak of ferments but used some of the alternative names which have been proposed for them, such as *zymases* or *enzymes*.

The classical case of fermentation, which is the root of our whole knowledge of the subject, is that of the conversion of sugar into alcohol. Its discovery has everywhere accompanied the first stages of civilization in the human race. Its details are now taught in our text books; and I should hardly hope to be excused for referring to it in any detail if it were not necessary for my purpose to draw your attention more particularly to one or two points connected with it.

Let us trace what happens in a fermenting liquid. It becomes turbid, it froths and effervesces, the temperature sensibly increases; this is the first stage. After this it begins to clear, the turbidity subsides as a sediment; the sugar which the fluid at first contained, has in great part disappeared, and a new ingredient, alcohol, is found in its place.

It is just fifty years ago that the great Dutch biologist Schwann made a series of investigations which incontrovertibly demonstrated that both fermentation and putrefaction were due to the presence of minute organisms which live and propagate at the expense of the liquids in which they produce as a result these extraordinary changes. The labors of Pasteur have confirmed Schwann's results, and—what could not have been foreseen—have extended the possibilities of this field of investigation to those disturbances in the vital phenomena of living organisms themselves which we include under the name of "disease," and which, no one will dispute, are matters of the deepest concern to every one of us.

Now, at first sight, the conversion of starch into sugar by means of diastase seems strikingly analogous to the conversion of sugar into alcohol. It is for this reason that the phenomena have been so long associated. But it is easy to show that they are strikingly different. Diastase is a chemical substance of obscure composition it is true, but inert and destitute of any vital properties, nor is it affected by the changes it induces. Yeast, on the other hand, which is the active agent in alcoholic fermentation, is a definite organism; it enormously increases during the process, and it appears to me impossible to resist the conclusion that fermentation is a necessary concomitant of the peculiar conditions of its life. Let me give you a few facts which go to prove this. In the first place, you can not ferment a perfectly pure solution of sugar. The fermentable fluid must contain saline and nitrogenous matters necessary for the nutrition of the yeast protoplasm. In pure sugar the yeast starves. Next, Schwann found that known protoplasmic poisons, by killing the yeast-cells, would prohibit fermentation. He found the same result to hold good of putrefaction, and this is the basis of the whole theory of antiseptics. Nor can the action of yeast be attributed to any ferment which the yeast secretes. It is true that pure cane-sugar can not be fermented, and that yeast effects the inversion of this, as it is called, into glucose and lævulose. It does this by a ferment which can be extracted from it, and which is often present in plants. But you can extract nothing from yeast which will do its pecular work apart from itself. Helmholtz made the crucial experiment of suspending a bladder full of boiled grape-juice in a vat of fermenting must; it underwent no change; and even a film of blotting-paper has been found a sufficient obstacle to its action. We are driven then necessarily to the conclusion that in the action of "ferments" or zymases we have to do with a chemical—*i. e.*, a purely physical process; while in the case of yeast we encounter a purely physiological one.

How then is this action to be explained? Pasteur has laid stress on a fact which had some time been known, that the production of alcohol from sugar is a result of which yeast has not the monopoly. If ripening fruits—such as plums—are kept in an atmosphere free from oxygen, Bérard found that they too exhibit this remarkable transformation; their sugar is converted appreciably into alcohol. On the other hand, Pasteur has shown that, if yeast is abundantly supplied with oxygen, it feeds on the sugar of a fermentable fluid without producing alcohol. But under the ordinary circumstance of fermentation, its access to oxygen is practically cut off; the yeast then is in exactly the same predicament as the fruit in Bérard's experiment. Sugar is broken up into carbon dioxide and alcohol in an amount far in excess of the needs of mere nutrition. In this dissociation it can be shown that an amount of energy is set free in the form of heat equal to about one-tenth of what would be produced by the total combustion of an equivalent of grape-sugar. If the protoplasm of the yeast could, with the aid of atmospheric oxygen, completely decompose a unit of grape-sugar, it would get ten times as much energy in the shape of heat as it could get by breaking it up into alcohol and carbon dioxide. It follows then that to do the same amount of growth in either case, it must break up ten times as much sugar without a supply of oxygen as with it. And this throws light on what has always been one of the most remarkable facts about fermentation—the enormous amount of change which the yeast manages to effect in proportion to its own development.

There are still two points about yeast which deserve attention before we dismiss it. When a fermenting liquid comes to contain about 14 per cent. of alcohol, the activity of the yeast ceases, quite independently of whether the sugar is used up or not. In other cases of fermentation the same inhibiting effect of the products of fermentation is met with.

Thus lactic fermentation soon comes to an end unless calcium carbonate or some similar substance be added, which removes the lactic acid from the solution as fast as it is formed.

The other point is that in all fermentations, besides what may be termed the primary products of the process, other bodies are produced. In the case of alcoholic fermentation the primary bodies are alcohol and carbon dioxide; the secondary, succinic acid and glycerine. Delpino has suggested that these last are residual products derived from that portion of the fermentable matter which is directly applied to the nutrition of the protoplasm.

Yeast, itself the organism which effects the remarkable changes on which I have dwelt, is somewhat of a problem. It is clear that it is a fungus, the germs of which must be ubiquitous in the atmosphere. It is difficult to believe that the simple facts, which are all we know about it, constitute its entire life-history. It is probably a transitory stage of some more complicated organism.

I can only briefly refer to putrefaction. This is a far more complex process than that which I have traced in the case of alcoholic fermentation. In that, nitrogen is absent, while it is an essential ingredient in albuminoids, which are the substances that undergo putrefactive changes. But the general principles are the same. Here, too, we owe to Schwann the demonstration of the fact that the effective agents in the process are living organisms. If we put into a flask a putrescible liquid such as broth, boil it for some time, and during the process of boiling plug the mouth with some cotton-wool, we know that the broth will remain long unchanged, while if we remove the wool putrescence soon begins. Tyndall has shown that, if we conduct the experiment on one of the high glaciers of the Alps, the cotton-wool may be dispensed with. We may infer then that the germs of the organisms which produce putrefaction are abundant in the lower levels of the atmosphere and are absent from the higher. They are wafted about by currents of air; but they are not imponderable, and in still air they gradually subside. Dr. Lodge has shown that air is rapidly cleared of suspended dust by an electric discharge, and this no doubt affords a simple explanation of the popular belief that thunderous weather is favorable to putrefactive changes.

Cohn believes that putrefaction is due to an organism called *Bacterium termo*, which plays in it the same part that yeast does in fermentation. This is probably too simple a statement; but the general phenomena are nevertheless similar. There is the same breaking down of complex into simpler molecules; the same evolution of gas, especially carbon dioxide; the same rise of temperature. The more or less stable products of the process are infinitely more varied, and it is difficult, if not impossible, to say, in the present state of our knowledge, whether in most cases they are the direct outcome of the putrefactive process, or residual products of the protoplasmic activity of the organisms which induce it. Perhaps, on the analogy of the higher plants, in which some of them also occur, we may attribute to the latter category certain bodies closely resembling vegetable alkaloids; these are called ptomaines, and are extremely poisonous. Besides such bodies, Bacteria undoubtedly generate true ferments and peculiar coloring-matters. But there are in most cases of putrefaction a profusion of other substances, which represent the various stages of the breaking up of the complex proteid molecule, and are often themselves the outcome of subsidiary fermentations.

These results are of great interest from a scientific point of view. But their importance at the present moment in the study of certain kinds of disease can hardly be exaggerated. I have already mentioned Henle as having first found the true clue to animal histology in the structure of plants. As early as 1840, the same observer indicated the grounds for regarding contagious diseases as due to living organisms. I will state his argument in the words of De Bary, whose "Lectures on Bacteria," the last work which we owe to his gifted hand, I can confidently recommend to you as a luminous but critical discussion of a vast mass of difficult and conflicting literature.

It was of course clear that contagion must be due to the communication of infectious particles or contagia. These contagia, although at the time no one had seen them, Henle pointed out, "have the power, possessed, as far as we know, by living creatures only, of growing under favorable conditions, and of multiplying at the expense of some other substance than their own, and therefore of assimilating that substance." Henle enforced his view by comparison with the theory of fermentation, which had then been promulgated by Schwann. But for many years his views found no favor. Botanists however as in so many other cases, struck on the right path, and from about the year 1850 steady progress, in which De Bary himself took a leading part, was made in showing that most of the diseases of plants are due to parasitic infection. The reason of this success was obvious; the structure of plants makes them more accessible to research, and the invading parasites are larger than animal contagia. On the animal side all real progress dates from about 1860, when Pasteur, having established Schwann's theory of fermentation on an impregnable basis, took up Henle's theory of living contagia.

The only risk now is that we may get on too fast. To put the true theory of any one contagious disease on as firm a basis as that of alcoholic fermentation is no easy matter to accomplish. But I believe that this is (notwithstanding a flood of facile speculation and imperfect research) slowly being done.

There are two tracts in the body which are obviously accessible to such minute organisms as Bacteria, and favorable for their development. These are the alimentary canal and the blood. In the case of the former there is evidence that every one of us possesses quite a little flora of varid forms and species. They seem for the most part, in health, to be comparatively innocuous; indeed it is believed that they are ancillary to and aid digestion. But it is easy to see that other kinds may be introduced, or those already present may be called into abnormal activity, and fermentative processes may be set up of a very inconvenient kind. These may result in mere digestive disorder, or in the production of some of those poisonous derivatives of proteids of which I have spoken, the effect of which upon the organism may be most disastrous.

The access of Bacteria to the blood is a far more serious matter. They produce phenomena the obvious analogy of which to fermentative processes has led to the resulting diseases being called zymotic. Take for example, the disease known as "relapsing fever." This is contagious. After a period of incubation, violent fever sets in, which lasts for something less than a week, is then followed by a period of absence, to be again followed in succession by one or more similar attacks, which ultimately cease. Now you will observe that the analogy to a fermentative process is very close. The period of incubation is the necessary interval between the introduction of the germ and its vegetative multiplication in sufficient numbers to appreciably affect the total volume of the blood. The rise in temperature and the limited duration of the attack are equally, as we have seen, characteristic of fermentative processes, while the bodily exhaustion which always follows fever is the obvious result of the dissipation by the ferment organisms of nutritive matter destined for the repair of tissue waste. During the presence of this fever there is present in the blood an organism, *Spirochæte obermeieri*, so named after its discoverer. This disappears when the fever subsides. It is found that if other individuals are inoculated with blood taken from patients during the fever attack, the disease is communicated, but that this is not the case if the inoculation is made during the period of freedom. The evidence then seems clear that this disease is due to a definite organism. The interesting point however arises, why does the fever recur, and why eventually cease? The analogy of fermentation leads to the hypothesis that as in the case of yeast the products of its action inhibit after a time the further activity of the *Spirochæte*. The inhibiting substance is no doubt eventually removed partially from the blood by its normal processes of depuration, and the surviving individuals of *Spirochæte* can then continue their activity, as in lactic fermentation. With regard to the final cessation of the disease, there are facts which may lead one to suppose that in this as in other cases sufficient of the inhibiting substance ultimately remains in the organism to protect it against any further outbreak of activity on the part of the *Spirochæte*.

Here we have an example of a disease which, though having a well-marked zymotic character, is comparatively harmless. In anthrax, which is known to be due to *Bacillus anthracis*, we have one which is, on the contrary, extremely fatal. I need not enter into the details. It is sufficient to say that there is reason to believe that the *Bacillus* produces, as one of those by-products of protoplasmic destruction to which I have already alluded, a most virulent poison. But the remarkable thing is that this *Bacillus*, which can be cultivated externally to the body, if kept at a heightened temperature, can be attenuated in its virulence. It drops in fact the excretion of the poison. It is then found that, if injected into the blood, it does no mischief, and, what is more extraordinary, if the *Bacillus* in its most lethal form is subsequently introduced, it too has lost its power. The explanation of the immunity in this case is entirely different from that which was suggested by a consideration of the facts of relapsing fever. The researches of Metschnikoff have led to the hypothesis that in the present case the white blood-corpuscles destroy the *Bacillus*. When they first come into contact with these in their virulent form, they are unable to touch them. But if they have been educated by first having presented to them the attenuated form, they find no difficulty in grappling with the malignant. This is a very remarkable view. I should not have put it before you had there not been solid reasons for regarding the idea of the education of protoplasm with scientific respect. The plasmodia of the Myxomycetes, which consist of naked protoplasm, are known to become habituated to food which they at first reject, and the researches of Beyerinck on the disease known as "gumming" in plants have apparently shown that healthy cells may be taught, as it were, to produce a ferment which otherwise they would not excrete.

If Metschnikoff's theory be true, we have a rational explanation of vaccination and of preventive inoculation generally. It is probably however not the only explanation. And the theory of the inhibitive action upon itself of the products of the ferment-organism's own activity is still being made the basis of experiment. In fact, the most recent results point to the possibility of obtaining protection by injecting into the blood substances artificially obtained entirely independent of the organisms whose development they inhibit.

It is impossible for me to touch on these important matters at any greater length, but I doubt if the theory of fermentation, as applied to the diseases of organisms, has as yet more than opened its first page. It seems to me possible, that besides the rational explanation of zymotic disease, it may throw light on others where owing to abnormal conditions, the organism, as in the case of Bérard's plums, is itself the agent in its own fermentative processes.

And now I must conclude. I have led you, I am afraid, a too lengthy and varied a journey in the field of botanical study. But to sum up my argument: I believe I have shown you that at the bottom of every great branch of biological inquiry it has never been possible to neglect the study of plants; nay more, that the study of plant-life has generally given the key to the true course of investigation. Whether you take the problems of geographical distribution, the most obscure points in the theory of organic evolution, or the innermost secrets of vital phenomena, whether in health or disease,—not to consider plants is still, in the words of Mr. Darwin, "a gigantic oversight, for these would simplify the problem."

ELEMENTARY PROBLEMS IN PHYSIOLOGY.*

By Prof. J. S. BURDON SANDERSON.

The work of investigating the special functions of organs, which during the last two decades has yielded such splendid results, is still proceeding, and every year new ground is being broken and new and fruitful lines of experimental inquiry are being opened up; but the further the physiologist advances in this work of analysis and differentiation, the more frequently does he find his attention arrested by deeper questions relating to the essential endowments of living matter of which even the most highly differentiated functions of the animal or plant organism are the outcome. In our science the order of progress has been hitherto and will continue to be the reverse of the order of nature. Nature begins with the elementary and ends with the complex (first the amœba, then the man). Our mode of investigation has to begin at the end. And this not merely for the historical reason that the first stimulus to physiological inquiry was man's reasonable desire to know himself, but because the differentiation actually involves simplification.

Physiology therefore first studies man and the higher animals, and proceeds to the higher plants, then to invertebrates and cryptogams, ending where development begins. - - -

It is not difficult to see whither this method must eventually lead us. For inasmuch as function is more complicated than structure, the result of proceeding, as physiology normally does, from structure to function, must inevitably be to bring us face to face with functional differences which have no structural difference to explain them. Thus, for example, if the physiologist undertakes to explain the function of a highly differentiated organ like the eye, he finds that up to a certain point, provided that he has the requisite knowledge of dioptrics, the method of correlation guides him straight to his point. He can mentally or actually construct an eye which will perform the functions of the real eye, in so far as the formation of a real image of the field of vision on the retina is concerned, and will be able thereby to under stand how the retinal picture is transferred to the organ of concious-

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*Presidential address before the Biological Section of the British Association, A. S., at Newcastle, September, 1889. (*Report of the British Association*, Vol. LIX, pp. 604–614.)

ness. Having arrived at this point he begins to correlate the known structure of the retina with what is required of it, and finds that the number of objects which he can discriminate in the field of vision is as numerous as, but not more numerous than, the parts of the retina, *i. e.*, the cones which are concerned in discriminating them. So far he has no difficulty; but the method of correlation fails him from the moment that he considers that each object point in the field of vision is colored, and that he is able to discriminate not merely the number and relations of all the object points to each other, but the color of each separately. He then sees at once that each cone must possess a plurality of endowments for which its structure affords no explanation. In other words, in the minute structure of the human retina we have a mechanism which would completely explain the picture of which I am conscious, were the objects composing it colorless, *i. e.*, possessed of one objective quality only, but it leaves us without explanation of the differentiation of color.

Similarly, if we are called upon to explain the function of a secreting gland, such, *e. g.*, as the liver, there is no difficulty in understanding that inasmuch as the whole gland consists of lobules which resemble each other exactly, and each lobule is likewise made up of cells which are all alike, each individual cell must be capable of performing all the functions of the whole organ. But when by exact experiment we learn that the liver possesses not one function but many,—when we know that it is a storehouse for animal starch, and that each cell possesses the power of separating waste coloring matter from the blood, and of manufacturing several kinds of crystallizable products, some of which it sends in one direction and others in the opposite, we find again that the correlation method fails us, and that all that our knowledge of the minute structure has done for us is to set before us a question which though elementary, we are quite unable to answer.

By multiplying examples of the same kind, we should in each case come to the same issue, namely, *plurality of function with unity of structure*, the unity being represented by a simple structural element—be it retinal cone or cell—possessed of numerous endowments. Whenever this point is arrived at in any investigation, structure must for the moment cease to be our guide, and in general two courses or alternatives are open to us. One is to fall back on that worn-out *Deus ex machina*—protoplasm, as if it afforded a sufficient explanation of everything which cannot be explained otherwise, and accordingly to defer the consideration of the functions which have no demonstrable connection with structure as for the present beyond the scope of investigation; the other is, retaining our hold of the fundamental principle of correlation, to take the problem in reverse, *i. e.*, to use analysis of function as a guide to the ultra-microscopical analysis of structure.

I need scarcely say that of these two courses the first is wrong, the second right, for in following it we still hold to the fundamental principle that living material acts by virtue of its structure, provided that we allow the term structure to be used in a sense which carries it beyond the limits of anatomical investigation, *i. e.*, beyond the knowledge which can be attained either by the scalpel or the microscope. We thus (as I have said) proceed from function to structure, instead of the other way. - - -

At present the fundamental questions in physiology,—the problems which most urgently demand solution, are those which relate to the endowments of apparently structureless living matter, and the problem of the future will be the analysis of these endowments. With this view, what we have to do is first, to select those cases in which the vital process offers itself in its simplest form, and is consequently best understood; and secondly, to inquire how far in these particular instances we may, taking as our guide the principle I have so often mentioned as fundamental, viz, the correlation of structure with function, of mechanism with action, proceed in drawing inferences as to the mechanism by which these vital processes are in these simplest cases actually carried out.

The most distinctive peculiarity of living matter, as compared with non-living, is that it is ever changing while ever the same, *i. e.*, that life is a state of ceaseless change. For our present purpose I must ask you, first, to distinguish between two kinds of change which are equally characteristic of living organisms, namely, those of growth and decay on the one hand, and those of nutrition on the other. Growth, the biologist calls evolution. Growth means the unfolding, *i. e.*, development of the latent potentialities of form and structure which exist in the germ, and which it has derived by inheritance. A growing organism is not the same to-day as it was yesterday, and consequently not quite the same now as it was a minute ago, and never again will be. This kind of change I am going to ask you to exclude from consideration altogether at this moment, (for in truth it does not belong to Physiology, but rather to Morphology,) and to limit your attention to the other kind which includes all other vital phenomena. I designated it just now as nutrition, but this word expresses my meaning very inadequately. The term which has been used for half a century to designate the sum or complex of the non-developmental activities of an organism is "exchange of material," for which Professor Foster has given the very acceptable substitute metabolism. Metabolism is only another word for "change," but in using it we understand it to mean that although an organism in respect of its development may never be what it has been, the phases of alternate activity and repose which mark the flow of its life-stream are recurrent. Life is a cyclosis in which the organism returns after every cycle to the same point of departure, ever changing—yet ever the same.

It is this antithesis which constitutes the essential distinction between the two great branches of biology, the two opposite aspects in which the world of life presents itself to the inquiring mind of man. Seen from the morphological side, the whole plant and animal kingdom constitutes the unfolding of a structural plan which was once latent in a form of living material of great apparent simplicity. From the physiological side this apparently simple material is seen to be capable of the discharge of functions of great complexity, and therefore must possess corresponding complexity of mechanism. It is the nature of this invisible mechanism that physiology thirsts to know. Although little progress has as yet been made, and little may as yet be possible, in satisfying this desire, yet, as I shall endeavor to show you, the existing knowledge of the subject has so far taken consistent form in the minds of the leaders of physiological thought, that it is now possible to distinguish the direction in which the soberest speculation is tending.

The non-developmental vital functions of protoplasm are the absorption of oxygen, the discharge of carbon dioxide and water and ammonia, the doing of mechanical work, the production of heat, light, and electricity. All these, excepting the last, are known to have chemical actions as their inseparable concomitants. As regards electricity, we have no proof of the dependence of the electrical properties of plants and animals on chemical action. But all the other activities which have been mentioned are fundamentally chemical.

Let us first consider the relation of oxygen to living matter and vital process. For three quarters of a century after the fundamental discoveries of Lavoisier and Priestley (1772–'76) the accepted doctrine was that the effete matter of the body was brought to the lungs by the circulation and burnt there, of which fact the carbon dioxide expired seemed an obvious proof. Then came the discovery that arterial blood contained more oxygen than venous blood, and consequently that oxygen must be conveyed as such by the blood stream to do its purifying work in all parts of the body.

Between 1872 and 1876, as the result of an elaborate series of investigations of the respiratory process, the proof was given by Pflüger* that the function of oxygen in the living organism is not to destroy effete matter either here or there, but rather to serve as a food for protoplasm, which so long as it lives is capable of charging itself with this gas, absorbing it with such avidity, that although its own substance retains its integrity, no free oxygen can exist in its neighborhood. The generally accepted notion of effete matter waiting to be oxidized, was associated with a more general one, viz, that the elaborate structure of the body was not permanent, but constantly undergoing decay and renewal. What we have now learnt is that the material to be oxidized comes as much from the outside, as the oxygen which burns it, though the re-action between them, *i. e.*, the oxidation, is intrinsic, *i. e.*, takes place within the living molecular frame-work.

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* Pflüger's *Archiv*, 1872, vol. VI, p. 43; and 1875, vol. X, p. 251. "Ueber die physiologische Verbrennung in den lebendigen Organismen."

Protoplasm therefore (understanding by the term the visible and tangible presentation to our senses of living material) comes to consist of two things, namely, of frame-work and of content,—of channel and of stream,—of acting part which lives and is stable,—of acted-on part which has never lived and is labile, that is, in a state of metabolism, or chemical transformation.

If such be the relation between the living frame-work and the stream which bathes it, we must attribute to this living, stable, acting part a property which is characteristic of the bodies called in physiological language ferments or enzymes, the property which, following Berzelius, we have for the last half century expressed by the word catalytic, which we use, without thereby claiming to understand it, to indicate a mode of action in which the agent which produces the change does not itself take part in the decompositions which it produces.

I have brought you to this point as the outcome of what we know as to the essential nature of the all-important relation between oxygen and life. In botanical physiology the general notion of a stable catalysing frame-work, and of an interstitial labile material, which might be called catalyte, has been arrived at on quite other grounds. This notion is represented in plant physiology by two words, both of which correspond in meaning,—Micellæ, the word devised by Nägeli, and the better word, Tagmata, substituted for it by Pfeffer. Nägeli's word has been adopted by Professor Sachs as the expression of his own thought in relation to the ultra-microscopical structure of the protoplasm of the plant cell. His view is that certain well-known properties of organized bodies require for their explanation the admission that the simplest visible structure is itself made up of an arrangement of units of a far inferior order of minuteness. It is these hypothetical units that Nägeli has called Micellæ.

Now, Nägeli[*], in the first instance, confounded the micellæ with molecules, conceiving that the molecule of living matter must be of enormous size. But inasmuch as we have no reason for believing that any form of living material is chemically homogeneous, it was soon recognized, perhaps first by Pfeffer,[†] but eventually also by Nägeli himself, that a micellæ, the ultimate element of living material, is not equivalent to a molecule, however big or complex, but must rather be an arrangement or phalanx of molecules of different kinds. Hence the word Tagma, first used by Pfeffer, has come to be accepted as best expressing the notion. And here it must be noted that each of the physiologists to whom reference has been made, regards the micellæ, not as a mere aggregate of separate particles, but as connected together so as to form a system;—a conception which is in harmony with the view I gave you just now from the side of animal physiology, of catalysing frame-work and interstitial catalysable material.

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[*] Nägeli, "Theorie der Gährung," *Beitrag zur Molecular Physiologie*, 1879, p. 121.

[†] Pfeffer, *Pflanzenphysiologie*, Leipsic, 1881, p. 12.

To Professor Sachs, this porous constitution of protoplasm serves to explain the property of vital turgescence, that is, its power of charging itself with aqueous liquid;—a power which Sachs estimates to be so enormous that living protoplasm may, he believes, be able to condense water which it takes into its interstices to less than its normal volume. For the moment it is sufficient for us to understand that to the greatest botanical thinkers, as well as to the greatest animal physiologists, the ultimate mechanism by which life is carried on is not as Professor Sachs* puts it, "slime," but "a very distensible and exceedingly fine net-work."

And now let us try to get a step further by crossing back in thought from plants to animals. At first sight the elementary vital processes of life seem more complicated in the animal than in the plant, but they are on the contrary simpler; for plant protoplasm, though it may be structurally homogeneous, is dynamically polyergic,—it has many endowments—whereas in the animal organism there are cases in which a structure has only one function assigned to it. Of this the best examples are to be found within so-called excitable tissues, viz, those which are differentiated for the purpose of producing (along with heat) mechanical work, light, or electricity. In the life of the plant these endowments, if enjoyed at all, are enjoyed in common with others.

By the study therefore of muscle, of light organ and of electrical organ, the vital mechanism is more accessible than by any other portal. About light organs we as yet know little, but the little we do know is of value. Of electrical organs rather more, about muscle a great deal.

To the case of muscle, Engelmann, one of the best observers and thinkers on the elementary questions which we have now before us, has transferred the terminology of Nägeli and Pfeffer as descriptive of the mechanism of its contraction. Muscular protoplasm differs from those kinds of living matter to which I have applied the term "polyergic," in possessing a molecular structure comparable with that of a crystal in this respect that each portion of the apparently homogeneous and transparent material of which it consists resembles every other.

With this ultra-microscopical structure, its structure as investigated by the microscope, may be correlated, the central fact being that, just as a muscular fiber can be divided into cylinders by cross-sections, so each such cylinder is made up of an indefinite number of inconceivably minute cylindrical parts, each of which is an epitome of the whole. These Engelmann, following Pfeffer, calls ino-tagmata. So long as life lasts each minute phalanx has the power of keeping its axis parallel with those of its neighbors, and of so acting within its own sphere as to produce, whenever it is awakened from the state of rest to that of activity, a fluxion from poles to equator. In other words, muscle, like plant protoplasm, consists of a stable framework of living catalysing substance, which governs the mechanical and chemical changes that occur in the interstitial catalysable material, with this difference, that here the ultra-microscopical structure resembles that of a uniaxial crystal, whereas in plant protoplasm there may be no evidence of such arrangement.*

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* Sachs, *Experimental-Physiologie*, 1865, p. 443; and *Lectures on the Physiology of Plants*, English translation, p. 206.

According to this scheme of muscular structure, the contraction, *i. e.*, the change of form which, if allowed, a muscle undergoes when stimulated, has its seat not in the tagma, but in the interstitial material which surrounds it, and consists in the migration of that labile material from pole to equator, this being synchronous with explosive oxidation, sudden disengagement of heat, and change in electrical state of the living substance. Let us now see how far the scheme will help us to an understanding of this marvellous concomitance of chemical, electrical, and mechanical change.

It is not necessary to prove to you that the discharge of carbon dioxide and the production of heat which we know to be associated with that awakening of a muscle to activity which we call stimulation are indices of oxidation. If we take this fact in connection with the view that has just been given of the mechanism of contraction, it is obvious that there must be in the sphere of tagma an accumulation of oxygen and oxidizable material, and that concomitantly with or antecedently to the migration of liquid from pole to equator these must come into encounter. Let us for a moment suppose that a soluble carbo-hydrate is the catalysable material, that this is accumulated equatorially, and oxygen at the poles, and consequently that between equator and poles water and carbon dioxide, the only products of the explosion, are set free. That the process is really of this nature is the conclusion to which an elaborate study of the electrical phenomena which accompany it, has led one of the most eminent physiologists of the present time, Professor Bernstein.† To this I wish for a moment to ask your attention.

Professor Bernstein's view of the molecular structure of muscular protoplasm is in entire accordance with the theory of Pflüger and with the scheme of Engelmann, with this addition, that each ino-tagma is electrically polarized when in a state of rest, depolarized at the moment of excitation or stimulation, and that the axes of the tagmata are so directed that they are always parallel to the surface of the fiber, and consequently have their positive sides exposed. In this amended form the theory admits of being harmonized with the fundamental facts of muscle-electricity, namely, that cut surfaces are negative to sound surfaces, and excited parts to inactive, provided that the direction of the hypothetical polarization is from equator to pole, *i. e.*, that in the resting state the poles of each tagma are charged with negative ions, the equators with positive, and consequently that the direction of the discharge in the catalyte at the moment that the polarization disappears, is from pole to equator.

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* Brücke, *Vorlesungen*, 2d edition, vol. II, p. 497.

† Bernstein, "Neue Theorie der Erregungsvorgänge und electrischen Ersheinungen an den Nerven- und Muskel-fasern," *Untersuchungen aus dem Physiologischen Institut*, Halle, 1888.

Time forbids me even to attempt to explain how this theory enables us to express more consistently the accepted explanations of many collateral phenomena, particularly those of electrotonus. I am content to show you that it is not impossible to regard the three phenomena, viz, chemical explosion, sudden electrical change, and change of form, as all manifestations of one and the same process,—as products of the same mechanism.

In plants, in certain organs or parts in which movement takes place as in muscles in response to stimulation, the physiological conditions are the same or similar, but the structural very different; for the effect is produced not by a change of form, but by a diminution of volume of the excited part, and this consists not of fibers but of cells. The way in which the diminution of volume of the whole organ is brought about is by diminution of the volume of each cell, an effect which can obviously be produced by flow of liquid out of the cell. At first sight therefore the differences are much more striking than the resemblances.

But it is not so in reality. For the more closely we fix our attention on the elementary process rather than on the external form, the stronger appears the analogy—the more complete the correspondence. The state of turgor, as it has been long called by botanical physiologists, by virtue of which the frame-work of the protoplasm of the plant retains its content with a tenacity to which I have already referred, is the analogue of the state of polarization of Bernstein. As regards its state of aggregation, it can scarcely be doubted that inasmuch as the electrical concomitants of excitation of the plant cell so closely correspond with those of muscle, here also the tagmata are cylindrical, and have their axes parallel to each other. Beyond this we ought perhaps not to allow speculation to carry us, but it is scarcely possible to refrain from connecting this inference with the streaming motion of protoplasm, which in living plant cells is one of the indices of vitality. If, as must I think be supposed, this movement is interstitial, *i. e.*, due to the mechanical action of the moving protoplasm on itself, we can most readily understand its mechanism as consisting in rhythmically recurring phases of close and open order in the direction of the tagmatic axes.

I have thus endeavored—building on two principles in physiology,—firstly, that of the constant correlation of mechanism and action, of structure and function,—and secondly, the identity of plant and animal life—both as regards mechanism and structure, and on two experimentally ascertained elementary relations, viz, the relation of living matter or protoplasm to water on the one hand and to oxygen and food on the other,—to present in part the outline or sketch of what might (if I had time to complete it) be an adequate conception of the mechanism and process of life as it presents itself under the simplest conditions.

To complete this outline, so far as I can to-day, I have but one other consideration to bring before you, one which is connected with the last of my four points of departure,—that of the relation of oxygen to protoplasm, a relation which springs out of the avidity with which, without being oxidized or even sensibly altered in chemical constitution, it seizes upon oxygen and stores it for its own purposes. The consideration which this suggests is that if the oxygen and oxidizable material are constantly stored, they must either constantly or at intervals be discharged; and inasmuch as we know that in every instance, without exception, in which heat is produced or work is done, these processes have discharge of water and of carbon dioxide for their concomitants, we are justified in regarding these discharges as the sign of expenditure, the charging with oxygen as the sign of restitution. In other words, a new characteristic of living process springs out of those we have already had before us, namely, that it is a constantly recurring alternation of opposite and complementary states, that of activity or discharge, that of rest or restitution.

Is it so or is it not? In the minds of most physiologists the distinction between the phenomena of discharge and the phenomena of restitution (*Erholung*) is fundamental, but beyond this, unanimity ceases. One distinguished man in Germany and one in England—Professor Hering and Dr. Gaskell—have taken, on independent grounds, a different view to the one suggested, according to which life consists not of alternations between rest and activity, charge and discharge, loading and exploding, but between two kinds of activity, two kinds of explosion, which differ only in the direction in which they act, in the circumstance that they are antagonistic to each other.

Now when we compare the two processes of rest, which as regards living matter means restitution, and discharge which means action, with each other, they may further be distinguished in this respect, that whereas restitution is autonomic, *i. e.*, goes on continuously like the administrative functions of a well-ordered community, the other is occasional, *i. e.*, takes place only at the suggestion of external influences; that, in other words, the contrast between action and rest is (in relation to protoplasm) essentially the same as between waking and sleeping.

It is in accordance with this analogy between the alternation of waking and sleeping of the whole organism, and the corresponding alternation of restitution and discharge, of every kind of living substance, that physiologists by common consent use the term stimulus (*Reiz*, *Prikkeling*), meaning thereby nothing more than that it is by external disturbing or interfering influence of some kind that energies stored in living material are (for the most part suddenly) discharged. Now, if I were to maintain that restitution is not autonomic, but determined, as waking is, by an external stimulus,—that it differed from waking only in the direction of which the stimulation acts, *i. e.*, in the tendency towards construction on the one hand, towards destruction on the other, I should fairly and as clearly as possible express the doctrine which, as I have said, the two distinguished teachers I have mentioned, viz, Dr. Gaskell and Professor Hering,* have embodied in words which have now become familiar to every student. The words in question, anabolism, which being interpreted means winding up, and catabolism, running down, are the creation of Dr. Gaskell. Professor Hering's equivalents for these are assimilation, which of course means storage of oxygen and oxidizable material, and disassimilation, discharge of these in the altered form of carbon dioxide and water. But the point of the theory which attaches to them lies in this, that that wonderful power which living material enjoys of continually building itself up out of its environment is, as I have already suggested, not autonomic, but just as dependent on occasional and external influences or stimuli, as we know the disintegrating processes to be; and accordingly Hering finds it necessary to include under the term stimuli not only those which determine action, but to create a new class of stimuli which he calls *Assimilations-Reize*, those which, instead of waking living mechanism to action, provoke it to rest.

It is unfortunately impossible within the compass of an address like the present, to place before you the wide range of experimental facts which have led two of the strongest intellects of our time to adopt a theory which, when looked at *à priori*, seems so contradictory. I must content myself with mentioning that Hering was led to it chiefly by the study of one of the examples to which I referred in my introduction, namely, the color-discriminating functions of the retina, Dr. Gaskell by the study of that very instructive class of phenomena which reveal to us that among the channels by which the brain maintains its sovereign power as supreme regulator of all the complicated processes which go on in the different parts of the animal organism, there are some which convey only commands to action, others commands to rest, the former being called by Gaskell catabolic, the latter anabolic. - - -

I have indicated to you that although scientific thought does not, like speculative, oscillate from side to side, but marches forward with a continued and uninterrupted progress, the stages of that progress may be marked by characteristic tendencies; and I have endeavored to show that in physiology the questions which concentrate to themselves the most lively interests are those which lie at the basis of the elementary mechanism of life. The word life is used in physiology in what, if you like, may be called a technical sense, and denotes only that state of change with permanence which I have endeavored to set forth to you. In this restricted sense of the word therefore, the question "What is Life?" is one to which the answer is approachable; but I need not say that in a higher sense—higher because it appeals to higher faculties in our nature—the word suggests something outside of mechanism, which may perchance be its cause rather than its effect.

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* Hering, *Zur Theorie Vorgänge in der lebendigen Substanz*, Prague, 1888, pp. 1-22. See also a paper by Dr. Gaskell in Ludwig's *Festschrift*, Leipsic, 1888, p. 115.

The tendency to recognize such a relation as this, is what we mean by vitalism. An anti-vitalistic tendency accompanied the great advance of knowledge that took place at the middle of the century. But even at the height of this movement there was a re-action towards vitalism, of which Virchow,* the founder of modern pathology, was the greatest exponent. Now, a generation later, a tendency in the same direction is manifesting itself in various quarters. What does this tendency mean? It has to my mind the same significance now that it had then. Thirty years ago the discovery of the cell as the basis of vital function was new, and the mystery which before belonged to the organism was transferred to the unit, which, while it served to explain everything, was itself unexplained. The discovery of the cell seemed to be a very close approach to the mechanism of life, but now we are striving to get even closer, and with the same result. Our measurements are more exact, our methods finer; but these very methods bring us to close quarters with phenomena which, although within reach of exact investigation, are as regards their essence involved in a mystery which is the more profound the more it is brought into contrast with the exact knowledge we possess of surrounding conditions.

If what I have said is true, there is little ground for the apprehension that exists in the minds of some, that the habit of scrutinizing the mechanism of life tends to make men regard what can be so learned as the only kind of knowledge. The tendency is now certainly rather in the other direction. What we have to guard against is the mixing of two methods, and so far as we are concerned, the intrusion into our subject of philosophical speculation. Let us willingly and with our hearts do homage to "divine philosophy," but let that homage be rendered outside the limits of our science. Let those who are so inclined, cross the frontier and philosophize; but to me it appers more conducive to progress that we should do our best to furnish professed philosophers with such facts relating to structure and mechanism as may serve them as aids in the investigation of those deeper problems which concern man's relations to the past, the present, and the unknown future.

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*Virchow, "Alter und neuer Vitalismus," *Archiv für path. Anat.* 1856, vol. IX, p. 1. See also Rindfleisch, *Aertzliche Philosophie*, Würzburg, 1888, pp. 10–13.

ON BOSCOVICH'S THEORY.*

By Sir WILLIAM THOMPSON.

Without accepting Boscovich's fundamental doctrine that the ultimate atoms of matter are points endowed each with inertia and with mutual attractions or repulsions dependent on mutual distances, and that all the properties of matter are due to equilibrium of these forces, and to motions, or changes of motion produced by them when they are not balanced, we can learn something towards an understanding of the real molecular structure of matter, and of some of its thermo-dynamic properties, by consideration of the static and kinetic problems which it suggests. Hooke's exhibition of the forms of crystals by piles of globes, Naviers' and Poisson's theory of the elasticity of solids, Maxwell's and Clausius' work in the kinetic theory of gases, and Tait's more recent work on the same subject—all developments of Boscovich's theory pure and simple—amply justify this statement.

Boscovich made it an essential in his theory that at the smallest distances there is repulsion, and at greater distances attraction; ending with infinite repulsion at infinitely small distance, and with attraction according to Newtonian law for all distances for which this law has been proved. He suggested numerous transitions from attraction to repulsion, which he illustrated graphically by a curve—the celebrated Boscovich curve—to explain cohesion, mutual pressure between bodies in contact, chemical affinity, and all possible properties of matter—except heat, which he regarded as a sulphureous essence or virtue. It seems now wonderful that after so clearly stating his fundamental postulate which included inertia, he did not see inter-molecular motion as a necessary consequence of it, and so discover the kinetic theory of heat for solids, liquids, and gases; and that he only *used* his inertia of the atoms to explain the known phenomena of the inertia of palpable masses, or assemblages of very large numbers of atoms.

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*A communication to Section A of the British Association A. S., at Newcastle, September 13, 1889. (*Report of the British Association*, vol. LIX, pp. 494-496. Also, *Nature*, October 3, 1889, vol. XL, pp. 545-547.)

It is also wonderful how much towards explaining the crystallography and elasticity of solids, and the thermo-elastic properties of solids, liquids, and gases, we find without assuming more than one transition from attraction to repulsion. Suppose for instance the mutual force between two atoms to be repulsive when the distance between them is $<$ Z; zero when it is $=$ Z; and attractive when it is $>$ Z; and consider the equilibrium of groups of atoms under these conditions.

A group of two would be in equilibrium at distance Z, and only at this distance. This equilibrium is stable.

A group of three would be in stable equilibrium at the corners of an equilateral triangle, of sides Z; and only in this configuration. There is no other configuration of equilibrium except with the three in one line. There is one, and there may be more than one, configuration of unstable equilibrium, of the three atoms in one line.

The only configuration of stable equilibrium of four atoms is at the corners of an equilateral tetrahedron of edges Z. There is one, and there may be more than one configuration of unstable equilibrium of each of the following descriptions:

(1) Three atoms at the corners of an equilateral triangle, and one at its center.

(2) The four atoms at the corners of a square.

(3) The four atoms in one line.

There is no other configuration of equilibrium of four atoms, subject to the conditions stated above as to mutual force.

In the oral communication to Section A, important questions as to the equilibrium of groups of five, six, or greater finite numbers of atoms were suggested. They are considered in a communication by the author to the Royal Society of Edinburgh, of July 15, to be published in the Proceedings before the end of the year. The Boscovichian foundation for the elasticity of solids with no inter-molecular vibrations was slightly sketched, in the communication to Section A, as follows:

Every infinite homogeneous assemblage* of Boscovich atoms is in equilibrium. So therefore is every finite homogeneous assemblage, provided that extraneous forces be applied to all within influential distance of the frontier, equal to the forces which a homogeneous continuation of the assemblage through influential distance beyond the frontier would exert on them. The investigation of these extraneous forces for any given homogeneous assemblage of single atoms, or of groups of atoms, as explained below, constitutes the Boscovich equilibrium-theory of elastic solids.

To investigate the equilibrium of a homogeneous assemblage of two or more atoms, imagine in a homogeneous assemblage of groups of i atoms, all the atoms except one held fixed. This one experiences zero resultant force from all the points corresponding to it in the whole assemblage, since it and they constitute a homogeneous assemblage of single points. Hence it experiences zero resultant force also from all the other $i-1$ assemblages of single points. This condition, fulfilled for each one of the atoms of the compound molecule, clearly suffices for the equilibrium of the assemblage, whether the constituent atoms of the compound molecule are similar or dissimilar.

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* "*Homogeneous assemblage of points*, or *of groups of points*, or *of bodies*, or *of systems of bodies*," is an expression which needs no definition, because it speaks for itself unambiguously. The geometrical subject of homogeneous assemblages is treated with perfect simplicity and generality by Bravais, in the *Journal de l'Ecole Polytechnique*, cahier xix, pp. 1-128. (Paris, 1850.)

When all the atoms are similar—that is to say, when the mutual force is the same for the same distance between every pair—it might be supposed that a homogeneous assemblage, to be in equilibrium, must be of single points; but this is not true, as we see synthetically, without reference to the question of stability, by the following examples, of homogeneous assemblages of symmetrical groups of points, with the condition of equilibrium for each when the mutual forces act.

Preliminary.—Consider an equilateral* homogeneous assemblage of single points, O, O′, etc. Bisect every line between nearest neighbors by a plane perpendicular to it. These planes divide space into rhombic dodekahedrons. Let A_1OA_5, A_2OA_6, A_3OA_7, A_4OA_8, be the diagonals through the eight trihedral angles of the dodekahedron inclosing O, and let $2a$ be the length of each. Place atoms Q_1, Q_5, Q_2, Q_6, Q_3, Q_7, Q_4, Q_8, on these lines, at equal distances, r, from O; and do likewise for every other point, O′, O″, etc., of the infinite homogeneous assemblage. We thus have, around each point A, four atoms, Q, Q′, Q″, Q‴, contributed by the four dodekahedrons of which trihedral angles are contiguous in A, and fill the space around A. The distance of each of these atoms from A is $a - r$.

Suppose, now, r to be very small. Mutual repulsions of the atoms of the groups of eight around the points O will preponderate. But suppose $a-r$ to be very small: mutual repulsions of the atoms of the groups of four around the points A will preponderate. Hence for some value of r between O and a, there will be equilibrium. There may (according to the law of force) be more than one value of r between O and a giving equilibrium; but whatever be the law of force, there is one value of r giving *stable* equilibrium, supposing the atoms to be constrained to the lines OA, and the distances r to be constrainedly equal. It is clear from the symmetries around O and around A, that neither of these constraints is necessary for mere equilibrium; but without them the equilibrium might be unstable. Thus we have found a homogeneous equilateral distribution of eight-atom groups, in equilibrium. Similarly, by placing atoms on the three diagonals, B_1OB_4, B_2OB_5, B_3OB_6

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*This means such an assemblage as that of the centers of equal globes piled homogeneously, as in the ordinary triangular-based, or square-based, or oblong-rectangle-based pyramids of round shot or of billiard balls.

through the six tetrahedral angles of the dodekahedron around O, we find a homogeneous equilateral distribution of six-atom groups, in equilibrium.

Place now an atom at each point O. The equilibrium will be disturbed in each case, but there will be equilibrium with a different value of r (still between o and a). Thus we have nine-atom groups and seven-atom groups.

Thus in all, we have found homogeneous distributions of six-atom, of seven-atom, of eight-atom, and of nine-atom groups, each in equilibrium. Without stopping to look for more complex groups, or for five-atom, or four-atom groups, we find a homogeneous distribution of three-atom groups in equilibrium by placing an atom at every point O, and at each of the eight points A_1, A_5, A_2, A_6, A_3, A_7, A_4, A_8. Thus we see by observing that each of these eight A's is common to four tetrahedrons of A's, and is in the center of a tetrahedron of O's; because it is a common trihedral corner point of four contiguous dodekahedrons.

Lastly, choosing A_2, A_3, A_4, so that the angles A_1OA_2, A_1OA_3, A_1OA_4, are each obtuse,[*] we make a homogeneous assemblage of two-atom groups in equilibrium by placing atoms at O, A_1, A_2, A_3, A_4. There are four obvious ways of seeing this as an assemblage of di-atomic groups, one of which is as follows: Choose A_1 and O as one pair. Through A_2, A_3, A_4 draw lines same-wards parallel to A_1O, and each equal to A_1O. Their ends lie at the centers of neighboring dodekahedrons, which pair with A_2, A_3, A_4, respectively.

For the Boscovich theory of the elasticity of solids, the consideration of this homogeneous assemblage of double atoms is very important. Remark that every O is at the center of an equilateral tetrahedron of four A's; and every A is at the center of an equal and similar, and same-ways oriented, tetrahedron of O's. The corners of each of these tetrahedrons are respectively A and three of its twelve nearest A neighbors; and O and three of its twelve nearest O neighbors.

[By aid of an illustrative model showing four of the one set of tetrahedrons with their corner atoms painted blue, and one tetrahedron of atoms in their centers, painted red, the mathematical theory which the author had communicated to the Royal Society of Edinburgh, was illustrated to section A.]

In this theory it is shown that in an elastic solid constituted by a single homogeneous assemblage of Boscovich atoms, there are in general two different rigidities, n, n_1, and one bulk-modulus, k; between which there is essentially the relation $3k = 3n + 2n_1$, whatever be the law of force. The law of force may be so adjusted as to make $n_1 = n$; and in this case we have $3k = 5n$, which is Poisson's relation. But no such relation is obligatory when the elastic solid consists of a homogeneous assemblage of double, or triple or multiple Boscovich atoms. On the contrary, any arbitrarily chosen values may be given to the bulk-modulus and to the rigidity, by proper adjustment of the law of force, even though we take nothing more complex than the homogeneous assemblage of double Boscovich atoms above described.

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[*] This also makes A_2OA_3, A_2OA_4, and A_3OA_4 each obtuse. Each of these six obtuse angles is equal to $180 - \cos^{-1}(\frac{1}{3})$.

The most interesting and important part of the subject, the kinetic, was, for want of time, but slightly touched in the communication to Section A. The author hopes to enter on it more fully in a future communication to the Royal Society of Edinburgh.

THE MODERN THEORY OF LIGHT.[*]

By Prof. OLIVER J. LODGE.

To persons occupied in other branches of learning, and not directly engaged in the study of physical science, some rumor must probably have travelled of the stir and activity manifest at the present time among the votaries of that department of knowledge.

It may serve a useful purpose if I try and explain to outsiders what this stir is mainly about and why it exists. There is a proximate and there is an ultimate cause. The proximate cause is certain experiments exhibiting in a marked and easily recognizable way the already theoretically predicted connection between electricity and light. The ultimate cause is that we begin to feel inklings and foretastes of theories, wider than that of gravitation, more fundamental than any theories which have yet been advanced; theories which, if successfully worked out, will carry the banner of physical science far into the dark continent of metaphysics, and will illuminate with a clear philosophy much that at present is only dimly guessed. More explicitly, we begin to perceive chinks of insight into the natures of electricity, of æther, of elasticity, and even of matter itself. We begin to have a kinetic theory of the physical universe.

We are living, not in a Newtonian, but at the beginning of a perhaps still greater, Thomsonian era. Greater not because any one man is probably greater than Newton,[†] but because of the stupendousness of the problems now waiting to be solved. There are a dozen men of great magnitude, either now living or but recently deceased, to whom what we now know towards these generalizations is in some measure due, and the epoch of complete development may hardly be seen by those now alive. It is proverbially rash to attempt prediction, but it seems to me that it may well take a period of fifty years for these great strides to be fully accomplished. If it does, and if progress goes on at anything like its present rate, the aspect of physical science bequeathed to the latter half of the twentieth century will indeed excite admiration, and when the populace are sufficiently educated to appreciate it, will form a worthy theme for poetry, for oratorios, and for great works of art.

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[*] Being the general substance of a lecture to the Ashmolean Society in the University of Oxford, on Monday, June 3, 1889. (*University College Magazine*, Liverpool, July, 1889, vol. IV, pp. 90–99.)

[†] Though indeed a century hence it may be premature to offer an opinion on such a point.

To attempt to give any idea of the drift of progress in all the directions which I have hastily mentioned, to attempt to explain the beginnings of the theories of elasticity and of matter, would take too long, and might only result in confusion. I will limit myself chiefly to giving some notion of what we have gained in knowledge concerning electricity, æther, and light. Even that is far too much; I find I must confine myself principally to light, and only treat of the others as incidental to that.

For now well-nigh a century we have had a wave theory of light; and a wave theory of light is quite certainly true. It is directly demonstrable that light consists of waves of some kind or other, and that these waves travel at a certain well-known velocity, seven times the circumference of the earth per second, taking eight minutes on the journey from the sun to the earth. This propagation in time of an undulatory disturbance necessarily involves a medium. If waves setting out from the sun exist in space eight minutes before striking our eyes, there must necessarily be in space some medium in which they exist and which conveys them. Waves we can not have unless they be waves in something.

No ordinary medium is competent to transmit waves at anything like the speed of light, hence the luminiferous medium must be a special kind of substance, and it is called the æther. The *luminiferous* æther it used to be called, because the conveyance of light was all it was then known to be capable of; but now that it is known to do a variety of other things also, the qualifying adjective may be dropped.

Wave motion in æther, light certainly is; but what does one mean by the term wave? The popular notion is, I suppose, of something heaving up and down, or perhaps of something breaking on the shore in which it is possible to bathe. But if you ask a mathematician what he means by a wave, he will probably reply that the simplest wave is

$$y = a \sin (pt - nx)$$

and he might possibly refuse to give any other answer.

And in refusing to give any other answer than this, or its equivalent in ordinary words, he is entirely justified; that is what is meant by the term wave, and nothing less general would be all-inclusive.

Translated into ordinary English the phrase signifies "a disturbance periodic both in space and time." Anything thus doubly periodic is a wave; and all waves, whether in air as sound waves, or in æther as light waves, or on the surface of water as ocean waves, are comprehended in the definition.

What properties are essential to a medium capable of transmitting wave motion? Roughly we may say two: *elasticity* and *inertia*. Elasticity in some form, or some equivalent of it, in order to be able to store up energy and effect recoil; inertia, in order to enable the disturbed substance to over-shoot the mark and oscillate beyond its place of equilibrium to and fro. Any medium possessing these two properties can transmit waves, and unless a medium possesses these properties in some form or other, or some equivalent for them, it may be said with moderate security to be incompetent to transmit waves. But if we make this latter statement one must be prepared to extend to the terms elasticity and inertia their very largest and broadest signification, so as to include any possible kind of restoring force, and any possible kind of persistence of motion respectively.

These matters may be illustrated in many ways, but perhaps a simple loaded lath or spring in a vise will serve well enough. Pull aside one end, and its elasticity tends to make it recoil; let it go and its inertia causes it to over-shoot its normal position; both causes together cause it to swing to and fro till its energy is exhausted. A regular series of such springs at equal intervals in space, set going at regular intervals of time one after the other, gives you at once a wave motion and appearance which the most casual observer must recognize as such. A series of pendulums will do just as well. Any wave-transmitting medium must similarly possess some form of elasticity and of inertia.

But now proceed to ask what is this æther which in the case of light is thus vibrating? What corresponds to the elastic displacment and recoil of the spring or pendulum? What corresponds to the inertia whereby it over-shoots its mark? Do we know these properties in the æther in any other way?

The answer, given first by Clerk Maxwell, and now reiterated and insisted on by experiments performed in every important laboratory in the world, is:

The elastic displacement corresponds to electro-static charge (roughly speaking, to electricity).

The inertia corresponds to magnetism.

This is the basis of the modern electro-magnetic theory of light. Now let me illustrate electrically how this can be.

The old and familiar operation of charging a Leyden jar—the storing up of energy in a strained di-electric—any electro-static charging whatever—is quite analogous to the drawing aside of our flexible spring. It is making use of the elasticity of the æther to produce a tendency to recoil. Letting go the spring is analogous to permitting a discharge of the jar—permitting the strained di-electric to recover itself—the electro-static disturbance to subside.

In nearly all the experiments of electro-statics ætherial elasticity is manifest.

Next consider inertia. How would one illustrate the fact that water, for instance, possesses inertia—the power of persisting in motion against obstacles—the power of possessing kinetic energy? The most direct way would be, to take a stream of water and try suddenly to stop it. Open a water tap freely and then suddenly shut it. The impetus or momentum of the stopped water makes itself manifest by a violent shock to the pipe, with which everybody must be familiar. This momentum of water is utilized by engineers in the "water-ram."

A precisely analogous experiment in electricity is what Faraday called "the extra current." Send a current through a coil of wire round a piece of iron, or take any other arrangement for developing powerful magnetism, and then suddenly stop the current by breaking the circuit. A violent flash occurs if the stoppage is sudden enough, a flash which means the bursting of the insulating air partition by the accumulated electro magnetic momentum.

Briefly we may say that nearly all electro-magnetic experiments illustrate the fact of ætherial inertia.

Now return to consider what happens when a charged conductor (say a Leyden jar) is discharged. The recoil of the strained di-electric causes a current, the inertia of this current causes it to over-shoot the mark, and for an instant the charge of the jar is reversed; the current now flows backwards and charges the jar up as at first; back again flows the current, and so on, charging and reversing the charge with rapid oscillations until the energy is all dissipated into heat. The operation is precisely analogous to the release of a strained spring, or to the plucking of a stretched string.

But the discharging body thus thrown into strong electrical vibration is embedded in the all-pervading æther, and we have just seen that the æther possesses the two properties requisite for the generation and transmission of waves, viz, elasticity, and inertia or density; hence just as a tuning-fork vibrating in air excites aerial waves or sound, so a discharging Leyden jar in æther excites ætherial waves or light.

Ætherial waves can therefore be actually produced by direct electrical means. I discharge here a jar, and the room is for an instant filled with light. With light, I say, though you can see nothing. You can see and hear the spark indeed (but that is a mere secondary disturbance we can for the present ignore), I do not mean any secondary disturbance. I mean the true ætherial waves emitted by the electric oscillation going on in the neighborhood of this recoiling di-electric. You pull aside the prong of a tuning-fork and let it go: vibration follows and sound is produced. You charge a Leyden jar and let it discharge: vibration follows and light is excited.

It is light, just as good as any other light. It travels at the same pace, it is reflected and refracted according to the same laws; every experiment known to optics can be performed with this ætherial radiation electrically produced, and yet you cannot see it. Why not? For no fault of the light, the fault (if there be a fault), is in the eye. The retina is incompetent to respond to these vibrations—they are too slow. The vibrations set up when this large jar is discharged are from a hundred thousand to a million per second, but that is too slow for the retina. It responds only to vibrations between 400 billion and 800 billion per second. The vibrations are too quick for the ear, which responds only to vibrations between 40 and 40,000 per second. Between the highest audible and the lowest visible vibrations there has been hitherto a great gap, which these electric oscillations go far to fill up. There has been a great gap simply because we have no intermediate sense organ to detect rates of vibration between 40,000 and 400,000,000,000,000 per second. It was therefore an unexplored territory. Waves have been there all the time in any quantity, but we have not thought about them nor attended to them.

It happens that I have myself succeeded in getting electric oscillations so slow as to be audible, the lowest I have got at present are 125 per second, and for some way above this the sparks emit a musical note; but no one has yet succeeded in directly making electric oscillations that are visible,—though indirectly every one does it when he lights a candle.

Here however is an electric oscillator which vibrates 300 million times a second, and emits ætherial waves a yard long. The whole range of vibrations between musical tones and some thousand million per second, is now filled up.

These electro-magnetic waves have long been known on the side of theory, but interest in them has been immensely quickened by the discovery of a receiver or detector for them. The great though simple discovery by Hertz of an "electric eye," as Sir W. Thomson calls it, makes experiments on these waves for the first time easy or even possible. We have now a sort of artificial sense organ for their appreciation,—an electric arrangement which can virtually "see" these intermediate rates of vibration.

The Hertz receiver is the simplest thing in the world; nothing but a bit of wire or a pair of bits of wire adjusted so that when immersed in strong electric radiation they give minute sparks across a microscopic air gap.

The receiver I have here is adapted for the yard-long waves emitted from this small oscillator; but for the far longer waves emitted by a discharging Leyden jar an excellent receiver is a gilt wall-paper or other interrupted metallic surface. The waves falling upon the metallic surface are reflected, and in the act of reflexion excite electric currents, which cause sparks. Similarly, gigantic solar waves may produce auroræ; and minute waves from a candle do electrically disturb the retina.

The smaller waves are however far the most interesting and the most tractable to ordinary optical experiments. From a small oscillator, which may be a couple of small cylinders kept sparking into each other end to end by an induction coil, waves are emitted on which all manner of optical experiments can be performed.

They can be reflected by plain sheets of metal, concentrated by parabolic reflectors, refracted by prisms, concentrated by lenses. I have at the college a large lens of pitch, weighing over three hundred-weight, for concentrating them to a focus. They can be made to show the phenomenon of interference, and thus have their wave-length accurately measured. They are stopped by all conductors and transmitted by all insulators. Metals are opaque, but even imperfect insulators such as wood or stone are strikingly transparent, and waves may be received in one room from a source in another, the door between the two being shut.

The real nature of metallic opacity and of transparency has long been clear in Maxwell's theory of light, and these electrically produced waves only illustrate and bring home the well known facts. The experiments of Hertz are in fact the apotheosis of that theory.

Thus then in every way, Maxwell's 1865 brilliant perception of the real nature of light is abundantly justified; and for the first time we have a true theory of light, no longer based upon analogy with sound, nor upon a hypothetical jelly or elastic solid.

Light is an electro-magnetic disturbance of the æther. Optics is a branch of electricity. Outstanding problems in optics are being rapidly solved, now that we have the means of definitely exciting light with a full perception of what we are doing and of the precise mode of its vibration.

It remains to find out how to shorten down the waves—to hurry up the vibration until the light becomes visible. Nothing is wanted but quicker modes of vibration. Smaller oscillators must be used—very much smaller—oscillators not much bigger than molecules. In all probability (one may almost say certainly) ordinary light is the result of electric oscillation in the molecules of hot bodies, or sometimes of bodies not hot, as in the phenomenon of phosphorescence.

The direct generation of visible light by electric means, so soon as we have learnt how to attain the necessary frequency of vibration, will have most important practical consequences.

Speaking in this university it is happily quite unnecessary for me to bespeak interest in a subject by any reference to possible practical applications. But any practical application of what I have dealt with this evening is apparently so far distant as to be free from any sordid gloss of competition and company promotion, and is interesting in itself as a matter of pure science.

For consider our present methods of making artificial light; they are both wasteful and ineffective.

We want a certain range of oscillation, between 800 and 400 billion vibrations per second; no other is useful to us, because no other has any effect on our retina; but we do not know how to produce vibrations of this rate. We can produce a definite vibration of one or two hundred or thousand per second; in other words, we can excite a pure tone of definite pitch, and we can command any desired range of such tones continuously by means of bellows and a keyboard. We can also (though the fact is less well known) excite momentarily definite ætherial vibrations of some million per second, as I have explained at length; but we do not at present seem to know how to maintain this rate quite continuously. To get much faster rates of vibration than this we have to fall back upon atoms. We know how to make atoms vibrate; it is done by what we call "heating" the substance, and if we could deal with individual atoms unhampered by others, it is possible that we might get a pure and simple mode of vibration from them. It is possible, but unlikely; for atoms, even when isolated, have a multitude of modes of vibration special to themselves, of which only a few are of practical use to us, and we do not know how to excite some without also the others. However, we do not at present even deal with individual atoms; we treat them crowded together in a compact mass, so that their modes of vibration are really infinite.

We take a lump of matter, say a carbon filament or a piece of quicklime, and by raising its temperature we impress upon its atoms higher and higher modes of vibration, not transmuting the lower into the higher, but superposing the higher upon the lower, until at length we get such rates of vibration as our retina is constructed for, and we are satisfied. But how wasteful and indirect and empirical is the process. We want a small range of rapid vibrations, and we know no better than to make the whole series leading up to them. It is as though, in order to sound some little shrill octave of pipes in an organ, we were obliged to depress every key and every pedal, and to blow a young hurricane.

I have purposely selected as examples the most perfect methods of obtaining artificial light, wherein the waste radiation is only useless, and not noxious. But the old-fashioned plan was cruder even than this; it consisted simply in setting something burning, whereby not the fuel but the air was consumed, whereby also a most powerful radiation was produced, in the waste waves of which we were content to sit stewing, for the sake of the minute, almost infinitesimal, fraction of it which enabled us to see.

Everyone knows now however, that combustion is not a pleasant or healthy mode of obtaining light; but everybody does not realize that neither is incandescence a satisfactory and unwasteful method which is likely to be praticed for more than a few decades, or perhaps a century.

Look at the furnaces and boilers of a great steam-engine driving a group of dynamos and estimate the energy expended, and then look at the incandescent filaments of the lamps excited by them, and estimate how much of their radiated energy is of real service to the eye. It will be as the energy of a pitch-pipe to an entire orchestra.

It is not too much to say that a boy turning a handle could, if his energy were properly directed, produce quite as much real light as is produced by all this mass of mechanism and consumption of material. There might perhaps be something contrary to the laws of nature in thus hoping to get and utilize some specific kind of radiation without the rest, but Lord Rayleigh has shown in a short communication to the British Association at York that it is not so, and that therefore we have a right to try to do it.

We do not yet know how, it is true, but it is one of the things we have got to learn.

Any one looking at a common glow-worm must be struck with the fact that not by ordinary combustion, nor yet on the steam-engine and dynamo principle, is that easy light produced. Very little waste radiation is there from phosphorescent things in general. Light of the kind able to affect the retina is directly emitted; and for this, for even a large supply of this, a modicum of energy suffices.

Solar radiation consists of waves of all sizes, it is true; but then solar radiation has innumerable things to do besides making things visible. The whole of its energy is useful. In artificial lighting nothing but light is desired; when heat is wanted it is best obtained separately, by combustion. And so soon as we clearly recognize that light is an electrical vibration, so soon shall we begin to beat about for some mode of exciting and maintaining an electrical vibration of any required degree of rapidity. When this has been accomplished, the problem of artificial lighting will have been solved.

MICHELSON'S RECENT RESEARCHES ON LIGHT.*

By JOSEPH LOVERING, President.

For many generations it was assumed that no sensible time was taken by light in moving over the largest distances. The velocity of sound was found by noting the time which elapsed between seeing the flash and hearing the report of an explosion. It was only in the vast spaces of astronomy that distances existed large enough to unmask the finite velocity of light, and, in extreme cases, to make it seem even to loiter on its way.

The satellites of Jupiter were discovered by Galileo in 1610; and the eclipses of these satellites by the shadow of Jupiter became an interesting subject of observation. It was soon noticed that the interval between successive eclipses of the same satellite was shorter when the earth was approaching Jupiter, and longer when the earth was receding from Jupiter. The change of pitch in the whistle of a locomotive, under similar motions, would suggest to the modern mind an easy explanation. A Danish astronomer, Römer, without the help of this analogy, deciphered the problem in astronomy. The eclipse was telegraphed to the observer by a ray of light, and the news was hastened or delayed in proportion to the distance from which it came. In this way it was discovered that light took about eighteen minutes to run over the diameter of the earth's orbit. This discovery was published by Römer in the Memoirs of the French Academy in 1675. The mathematical astronomer Delambre, from a discussion of one thousand of these eclipses observed between 1662 and 1802, found for the velocity of light 193,350 miles a second.

Meanwhile Römer's method, after fifty years of waiting, had been substantially confirmed in an unexpected quarter. Dr. Bradley, of the Greenwich Observatory, the greatest astronomical observer of his day, was perplexed by certain periodical fluctuations, of small amount, in the position of the stars. Suddenly the explanation was flashed upon him by something he observed while yachting on the River Thames. He noticed that, whenever the boat turned about, the direction of the

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vane altered. He asked the sailors, Why? All they could say was, that it always did. Reflecting upon the matter, Bradley concluded that the motion of the boat was compounded with the velocity of the wind, and that the vane represented the resultant direction. He was not slow in seeing the application of this homely illustration of the parallelogram of motion to his astronomical puzzle. The velocity of light was compounded with the velocity of the earth in its orbit, so that its apparent direction differed by a small angle from its true direction, and the difference was called aberration. In spearing a fish or shooting a bird, the sportsman does not aim *at* them, but *ahead* of them. This inclination from the true direction is similiar, in angular measure, to what the astronomer calls aberration. Struve's measurement of aberration combined with the velocity of the earth in its orbit gave for the velocity of light 191,513 miles a second. Both of the two methods described for obtaining the velocity ot light depend for their accuracy upon the assumed distance of the earth from the sun. The distance adopted was the one found by the transits of Venus in 1761 and 1769, viz. 95,360,000 miles.

During the last forty years, the opinion has been gaining ground among astronomers that the distance of the sun, as deduced from the transits of Venus in 1761 and 1769, was too large by 3 per cent. Expeditions have been sent to remote parts of the earth for observing the planet Mars in opposition. The ablest mathematical astronomers, as Laplace, Pontecoulant, Leverrier, Hansen, Lubbock, Airy, and Delaunay, have applied profound mathematical analysis to the numerous perturbations in planetary motions, and proved that the sun's distance must be diminished about 2,000,000 miles in order to reconcile observations with the law of gravitation. Airy reduced the distance of the sun by more than 2,000,000 miles, to satisfy the observations on the transit of Venus in 1874. Glasenapp derived from observed eclipses of Jupiter's satellites a distance for the sun of only 92,500,000 miles. From these and similar data, Delaunay concluded that the velocity of light is about 186,420 miles a second.

These triumphs of astronomical theory recall the witty remark of Fontenelle, that Newton, without getting out of his arm chair, calculated the figure of the earth more accurately than others had done by travelling and measuring to the ends of it. And Laplace, in contemplation of similar mathematical achievements, says: "It is wonderful that an astronomer, without going out of his observatory, should be able to determine exactly the size and figure of the earth, and its distance from the sun and moon, simply by comparing his observations with analysis; the knowledge of which formerly demanded long and laborious voyages into both hemispheres."

The ancients supposed that light came instantaneously from the stars; a consolation for those who believed that the heavens revolved around the earth in twenty-four hours. Galileo and the academicians of Florence obtained even negative results, While the number of physical sciences has received numerous additions during the last half-century, new affiliations and a more intimate correlation have been manifested. In this mutual helpfulness light has played an important part. The optical method of studying sound, and the many varieties of flame apparatus, have made acoustics as intelligible through the eye as through the ear.

Velocity being expressed by space divided by time, it is evident that in measuring an immense velocity we must have at our command an enormous distance, such as we find only in astronomy, or else possess the means of measuring fractions of time as small as one-millionth of a second. The first successful attempt to measure such a velocity was made by Wheatstone in 1834. Discharges from a Leyden jar were sent through a wire, having two breaks in it one-fourth of a mile apart. The wire was in the form of a loop, so as to bring the breaks into the same vertical line. The sparks seen at these breaks were reflected by a mirror at the distance of 10 feet, and revolving eight hundred times per second. The images of the two sparks were relatively displaced in a horizontal direction. As the displacement did not exceed one-half of an inch, the time taken by electricity to go from one break to the other was less than a millionth of a second. Since the distance was one-quarter of a mile, the electricity travelled *in that case* at the rate of 288,000 miles a second. If this experiment is interpreted to mean that electricity would go over 288,000 miles of similar wire in one second, as it probably often was at that time, the conclusion is fallacious. The velocity of electricity, unlike that of sound or light, diminishes when the length of wire increases.

In 1838, Wheatstone suggested a method for measuring the velocity of light, which he thought was adequate for giving not only the absolute velocity but the difference of velocity in different media.

In that year Arago communicated to the French Academy the details of an experiment which he thought would give the velocity of light in air or a vacuum. As his own health was broken down (he died in 1853) he appealed to two young French physicists to undertake the experiment. On July 23, 1849, Fizeau, by a method wholly his own, made a successful experiment. A disk cut at its circumference into 720 teeth and intervals, and made by Breguet, was rapidly rotated by a train of wheels and weights. A concentrated beam of light was sent out through one of the intervals between two teeth of the disk, which was mounted in a house in Suresne, near Paris, and was sent back by a mirror placed on Montmartre, at a distance of about 5 miles. The light, on its return, was cut off from the eye or entered it, according as it encountered a tooth or an interval of the disk. If the disk turned 12.6 times in a second the light encountered the tooth adjacent to the interval through which the light went out. With twice as many rotations in the disk the light could enter the eye through the adjacent interval. With three times the original velocity, it was cut off by the next tooth but one, and so on. From the number of teeth and the number of rotations in a second the time taken by the light in going and returning was easily calculated. In this way the velocity of light was found to be 195,741 miles per second. In 1856, the Institute of France awarded to Fizeau the Imperial prize of 30,000 francs in recognition of this capital experiment.

In 1862, Foucault succeeded in measuring the velocity of light by a wholly different method, all parts of the apparatus for it being embraced within the limits of his laboratory. The light emanated from a fine reticule, ruled on glass and strongly illuminated by the sun. It then fell upon a plane mirror revolving four hundred times a second, by which it was reflected successively to five other mirrors, the last of which was plane, and returned it back by the same path to the revolving mirror and reticule. The total distance traveled was only about 66 feet. As the revolving mirror had moved while the light was making this short journey, the image of the reticule was displaced in reference to the reticule itself; and this displacement was the subject of measurement. Although the time involved was only about one fifteen-millionth of a second, this brief interval was translated by the method of the experiment into a measurable space, and gave 185,177 miles per second for the velocity of light, differing from the best results of astronomical methods by only 1,243 miles. Foucault was prompted to this experiment by Leverrier, director of the observatory. Arago was the first to propose the experiment. To obtain greater accuracy he placed the moving mirror in a vacuum, but without any advantage. He said, "Le mieux est l'ennemi du bien." His modest claim was that he had suggested to Foucault the problem and indicated certain means of resolving it. Babinet thought that the experiment admitted of ten times greater accuracy. With three times only it might correct Struve's value of aberration.

In 1873, Cornu, another French physicist, repeated the experiments of Fizeau with a toothed wheel, the work extending over three years. The observer was stationed at the École Polytechnique. The reflecting mirror and collimating telescope were placed on Mont Valerian, at a distance of about 33,816 feet. Three different wheels were tried, having 104, 116, and 140 teeth respectively, and rotating between seven and eight hundred times a second, the velocity being registered by electricity. Cornu used at times all the eclipses from the first to the seventh order. Calcium and petroleum light were tried, as well as sunlight. A chronograph with three pens recorded automatically seconds, the rotations of the toothed wheel, and the time of the eclipse. More than a thousand experiments were made, six hundred of which were reduced. The velocity of light as published by Cornu in 1873, was 185,425.6 miles per second. The probable error was 1 per cent. In 1874, Cornu gave the result of a new set of experiments made by him in conjunction with Fizeau over a distance of more than 14 miles between the Observatory and Montlhéry. The experiments were repeated more than five hundred times, mostly at night with the lime light. The light was sent through a 12 inch telescope and returned through a 7-inch telescope. The toothed wheel which produced the eclipse was capable of rotating sixteen hundred times a second. From these experiments the velocity of light was placed at 186,618 miles. The probable error did not exceed 187 miles. The time was recorded accurately within a thousandth of a second.

I come now to that which most interests us to-night, viz, the part taken in this country for the measurement of these great velocities. About 1854, Dr. Bache, chief of the U. S. Coast Survey, appropriated $1,000 for the construction of apparatus to be used in repeating Wheatstone's experiment on the velocity of electricity. But those who were expected to take part in the investigation were called to other duties, and the money was never drawn.

In 1867, Professor Newcomb recommended a repetition of Foucault's experiment, in the interest of astronomy, to confirm or correct the received value of the solar parallax. In August, 1879, Mr. Albert A. Michelson, then a master in the United States Navy, presented a paper to the meeting of the American Association for the Advancement of Science, on the measurement of the velocity of light. This paper attracted great attention. Mr. Michelson adopted Foucault's method with important modifications. In Foucault's experiment the deflection of the light produced by the revolving mirror was too small for the most accurate measurement. Mr. Michelson placed the revolving mirror 500 feet from the slit (which was ten times the distance in Foucault's experiment) and obtained a deflection twenty times as great, although the mirror made only one hundred and twenty-eight turns in a second. With apparatus comparatively crude, he obtained for the velocity of light 186,500, with a probable error of 300 miles. This preliminary experiment, made in the laboratory of the Naval Academy in May, 1878, indicated the directions in which improvements must be made in order to insure greater accuracy. The distance from the slit to the revolving mirror must be increased, the mirror must revolve at least two hundred and fifty times a second, and the lens for economizing the light must have a large surface and a focal length of about 150 feet. With the aid of $2,000 from a private source Mr. Michelson was able to carry out his ideas on a liberal scale.

His new experiments were made in the summer of 1879. The revolving mirror, made by Alvan Clark & Sons, was moved by a turbine wheel. Its rapidity of revolution was measured by optical comparison with an electric fork which made about one hundred and twenty-eight vibrations a second, the precise value being accurately measured by reference to one of König's standard forks. The velocity generally given to the mirror was about two hundred and fifty-six turns a second. The distance between the revolving and the fixed mirror was 1,986.26 feet.

The light from the moving mirror was concentrated on the fixed mirror by a lens 8 inches in diameter, with a focal length of 150 feet. These improvements on Foucault's arrangement were so advantageous that Mr. Michelson obtained, even with a smaller speed in the revolving mirror, an angle of separation between the outgoing and returning rays of light so great that the inclined plate of glass in front of the micrometer was not necessary; the head of the observer not shutting off the light. The mean result of one hundred observations taken on eighteen different days made the velocity of light 186,313 miles per second, with a probable error of 30 miles.

In 1882, at the request of Professor Newcomb, Mr. Michelson made a redetermination of the velocity of light at the Case Institute, in Cleveland, Ohio, by the method already described, with some modifications. The space traversed by the light in going and returning between the two mirrors was 4,099 feet. Two slight errors in the reduction of his former work were corrected in this. The velocity deduced from five hundred and sixty-three new observations was 186,278 miles, with a probable error of 37 miles.

In March, 1879, Congress had voted an appropriation of $5,000 for experiments on the velocity of light, to be made under the direction of Professor Newcomb. All the delicacy of instrumental construction, all the skill of scientific observation, and all the resources of mathematical discussion were enlisted in this service. The method adopted was that of the revolving mirror. The movable mirror was mounted at Fort Myer. Two different locations were selected for the fixed mirror, viz, the Naval Observatory and the Washington Monument. In one case the distance was 2,550.95 meters, or about 8,367.12 feet; in the second case, 3,721 meters, or about 12,205.57 feet. Mr. Michelson assisted in the observations until his removal to Cleveland, in the autumn of 1880. The observations began in the summer of 1880, and were continued into the autumn of 1882, the most favorable days in spring, summer, and autumn, being selected. In all five hundred and four sets of measurements were made, viz, two hundred and seventy-six by Professor Newcomb, one hundred and forty by Professor Michelson, and eighty-eight by Mr. Holcombe. After a full discussion of all the observations and the possible sources of error, Professor Newcomb decided to rest the final result on the one hundred and thirty-two sets of observations made in 1882 over the long distance between Fort Myer and the Washington Monument. The velocity then obtained was 186,282 miles. The velocity deduced from the three sets of observations was 186,251 miles. The probable error of the first result was about 19 miles.

For some future attack upon this problem Professor Newcomb suggested a prism for the reflector with a pentagonal section, and placed at such a distance that it could revolve through an arc of 36° while the light was going and returning; five hundred turns a second and a distance of 19 miles would fulfill this condition. In the Rocky Mountains, or the Sierra Nevada, stations from 20 to 30 miles distant could be found, and with no greater loss of light from absorption than is produced by 2 or 3 miles of common air.

The first experiments made in Great Britain for the measurement of the velocity of light were published by James Young and Prof. G. Forbes in the Philosophical Transactions of 1882. They adopted the method of Fizeau. In 1878, between six and seven hundred observations were made; but the number of teeth in the rotating wheel was insufficient. New experiments were made in 1880–'81 across the river Clyde. Two reflectors were used at unequal distances, and the time was noted when an electric light after the two reflections was at its maximum. The corrected distances for the two mirrors were 18,212.2 and 16,835 feet. After an elaborate mathematical discussion of the theory of this method, the velocity of light was placed at 187,221 miles. This value exceeded those obtained by Cornu or Michelson; but this might be explained by the color of the light used in the different experiments. Mr. Young and Professor Forbes made some experiments with lights of different colors, in confirmation of this view. But Professor Michelson compared his three hundred and eighteen observations with sunlight and two hundred and sixty-seven observations with electric light, and found that the difference was in the opposite direction; and in a differential experiment, when half the slit was covered with red glass, he found no displacement. Young and Forbes were attracted to their experiments on the velocity of light by Maxwell's speculations on the electro-magnetic theory of light, and also as promising the most accurate method of obtaining the parallax and distance of the sun. Their velocity of light combined with Struve's constant of aberration made the sun's parallax 20″.445, and its distance 93,223,000 miles.

When Arago, in 1838, suggested to the French Academy an experiment on the velocity of light, and explained his method of making it, which was essentially the one afterwards adopted by Foucault, he had in view the settlement of the long controversy between the advocates of the corpuscular and undulatory theories. Almost all of the different classes of phenomena in geometrical optics can be explained by either one of these theories, though even here the undulatory has the advantage of greater simplicity. But in one respect the two theories are antagonistic. According to the corpuscular theory, light should move faster in glass or water than in air, for example. The undulatory theory reversed this proposition. Here was an *experimentum crucis*. In 1850, Fizeau and Foucault made the experiment, each in his own way, and in both experiments the result was in favor of the theory of undulations. It has been shown that in the case of air alone lengths of many thousand feet are practicable. But the absorbing power of water prevents the use of greater lengths than about 10 feet. Light would pass through 10 feet of air in less time than one eighteen-thousandth of a second;

and the difference of time for air and water would be only a fraction of that small fraction. Hence the exceeding delicacy of the experiment.

In 1883, Mr. Michelson, at the request of Professor Newcomb, repeated Foucault's experiments for finding the difference of velocity of light in air and water. Foucault did not aspire to quantitative precision in his results. The experiments of Michelson proved that the ratio of the velocities was inversely as the indices of refraction. The velocity with sunlight was a little greater than with the electric light; which opposes the conclusion of Young and Forbes. When Mr. Michelson covered half of the slit with red glass, the two halves of the image were exactly in line. Experiments were also made on the velocity of light in carbon disulphide, which led to the inference that its index of refraction was 1.77, and that orange-red light traveled from one to two per cent. faster than greenish blue light. Mr. Michelson was enabled to make this investigation by a grant from the trustees of the Bache Fund.

Various other methods of measuring the velocity of light have been proposed. About 1850, Laborde suggested, in a letter to Arago, a mechanical method of measuring the velocity of light. He supposes two disks, with many holes at the outside, connected by a very long axis and rotating rapidly. The light which was sent out through a hole in one wheel would be transmitted or arrested by the second wheel, behind which an observer was stationed. The distance between the wheels, the time of rotation, and the order of the eclipse, would be sufficient for calculating the velocity of light. Laborde imagined an enormous axis more than 200,000 miles long. Moigno recommended the substitution of a mirror for the observer and the second wheel, which would double the distance travelled by the light. A distance of 1,640 feet, a disk 25 feet in radius, with 1,000 holes, and turning 360 times a second, would be more than sufficient to surprise the reflected ray and stop it.

In 1874, Burgue suggested a new way of finding the velocity of light by experiment. If a white disk, with a black radius, is rotated rapidly, and at each turn is illuminated by an instantaneous flash, this radius will appear immovable. If this flash is reflected on the disk from a distant mirror, the black radius will be displaced. No details of the arrangement of apparatus and no experiments were published.

In 1885, Wolf proposed the following arrangements: Two mirrors were placed 5 meters apart and facing each other. The radius of curvature of each mirror was 5 meters. The first mirror was 0.20 of a meter in diameter; the other, 0.05 meter, revolved rapidly (two hundred turns a second). A slit was made in the center of the large mirror through which light was sent to the small mirror, forming an image on the surface of the large mirror; this image became an object for the small mirror, forming another image on the larger mirror, at a distance from the first mirror depending on the velocity of rotation. These images could be sent out laterally by an inclined plate of thin glass, and their distance measured by a micrometer. Wolf expected advantages from the proximity of the two mirrors which would more than balance those of the long distances used by Foucault and Michelson.

The greatest difficulty which the undulatory theory of light has encountered is found in the attempted reconciliation between the requirements of the refraction of light and the aberration of light. To explain refraction, the density of the luminiferous æther must be greater when the index of refraction is greater. If a body moves, it must carry its inclosed æther with it, as its refractive power does not change. On the other hand, to explain the aberration of light, it must be supposed that the æther in the telescope does not move with the telescope; that the æther sifts through the telescope, the æther in front taking the place of the æther left behind; or, as Young expressed it, that the æther flows through the air and solid earth as easily as the wind blows through the trees of a forest.

The difficulty can be eluded by supposing that a refracting body carries along with it as much of the æther as it possesses in excess of what would exist in a vacuum of the same bulk. This, added to what is always sifting through it, would maintain its æther at a constant density. What this fraction is which must travel with the body was calculated by Fresnel. But while the refracting power has been protected, how is it with aberration? That would be increased to a small extent. But as the aberration is very small, only about 20½″ at its maximum, the required change in its value might be masked by ordinary errors of observation. Boscovich suggested to Lalande, in 1766, that a telescope filled with water instead of air would test the theory; but he made no experiment. Wilson, of Glasgow, also proposed a water telescope in 1782. In the course of time it appeared that not only was the effect of the earth's motion on refraction and aberration under trial, but also the solar parallax, the motion of the solar system, and that of other stars.

The case is clearly stated by Lodge in this way: Sound travels quicker with the wind than against it. Is it the same with light? Does light travel quicker with the wind? Well, that depends altogether on whether the æther is blowing along as well as the air. If it is, then its motion must help the light on a little; but if the æther is at rest, no motion of the air, or of matter of any kind, can make any difference. According to Fresnel, the free æther is at rest, the bound is in motion. Therefore the speed of light will be changed by the motion of the medium; but only by a fraction, depending on its index of refraction,—infinitesimal for air, but sensible for water.

At an early day Arago investigated the effect which a change in the velocity of light would produce on aberration and refraction. He saw that a change of 5 per cent. in the velocity of light would alter the aberration by only one second, whereas the refraction in a prism of 45° would be affected to the extent of two minutes. He observed the zenith distances of stars with and without the prism; and also the deviation of stars which passed the meridian at 6 A. M. and 6 P. M. The observations were made with a mural circle and a repeating circle. Arago expected to find a difference of ten or fifteen seconds, but found none. He thought that a difference no greater than one ten-thousandth would have been manifested by his observations had it existed. Arago attempted to explain his negative results by assumptions based upon the corpuscular theory of light. But Lloyd thought that the change in the length of the wave would balance the change in the direction of the ray. Arago's observations were communicated to the Institute on December 10, 1816, and excited great interest. They were quoted by Laplace and Biot. But the manuscript was mislaid and not found until 1853, when it was published. Mascart thinks that this experiment of Arago owes its reputation to Fresnel's explanation of it by his fraction.

In regard to the wave-motion involved in the transmission of light, Maxwell says: "It may be a displacement, or a rotation, or an electrical disturbance, or indeed any physical quantity which is capable o assuming negative as well as positive values. But the æther is loosely connected with the particles of gross matter; otherwise they would reflect more light." Then he asks the question, "Does the æther pass through bodies as water through the meshes of a net which is towed by a boat?" It is difficult to obtain the relative motion of the earth and æther by experiment, as the light must move forward and then back again. One way is to compare the velocities of light obtained from the eclipses of Jupiter's satellites when Jupiter is in opposite points of the ecliptic. Cornu referred, in 1883, to the difficulty of observing these eclipses, especially when Jupiter is in conjunction with the sun. On account of this difficulty observations have been neglected for the last fifty years. Observations must be made near quadratures. Cornu suggests a proper arrangement for this purpose.

At various times between 1864 and 1868, Maxwell repeated Arago's experiment in a more perfect form. A spectroscope was used, having three prisms of 60° each. A plane miror was substituted for the slit of the collimator. The cross-wires of the observing telescope were illuminated by light reflected by a plate of thin glass placed at an angle of 45°. Light went to the mirror and was sent back to the wires from which it started after passing through six prisms. The experiment was tried when the light started in the direction of the earth's motion, and when in the opposite; also, at different seasons of the year. In all cases the image of the wires coalesced with the wires.

Lodge states the case clearly thus: "If all the æther were free there would have been a displacement of the image of the wires. If all the æther were bound to the glass there would have been a difference on the other side. But, according to Fresnel's hypothesis there should be no difference either way. According to his hypothesis, the free æther, which is the portion in relative motion, has nothing to do with the refraction. It is the addition of the bound æther which causes the refraction, and this part is stationary relatively to the glass, and is not streaming through it at all. Hence the refraction is the same whether the prism be at rest or in motion through space." Maxwell is more guarded in his own statement of the case. He says: "We can not conclude certainly that the æther moves with the earth, for Stokes has shown from Fresnel's hypothesis that the relative velocities of the æther in the prism and that outside are inversely as the square of the index of refraction, and the deviation in this case would not be sensibly altered, the velocity of the earth being only one ten-thousandth of the velocity of light."

In 1879, Maxwell wrote to Prof. D. P. Todd, then at the Nautical Almanac Office in Washington, asking him if he had observed an apparent retardation of the eclipses of Jupiter's satellites depending on the geocentric position of the planet. Such observations, he thought, would furnish the only method he knew of finding the direction and velocity of the sun's motion through the surrounding medium. In terrestrial methods of measuring the velocity of light, it returns on its path, and the velocity of the earth in relation to the æther would alter the whole time of passage by a quantity depending on the square of the ratio of the velocities of the earth and light, and this is quite too small to be observed.

In 1839, Babinet made a very delicate experiment on the relation of the luminiferous æther to the motion of the earth. He found that when two pieces of glass of equal thickness were placed across two beams of light which interfered so as to produce fringes, one of them moving in the direction of the earth's motion and the other contrary to it, the fringes were not displaced. The experiment was made three times by Babinet, with new apparatus each time. He concludes that here is a new condition to be fulfilled by all theories in regard to the propagation of light in refracting media. According to all the theories admitted or proposed, the displacement of the fringes should have been equal to many lengths of a fringe—that is, many millimeters—while by observation it was nothing. Stokes has calculated the result according to Fresnel's theory, or his own modification of it, and found that the retardation expressed in time was the same as if the earth were at rest. Fizeau has pointed out a compensation in the effect of Babinet's experiment. He says: "When two rays have a certain difference of march, this difference is altered by the reflection from the turning mirror." By calculating the two effects in Babinet's experiment, Fizeau finds that they have sensibly equal values, and of opposite sign.

In 1860, Angström communicated to the Royal Society of Upsala a method of determining the motion of the solar system by observations on the bands of interference produced by a glass grating. In 1863, he published the results which he had obtained. After allowing for Babinet's correction on account of the motion of the grating, Angström finds that a difference in the direction of the observing telescope with reference to the earth's motion might produce a displacement of the fringes amounting to 49″.8. Selecting the line D in the fourth spectrum, he thought that the influence of the earth's annual motion was verified, but that of the motion of the solar system was less decided. The observations were more consistent with the assumption that the solar system moved with a velocity equal to one-third of that in its orbit, than with an equal velocity, or none at all. In 1862–'63, Babinet presented to the Academy of Paris a paper on the influence of the motion of the earth on the phenomena produced by gratings, which depend not on reflection, refraction, or diffraction, but on interference. His principal object was a study of the motion of the solar system. He calculated the effects to be expected, but published no observations. In 1867, Van der Willigen measured the length of waves of light by means of a grating. When a slit was used, no effect was produced by the motion of the earth, the slit partaking of that motion. With a star, a movement of the earth in the direction of the light had an effect. This is the theoretical result, and agrees with Babinet's experiment, but is not applicable to solar light when reflected by a mirror. That behaves as light from a terrestrial source. In 1873, he rejects the proposition that the refraction of light is modified by the motion of its source or of the prism. In 1874, he seems to doubt the reality of the effect produced on diffraction.

In 1867, Klinkerfues used a transit instrument having a focal length of 18 inches. In the tube was a column of water 8 inches long, and a prism. He observed transits of the sun and of certain stars whose north polar distance was equal to the sun's, and which passed the meridian at midnight. The difference of right ascension is affected by double the coefficient of aberration. He computed that the column of water and the prism would increase the aberration by 8″. The amount observed was 7″.1. In 1868–'69, Hoek of Amsterdam discussed the influence of the earth's motion on aberration. Delambre had calculated from the eclipses of Jupiter's satellites that light must take 493ˢ.2 in coming from the sun. Hence the aberration must be 20″.255. Struve's observed aberration made the time 497ˢ.8. Hoek decided in favor of Struve; but he thought that it was desirable that a new set of observations should be made on the eclipses. Klinkerfues espoused the side of Delambre. Hoek said that, if the earth's motion was taken into account, according to Fresnel's fraction, different results would be harmonized. In 1868, he made experiments on a divided beam of light, the two parts going in opposite directions through tubes filled with water. There was no interference attributable to the effect of the earth's motion. As to any influence to be expected from the motion of the solar system, he thinks that motion must be insignificant compared with the initial motion of the comets, and with the cometary orbits, which are parabolas with few hyperbolas.

In 1872, and on several previous occasions, one of the grand prizes of the Academy of Paris was offered for an investigation of the effect produced by the motion of the luminary or of the observer. This prize, consisting of a gold medal or 3,000 francs, was awarded in 1874 to Mascart. He maintained that in Arago's experiment the change in refraction produced by the fraction of the earth's motion was compensated by the displacement of the observing telescope. Mascart repeated Babinet's experiment with gratings, where the effects of the motion of the telescope and of the grating would be additive, and found the sum small compared with Babinet's calculation. He thinks that the change in the length of the wave caused by the motion is compensated by the displacement of the measuring apparatus. He concludes that reflection, diffraction, double refraction, and circular polarization are powerless to show the motion of the earth, either with solar light or that from a terrestrial source.

In 1871, Airy used a vertical telescope, and measured the meridional zenith distance of γ Draconis, the star by which Bradley discovered aberration. It is about 100″ north of the zenith. The tube of the telescope, which was 35.3 inches long, was filled with water. The days of observation included the seasons of the equinoxes, when the star is most affected in opposite directions by aberration. The observations were repeated in the spring and autumn of 1872. No increase was produced in the aberration by the water in the telescope.

In 1873, Ketteler, in the preface to the "Laws of the Aberration of Light," enumerates thirty-nine persons who have investigated the effect of motion on the phenomena of sound and light. From his own analysis he concludes: (1) that a motion of the prism and telescope perpendicular to the direction of a star produces no effect on the refraction; (2) that when the motion is in the direction of the star, the velocity of the light is changed according to Fresnel's fraction of that motion; and (3) that for any intermediate direction it is changed to the extent of that fractional part of the motion multiplied by the cosine of the angle between the direction of the motion and the direction of the star.

In 1859, Fizeau proposed an experiment for ascertaining if the azimuth of the plane of polarization of a refracted ray is influenced by the motion of the refracting medium. When a ray of polarized light passes through an inclined plate of glass, the plane of polarization is changed, according to certain laws investigated by Malus, Biot, and Brewster. The degree of change depends upon the inclination of the ray, the azimuth of the plane of primitive polarization, and the index of refraction of the glass. The incidence and azimuth being constant, this rotation of the plane of polarization increases with the index of refraction. This index being inversely as the velocity of light, the rotation is smaller the greater this velocity. Fizeau used two bundles of glass, four plates in each, and slightly prismatic, inclined to one another. One bundle was made of common glass; the other of flint glass. The angle of incidence for the ray was 58° 49′. When the azimuth of the primitive plane of polarization was 20°, the rotation of the plane of polarization was 18° 40′ and 24° 58′ for the two bundles. By Fresnel's hypothesis the change in the velocity of light from the motion of the medium is $\pm\left(\frac{\mu^2-1}{\mu^2}\right)v$. The greatest available velocity for the medium is that of the earth in its orbit, viz, 101,708 feet per second (31,000 meters). At the time of the solstices this motion is horizontal, and from east to west at noon. If the incident light comes from the west, the velocity of light is diminished by Fresnel's fraction of the velocity of the earth. If the light comes from the east, its velocity is increased by the same amount. The change in the index of refraction $\left(\text{or}\frac{\delta\mu}{\mu}\right)$ is equal to $\frac{v'}{v}(\mu^2-1)$; this for an index of 1.513 amounts to $\frac{1}{11740}$. Measurements show that in glass, the index increasing by a certain fraction, the rotation increases by a fraction four and one-half times greater, and the consequent change in the plane of polarization would be $\frac{1}{2500}$. The total change on reversing the direction from which the light came would be $\frac{1}{1250}$. If the incidence is 70°, and allowance is made for the change of direction inside of the glass, the fraction becomes $\frac{1}{1500}$. When a ray of light falls on a single plate of glass at an angle of 70°, if its plane of primitive polarization makes an angle of 20° with the plane of refraction, this plane is changed by 6° 40′. This multiplied by $\frac{1}{1500}$ gives sixteen seconds for the probable effect of the earth's motion. With forty such plates the effect would be increased to ten and two-third minutes. Two mirrors were used, one to the east and the other to the west, and light could be sent by a heliostat upon either one. The apparatus was easily turned through 180° so as to receive successively the light which travelled with or against the earth's motion.

With a single pile of plates highly inclined and a second pile less inclined, of more highly tempered glass and in the opposite azimuth, a rotation of 50° could be produced, while the tendencies to elliptical polarization were exactly balanced. The motion of the earth could modify this result to the extent of only two minutes; which is too small for accurate observation. Fizeau then resorted to a device already indicated by Botzenhart for amplifying this effect. A small variation in the primitive plane of polarization produces a greater effect the smaller the azimuth of this plane. If the original azimuth is only 5°, a small change in the azimuth trebles the value of the rotation. A large rotation is first produced on a ray whose azimuth is large, and then this rotation is largely changed by another pile so placed that the ray enters it under a small azimuth. More than two thousand measurements were made under various conditions. For noon observations at the time of solstice the rotation was always greater when the light came from the west, and was less at other times of day. The excess in the value of the rotation when the light came from the west varied between 30′ and 155′, according to the different ways in which the piles of plates were combined. The difference in the values of the rotation according as the light came from the west or east was consistent with a change in the index of refraction corresponding to Fresnel's hypothesis. Fizeau indicated his intention of renewing the research with improved apparatus, but no further publication on the subject by him can be found.

Faye has criticised this investigation of Fizeau, on the ground that he has taken no account of the motion of the solar system towards the constellation Hercules. This motion, recognized by astronomers on substantial evidence, amounts to 25,889 feet per second (7,894 meters) at its maximum. Its influence is almost zero at noon of the solstices. But it increases after noonday. Faye examines Fizeau's observations at 4 P. M., and finds discrepancies of 12′ or 15′ between the results of theory and observation. By neglecting the term which corresponds to the motion of the solar system, Fizeau's observations accord better at all hours of the day. Must the inference be, Faye asks, that the solar system does not move? Tessan, in reply to Faye, says that the sun, from which Fizeau derived the light used in his experiments, moves with the rest of the solar system; and that therefore Fizeau was justified in neglecting the term which expresses this motion, as of no effect on his calculations. Fizeau's theory depends only on the relative velocity between the source of light and the body which receives it; that is, the velocity of revolution and rotation of the earth.

In 1881, Professor Michelson published the results of his investigation on this delicate problem. He first calculates the probable difference of time taken by the light in going and returning over a given distance, according as that distance lies in the direction of the earth's motion or at right angles to it. If the distance were 1,200 millimeters, the difference of time translated into space would be equal to one-twenty-fifth of a wave-length of yellow light. The apparatus was ingeniously devised so as to bring about fringes of interference between the two rays which have travelled on rectangular paths. The whole apparatus was then turned round bodily through 90°, so as to exchange the conditions of the two interfering rays. Special apparatus was made for this experiment by Schmidt and Haensch of Berlin, and was mounted on a stone pier at the Physical Institute of Berlin. It was so sensitive to accidental vibrations that it could not be used in the day-time, nor indeed earlier than midnight. To secure greater stability the apparatus was moved to the Astrophysikalisches Observatorium in Potsdam, in charge of Professor Vogel. But even here the stone piers did not give sufficient protection against vibration. The apparatus was then placed in the cellar, the walls of which formed the foundation for an equatorial. But stamping with the feet, though at a distance of 100 meters, made the fringes disappear.

The experiments were made in April, 1881. At this time of the year, the earth's motion in its orbit coincides roughly with the motion of the solar system, viz, towards the constellation Hercules. This direction is inclined about 26° to the plane of the earth's equator, and a tangent to the earth's motion in its orbit makes an angle of 23½° with the plane of the equator. The resultant would be within 25° from the equator. The nearer the components are in magnitude, the more nearly would the resultant coincide with the equator. If the apparatus is placed so that the arms point north and east *at noon*, the eastern arm would coincide with the resultant motion of the earth, and the northern arm would be at a right angle to it. The displacement produced by revolving the whole through 90° should amount to one-twenty-fifth of the interval between two fringes. If the proper motion of the solar system is small compared with the velocity of the earth in its orbit, the displacement would be less. Mr. Michelson drew from these experiments the conclusion that there was not a sufficient displacement of the fringes to support the theory of aberration, which supposes the æther to move with a certain fraction of the earth's velocity. The displacement however was so small that it easily might have been masked by errors of experiment. Mr. A. Graham Bell supplied Mr. Michelson with the money required for this investigation,

In 1886, Mr. Michelson and Mr. Morley published a paper on the influence of the motion of the medium traversed by the light on its velocity. Fizeau had made similar experiments. In both cases the interfering rays were changed in velocity in opposite ways by flowing air or water through which they were transmitted. With air having a velocity of about 82 feet (25 meters) a second, the effect was so small that it might easily be covered up by errors of experiment; but with water it was measurable, and the result corresponded with the assumption of Fresnel, that the æther in a moving body is stationary, except the portions which are condensed around its particles. In this sense, it may be said that the æther is not affected by the motion of the medium which it permeates. For this investigation, which was made possible by a grant from the Bache Fund of the National Academy, Mr. Michelson and Mr. Morley devised a new instrument, called the refractometer. Cornu writes of Michelson's experiments on moving media: "Leur travail conçu dans l'esprit le plus élevé éxecuté avec ces puissant moyens d'action que les savants des États-Unis aimant à déployer dans les grandes questions scientifiques fait le plus grand honneur à leurs auteurs."

In 1887, Professor Michelson published another investigation of the question whether the motion of the earth in its orbit carried its æther with it. In his previous experiment his apparatus was sensitive to the smallest jars, and it was difficult to revolve it without producing distortion of the fringes, and an effect amounting to only one-twentieth of the distance between the fringes might easily be hidden by accidental errors of experiment. In the new experiment the apparatus was placed on a massive rock, which rested on a wooden base, which floated upon mercury. The stone was 1.5 meters square and 0.3 of a meter thick.

At each corner four mirrors were placed, by reflection from which the length of path traversed by the light was increased to ten times its former value. The width of the fringes of interference, which were the subject of observation, measured from forty to sixty divisions of the observing micrometer. The light came from an Argand burner sent through a lens. To prevent jars from stopping and starting, the float was kept constantly in slow circulation, revolving once in six minutes. Sixteen equidistant marks were made on the stationary frame-work within which the float moved. Observations were taken on the fringes whenever any one of these marks came in the range of the micrometer. The observations were made near noon and at 6 P. M. The noon and evening observations were plotted on separate curves. One division of the micrometer measured one-fiftieth of a wave-length. Mr. Michelson was confident that there was no displacement of the fringes exceeding one-hundredth of a wave length. It should have been from twenty to forty times greater than this. Mr. Michelson concludes that this result is in opposition to Fresnel's theory of aberration.

As late as 1872, Le Verrier thought that a new measurement of the velocity of light by Fizeau very important in the interest of astronomy; and in 1871, Cornu wrote that the parallax of the sun, and hence the size of the earth's orbit, were not yet known with the desirable precision. In 1875, Villarceau made a communication to the Paris Academy on the theory of aberration. He says that the parallax of the sun by astronomical measurement is 8″.86. Foucault's velocity of light combined with Struve's aberration makes the sun's parallax 8″.86. Cornu's velocity of light gives the same result only when it is combined with Bradley's aberration, which differs from that of Struve by 0″.20. Villarceau thinks that there is an uncertainty about the value of aberration on account of the motion of the solar system. In 1883, M. O. Struve discussed seven series of observations made by his father, Nyrén, and others, with various instruments and by different methods, at the Observatory of Pulkowa. He was certain that the mean result for the value of aberration was 20″.492, with a probable error of less than $\frac{1}{100}$ of a second. This aberration, combined with the velocity of light as deduced from the experiments of Cornu and Michelson, made the parallax of the sun 8″.784; differing from the most exact results of the geometric method by only a few hundredths of a second. Villarceau proposed to get the solar motion by aberration; selecting two places on the earth in latitude 35° 16′ north and south, and after the example of Struve, observing the zenith distances of stars near the zenith. The tangents of these latitudes are $\pm \frac{1}{\sqrt{2}}$ so that they contain the best stations for obtaining the constant of aberration, and the three components of the motion of translation of the solar system. In 1887, Ubaghs, a Belgian astronomer, published his results on the determination of the direction and velocity of the movement of the solar system through space. For finding the direction he used the method of Folie. For calculating the velocity he combined the observations on three groups of stars, the brightest belonging probably to the solar nebula. The resulting velocity was only about 10,000,000 miles a year. Homann, working on the spectroscopic observations at Greenwich, had obtained a velocity of 527,000,000 of miles. As late as 1887, Fizeau studied the nature of the phenomena when light was reflected from a mirror moving with a great velocity, and inferred that aberration was the same in this case as when the light was taken directly from a star.

The solar parallax, calculated from Cornu's last experiment on the velocity of light and Delambre's equation of light (493″.2 being the time for passing over the radius of the earth's orbit)	=8″.878
From Struve's observed aberration	8″.797
From Bradley's observed aberration	8″.881
From Foucault's velocity with Struve's aberration	8″.860
From Le Verrier's latitudes of Venus by transits	8″.853
From meridian observations of Venus during 106 years	8″.859
From occultations of χ Aquarius in 1672	8″.866

Glasenapp calculated the time taken by the light in travelling the mean distance of the earth's orbit as equal to $500^{\text{s}}.85 \pm 1.02$. This time combined with Michelson's velocity of light makes the solar parallax 8″.76. Struve's constant of aberration with Michelson's velocity gives a parallax of 8″.81. From Gill's mean of the nine best modern determinations of aberration (=20″.496) the parallax comes out equal to 8″.78. If we regard the solar parallax as known, the eclipses give nearly the same velocity as aberration, though the former is a group-velocity and the latter a wave-velocity. Gill's parallax from observations of Mars (8″.78) agrees with Michelson's velocity of light and the mean constant of aberration.

In 1877–'78, Lord Rayleigh, in his profound treatise on the Theory of Sound, discussed the distinction between wave-velocity and group-velocity. In 1881, he recognized the same difference in the case of luminous waves. In the experiments of Young and Forbes, the wave-velocity might be nearly three per cent. less than the group-velocity. With toothed wheels and the revolving mirror, group-velocity was the subject of observation. Aberration gave wave-velocity; Jupiter's satellites, group-velocity; experiment however showed but little difference. Lord Rayleigh found formulæ for the relation between these two kinds of velocity, which involved the wave-length and the idex of refraction, and J. Willard Gibbs has compared them, and other formulæ proposed by Schuster and Gouy, with the experimental velocities of light. Michelson's experiment on the index of refraction of carbon disulphide agrees with the assumption that he was dealing with the group-velocity.

Although there is not a complete accordance between the results of different methods of investigation, astronomers and physicists will be slow to abandon the theory of undulations, and take up again the corpuscular theory of light. The latter theory has received fatal blows from which it cannot recover. The undulatory theory, which started with Huyghens more than two hundred years ago, and was elaborated by Fresnel sixty years ago, has survived many crises in its history, and is supported by a wonderful array of experiments. Some of the experiments of Mr. Michelson may require a modification in Fresnel's interpretation. Stokes and Challis have worked for many years upon it, and established it on mathematical principles differing from Fresnel's and from each other. Ketteler in his *Theoretische Optik*, published in 1885, builds upon the Sellmeier hypothesis, that ponderable particles are excited by the ætherial vibrations and then re act upon them. There remains Maxwell's electro-magnetic theory of light, which has been elaborated by Glazebrook and Fitzgerald, and is supported, to say the least of it, by remarkable numerical coincidences.

Discrepancies between theory and experiment are always to be welcomed, as they contain the germs of future discoveries. We have learned in astronomy not to be alarmed by them. More than once the law of gravitation has been put on trial, resulting in a new discovery or in improved mathematical analysis. We may not expect in light such a brilliant discovery as that of the planet Neptune. The luminiferous æther is a mysterious substance, enough of a fluid for the planets to pass easily through it, but at the same time enough of a solid to admit of transverse vibrations. Stokes suggests water with a little glue dissolved in it as a coarse representation of what is required of the æther.

Mr. G. A. Hirn has written recently on the constitution of celestial space. He decides against the existence of an all-pervading medium. He thinks that matter exists in space only in the condition of distinct bodies, such as stars, planets, satellites, and meteorites. In nebulæ it is in a state of extreme diffusion; but elsewhere space is empty. But how would it be after the correction is applied for the equation of light? Humboldt said that the light of distant stars reaches us as a voice from the past. The astronomer is not seeing for the most part contemporaneous events. He is reading history; and often ancient history, and of very different dates. Stellar photography reveals millions of stars which cannot be seen in the largest telescopes, and new harvests of these blossoms of heaven (as they have been called) spring up like the grass in the night. Numbers fail to express their probable distances and the time taken by their light in coming to the earth. In the theogony of Hesiod, the brazen anvil took only nine days in falling from heaven to earth. On the other hand, the reduction of the sun's distance by three per cent. not only affects its mass and heat, but it changes the unit of measure for the universe. Such are the remote results of any change in the estimated velocity of light.

We may thank Professor Michelson not only for what he has established, but also for what he has unsettled. In his various researches, which I have hastily sketched, but which require diagrams or models to be clearly understood, he has displayed high intelligence, great experimental skill and ingenuity, and unflagging perseverance. With a full appreciation of his work, the Rumford Committee recommended, and the Academy voted, that the Rumford Premium be awarded to him.

PHOTOGRAPHY IN THE SERVICE OF ASTRONOMY.*

By R. RADAU.

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To obtain the greatest result with the least effort, is not this the whole problem of modern industry, a problem which determines gradually the development of implements and machines? The engines which he invents permit man to infinitely multiply the efficiency of his organs, to extend their fitness, and to relieve them from the demands of excessive efforts; they assist him, free him, more and more from the harsh servitude of material labor. Confining himself henceforth to overseeing the apparatus which labors for him, in proportion as he fatigues himself less, he produces more and at much better advantage. There is no comparison between the fabrication of a thousand needles from a manufactory, and the work of an artisan who undertakes fashioning them one by one by his own unaided efforts.

It is a progress of the same order which realizes to-day the definitive introduction of photography in astronomical observations: It is to deliver the astronomer from a labor, thankless, painful, irksome, and fatal for the eyes. When ten years ago I spoke in this journal of the great prospects of celestial photography † I dared scarcely to hope that routine and prejudice would be disarmed so speedily. Indeed the first attempts in astronomical photography go as far back as 1840, and during nearly half a century frequent attempts, unhappily always isolated, have shown that the difficulties of the problem have not been insoluble; but on account of tenacious prejudices, a blind adherence to the past proscribed the paraphernalia of photography from the sanctuaries or kept up the traditions of Cassini and of Bradley. It is in these later years that finally this spontaneous enthusiasm appeared, this grand movement which has found its expression in the "Astro-photographic Congress," convened in Paris in the month of April, 1887, and which promises to begin a work of the highest importance for future ages, the photographic execution of a general chart of the sky.

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* From the *Revue des Deux Mondes*; April 1, 1889, vol. XCII, pp. 626–649. I.—E. Mouchez, "Astronomical Photography at the Paris Observatory and the chart of the sky," Paris, 1887. II.—Bulletin of the Permanent International Committee for the execution of the photographic chart of the sky, 1888, 1889.

† *Revue des Deux Mondes* of February 15, 1878.

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This application of photography is however so reasonable, its rôle was so clearly indicated and so well foreseen, that it seems an equal progress ought to have been obtained indeed from the first. There was one problem whose solution was perfectly circumscribed; it was truly no more than a question of time and money. The history of photography, since its origin, is like the logical development of a thought which is realized in a continuous manner before our eyes. The gropings by which we discover substances more and more sensitive, or the means of retaining, of fixing, more and more permanently, the fugitive traces of phenomena, all these ought to be, most certainly, hastened and matured more speedily in the fact of having a prize to gain; and here appear clearly the conditions always more tyrannical than expense in the scientific enterprises of our epoch.

Chemistry and the mechanical arts have singularly multiplied the resources of astronomers of the close of this century. Is there any occasion to recall the progress accomplished in the manufacture and grinding of optical glass, in the mounting of great telescopes, in silvered mirrors, in electric chronographs, in the spectroscope and in spectral analysis, whose entry on the scene, so brilliant and so unexpected, probably diverted for some time the attention of astronomers from the development of photographic processes? Unhappily this instrument, so powerful, this new apparatus which has extended the domain of observation, is very costly. In order to bring it into service, great efforts of eloquence are almost always necessary, because the scientific budget, as is well known, is one whose endowment is generally measured with the greatest parsimony. It is in such a situation as this that the assembly of a congress, with its solemn publicity, its persuasive programmes, and its imperious desires, offers always the best means of overcoming an opposition which is inspired by an ill-conceived economy.

The congress which held its sessions at the Paris Observatory, two years since, and which was called by Admiral Mouchez, under the auspices of the Academy of Sciences, had in view, primarily, the execution of a chart of the sky. It comprised fifty astronomers, who came from all parts of the globe, some already familiarized for a long time with the pratique of celestial photography.

It would be irksome to enumerate here even once, all the attempts which have been made, since Daguerre, to bring photography into the service of descriptive astronomy and the astronomy of precision. Recalling only that the most difficult part of the problem, the photographic re-production of stars, had been entered upon with some success in America by G. P. Bond (soon after the introduction of the collodion process permitted the shortening of the time of exposure), about 1857, the photography of stars to the sixth or seventh magnitude had been attained. These trials were repeated some years later in England by

Warren de La Rue, and later in America, with a success continually increasing, by Mr. Rutherfurd and by Mr. B. A. Gould, charged with the direction of the Cordoba Observatory under the fine sky of the Argentine Republic. Gould began his work in this line about 1875 and succeeded in gathering, in a few years, a collection of more than a thousand stellar photographs of the highest interest. After having tested, himself, the slowness of the wet collodion process, he was able at a later date to utilize bromide of silver gelatine dry plates, the invention of which marked a new phase in celestial photography.* It is necessary, finally, to mention here the attempts in stellar photography of Henry Draper, of Ainslie Common, and Isaac Roberts, who have studied the respective advantages of refractors and reflectors with silvered mirrors; of Pickering, who has constructed for the Harvard College observatory at Cambridge, United States of America, a photographic equatorial specially designed for the rapid execution of celestial charts on a moderate scale; of David Gill, the eminent director of the observatory of the Cape of Good Hope, who commenced in 1885 a photographic revision of the southern sky, comprising the stars to the ninth or tenth magnitudes, similar to the catalogue prepared by Argelander for the northern sky.

At the Paris Observatory like labors have been also pursued for some years with most marked success. Paul and Prosper Henry had undertaken, in 1871, to continue the "ecliptic chart" commenced by Chacornac who had been able to execute it only in part. This chart, extremely useful in searching for small planets, which was to contain all stars to the thirteenth and fourteenth magnitudes, extended along the ecliptic in a zone five degrees in breadth. Now at a certain point the Henry brothers found themselves arrested in this work by the manifest impossibility of constructing, by old processes, the sections of the charts where the swarming of the stars announces the approach of the Milky Way. It was then that they decided to resort to photography. They were, says Admiral Mouchez, admirably prepared to conquer these difficulties. "Following the traditions to-day too much forsaken, of great astronomers of former times who employed their own hands in the construction of their instruments, they devoted for a long time, in their modest work-shop of Montrouge, all the moments of liberty which were left them from their very active service at the Paris observatory, in the study of the grinding and polishing of optical glass. An extensive acquaintance with the questions for solution, the harmony of fitness somewhat different and very happily associated in two brothers, an energetic will and a persevering labor, which no distraction ever chanced to trouble, could not fail in assuring for them a well-merited success. They became, in a few years, the most skillful artists of France, and their fame was no less great among foreigners." After having constructed, by way of trial, an objective of 0.16^m, which gave very good results, the Henry brothers undertook to execute the optical part of a definitive apparatus of 0.33^m in aperture, for which Mr. Gautier was to furnish the mechanical part. The new instrument was mounted at the observatory in 1885 and has not ceased since then to be in active use. The sensitiveness of the plates is such that the image of a star of the first magnitude is obtained in less than a hundredth of a second; that of a star of the sixth magnitude in a half second; for the tenth magnitude the length of exposure is about twenty seconds; for the fifteenth about thirty-three minutes; for the sixteenth one hour and twenty minutes is necessary.* The stars of the sixteenth magnitude! Here we are already far beyond the limits of visibility for the best telescopes under the sky of Paris! "Says Mouchez, even stars of the seventeenth magnitude have been certainly obtained, which without doubt have never before been seen." Finally the Paris plates have revealed the existence of nebulæ hitherto unknown in the regions which had been often explored with the aid of the most powerful instruments; such is the nebula of Maïa, in the Pleiades, whose presence has been since directly verified.

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* Rayet: "Notes on the history of astronomical photography." (*Bulletin astronomique* t. IV, p. 318.)

After such success we understand that the director of the observatory has not hesitated in taking the initiative in an international agreement on the subject of the execution of the complete chart of the sky, by the means of photography. The possibility of this considerable work being to-day fully demonstrated, he has said to astronomers, we have assumed, for the science of the future, the trust of going about it without delay; whatever may be the value of the works in course of execution in the various observatories, they will never have, for the astronomers of future ages, an importance comparable with that of this general inventory which we shall be able to bequeath to them. It is besides, indispensable for us to concert together and distribute the labor and conclude upon a plan of work, in order to avoid the waste of force, the gaps, and the useless repetitions, and the result will be a work truly homogeneous. In regard to the expense which the enterprise will involve, it will be without doubt large enough necessarily, but very inconsiderable relatively to the importance of the result.

The astro-photographic congress convened in Paris, as we have said, in the month of April, 1887; sixteen nations were there represented. At the commencement certain technical questions were settled; the employment of reflecting telescopes, in spite of the advantages which they offer in some connections, has been rejected for the execution of the chart of the sky, and a unanimous vote recommended refracting telescopes; they will be constructed similar to the photographic telescope of the Paris observatory. In regard to the limit of the magnitudes of the stars to be photographed there was some difficulty in coming to an agreement. Taking into consideration the notable difference in the length of exposure necessary for bright stars and very faint stars, it was finally decided to make two classes of plates designed for two different uses.

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* We take this information from the notice of Admiral Mouchez, which dates from 1887, but these times of exposure are already much shortened by employing more sensitive plates, such as the American plates which Pickering uses, and which the Henrys have tested in their turn.

For the double series of plates devoted to the picture of the sky, which is to comprise the stars to the fourteenth magnitude, the length of the exposure will be (in the climate of Paris at least) in the vicinity of twelve minutes.* For the supplementary series of plates comprising the stars to the eleventh magnitude only, and which must, on the one hand, secure an extreme precision in the micrometrical measurement of the stars of reference, and on the other hand, furnish the elements for a catalogue, the length of exposure will be much shorter (about thirty-five to forty seconds). This catalogue will probably contain one and one-half million stars,—more than double the number that are certainly known to-day. In regard to the number of stars which will be found represented upon the chart properly so called, it may be estimated to be from ten to fifteen millions. The two series of plates which will serve for constructing the chart will be arranged in such a way that the image of a star, situated in the corner of one plate of the first series, will be found as near as possible in the center of a plate of the second series; it is hoped that this will suffice for eliminating false stars and remove the inconvenience of unsensitized points which must exist on the plates.

In adopting for the chart an exposure of thirty minutes, it would be possible to reach the fifteenth magnitude and obtain a double or triple number of stars, perhaps thirty or forty millions, and possibly more. This was what several members of the congress desired, who could only with reluctance decide to curtail thus the common work of the astronomers of the nineteenth century. Mouchez, notably, has made the remark that the limit to which we are confined is very near that of the asteroids which are discovered every day; to obtain appreciable traces of these small stars, the exposure of twelve minutes runs the risk of being insufficient. Those who have combated the extension of the survey beyond the fourteenth magnitude have pleaded, in the first place, the length of time which the completion of the chart, under these conditions, would demand. In reply to them it may be said that in fixing at 14,000 † the total number of plates necessary for the execution of the chart, and in supposing that the work will be distributed among fifteen or twenty observatories each observatory will have only 1,000 plates to furnish; in counting twelve minutes to each plate, the work would easily be accomplished in one or two years; four years would suffice, in adopting a length of exposure of thirty minutes in order to reach the fifteenth magnitude.

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*Perhaps also much less with the more sensitive plates.

† In counting 6 square degrees to a plate, 7,000 will be needed to cover the sky, and 14,000 with the duplicates.

Another objection, perhaps more serious, is drawn from the impossibility of utilizing such a super-abundance of material. What will you do, said David Gill, with the images of so many millions of stars when once you have obtained them? Where will you find enough astronomers to make use of them? We are not in the floating island of Laputa, where all men are exclusively occupied with mathematics so that it is always necessary to strike them on the head with a bag containing dry peas to awaken them. These remarks under their playful form are very just. The answer to this was that the future would devise, without doubt, processes of study more rapid than ours, and that it would not be necessary to deprive our successors of treasures which cost us so little to bequeath to them. In any case, Mouchez said, we would always consider these plates as documents to consult, without being compelled to study them in their smallest details, in the same manner as we possess a library or encyclopedia, not for the reading of all their volumes from one end to the other, but for searching there in a given juncture for the needed information.

However, if we wish to compute the amount of labor which would be necessary to utilize the data which the enterprise will furnish, limited as it is by the resolutions of the congress, it will be found perhaps that there was wisdom in not wishing to comprehend too much. There is nothing evidently to prevent the enlargement of the scale of the enterprise at a later date, in ten or twenty years; up to that time it may be said that in limiting it the chances of success will be particularly increased.

The congress of 1887, before adjournment, constituted a permanent committee charged with securing the execution of its decisions, of centralizing the accounts, and of maintaining the associated observatories in continued correspondence. This committee, in its turn, has formed a bureau of nine members,* which has already commenced the publication of a special bulletin, designed to keep astronomers constantly advised of the state of advancement of the preparatory labors, the necessity of which the congress had recognized. The committee will meet in Paris the 15th of next September.

The number of observatories which have promised to take part in the making of a chart of the sky, and who have already ordered their photographic telescope, is at present sixteen. These are, outside of the French observatories (Paris, Bordeaux, Toulouse, Algiers), those of the Cape of Good Hope (Africa), Potsdam (Germany), Oxford and Greenwich (England), Melbourne and Sydney (Australia), Helsingfors (Russia), San Fernando (Spain), Santiago (Chili), Rio de Janeiro (Brazil), Tacubaya (Mexico), La Plata (Argentine Republic). The Royal Society of London contemplates establishing an observatory in New Zealand; others, as those of Harvard College, Meudon, Poulkova, and Leyden, will contribute actively in special researches for the advancement of the common work. These relate to the following: The preparation of reticules whose image impressed on the plate can furnish the standard for micrometrical measures, and permit to recognize deformations of the sensitive film; it is necessary to make a preliminary study of the scale of photographic magnitudes of stars; to consider the means of determining the optical distortion of the field of the telescope; to study the method of measuring and reducing the plates, etc. It is of much importance to thus clear up the ground before commencing the execution of the chart, in order not to be retarded afterwards by unforeseen obstacles. We can now attempt to gain an idea of the expense which the projected enterprise will involve. The cost of the construction of a photographic refracting telescope is estimated at from 50,000 to 60,000 francs; the fifteen or sixteen telescopes which will be needed will cost, then, nearly 1,000,000 francs.

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* President, Mouchez; members, Christie, Duner, Janssen, Struve; secretaries, Gill, Loewy, Vogel.

In adding to this sum the price of the plates, and for each observatory, the appointment of at least two operators during two years, we arrive at a total in the neighborhood of 1,500,000 francs. It is true that the instruments remain the property of the establishments which ordered their construction and that the work could be confined to the existing personnel. But the execution of the photographs is not the most costly part of the enterprise. David Gill, in a memoir inserted in the first fascicule of the *Bulletin of the International Permanent Committee*, has elaborated a detailed plan of organization of the office work which ought to be accomplished in view of the publication of the results, and which will consist, before all, in the measure and reduction of the plates designed for the formation of catalogue. This work, of a nature so special, says Mr. Gill, requires so perfect an experience and so skillful an organization, in order to be conducted to a successful termination without too much cost, that it will be most necessarily in charge of a central bureau. Under these conditions this will be an outlay to be provided for.

Mr. Gill supposes that the plates of the catalogue will be made, as those of the chart, in duplicate, and that each plate will cover four degrees square, so that the total number of plates will amount to about twenty thousand. The labor of measuring and reducing must be done under the direction of an energetic and skillful chief, by young persons of both sexes of average intelligence; not less than thirty will be needed, and the entire completion of the work will demand from seventeen to twenty-five years. The publication of the catalogue will keep up with the calculations. For the chart of the sky, so called, it will suffice to issue to *subscribers*, namely, to observatories, societies, or nations who have relations with the bureau, positives on glass, obtained by means of the original negatives. These copies will be executed by a photographer assisted by two aids. After detailed estimate of the outlay, which will result from this organization, Mr. Gill thinks that the budget of the central bureau ought to be fixed at a minimum of 200,000 francs a year, but probably it will be necessary to put it at 250,000 francs. The total outlay will thus rise to a little more than 6,000,000 francs. This is the sum which appears necessary to secure the publication of the catalogue of all the stars to the eleventh magnitude, and that of the photographic re-production of all the stars to the fourteenth magnitude, beside the cost of the telescopes, the salaries of the astronomers, etc., expenses which we have already estimated in the lump at 1,500,000 francs. It would be reasonable to subtract from this the return from the sale of copies of the chart, which will yield, after Mr. Gill, about 1,000,000 francs in twenty-five years, or enough to pay for the telescopes.

Is this amount of 6,000,000 or 7,000,000 francs, to which we have thus definitely arrived, exorbitant if we take into account the importance of the results which we are concerned in obtaining? It appears, on the contrary, a trifling price in comparison with what it would be necessary to expend in order to arrive at the same results by the old processes. In the present state of astronomy the formation of a catalogue comprising all the stars to the eleventh magnitude (which is the practical limit of comparison stars in current observations, with the ordinary observatory instruments) must be considered as an absolute necessity. Now we know by experience, says Mr. Gill, that the cost of a single exact meridian observation of a star (comprising the cost of reduction and publication) is never less than 10 francs, and often surpasses this figure. The catalogue which it is proposed to form with the aid of photography will comprise nearly two millions of stars, each of which will have been determined two times in turn. To obtain the same number of independent positions from meridian observations (supposing that sufficiently powerful meridian instruments are found to furnish them) it would be necessary evidently to spend about 50,000,000 francs. This is eight times more than the cost of the photographic catalogue and the general chart of the sky. In regard to the precision of the photographic positions, it will be superior to that of direct observations. It suffices in this regard to mention the remarkable results which Mr. Thiele, director of the Copenhagen observatory, has obtained by micrometric measures executed upon three plates of a star cluster which had been communicated to him by Messrs. Henry.

It is necessary to say here a few words on the appearance which the photographic images of stars present. These images, upon the plates, have the form of small black disks, of a diameter nearly proportional to the stellar magnitude, such as is figured upon celestial charts; their dimensions increase gradually according as we prolong the exposure, which it may be said in passing is a somewhat serious obstacle in photometric researches, for the experiments of Mr. Scheiner have shown that the augmentation does not follow a simple law. Under the microscope* these round black spots are resolved into a multitude of black points, very crowded at the center, for the stars of the first ten magnitudes, more and more thinly distributed for the fainter stars, down to the doubtful traces which mark the extreme limit of chemical sensitiveness. At present this limit is much further removed than that of the penetration of the eye armed with a telescope. The dotted character of the images proceeds evidently from the action of light upon the molecules of the salt of silver incorporated in the sensitive film. These photographic stars resemble thus clusters of stars, or nebulæ more or less resolvable.

This aspect is so characteristic that there is little risk of confounding very small stars with accidental spots, as was feared at first, and it follows that a duplication of exposures may often be dispensed with. Messrs. Henry, to avoid all confusion, have confined themselves to repeating three times the exposures on the same plate, by displacing the telescope each time in such a way as to form with each star a small equilateral triangle of 3 to 4 seconds on a side. This triangular appearance is not at all perceptible except with a lens; the paper prints give images which appear perfectly round. A subsidiary advantage of this mode of operating is that it thus becomes possible to remove farther yet the limit of visibility of the stars; thus may be established very easily in this manner the presence of an unknown planet, whose proper motion would deform the microscopic triangle. But it is clear that the triple exposure involves a great loss of time. The congress has preferred for the execution of the chart of the sky, as we have seen, two parallel and independent series of plates.

The plates will not acquaint us with the absolute positions of the stars; they will only permit us to determine their relative situations. It is also necessary, in order to obtain these with the desired precision, to provide a system of standards. These standards will be procured by the re-production of the reticules Mr. Vogel prepares for this object, and which are traced with a steel point on plates of silvered glass; placed upon the sensitive plate, the reticule leaves there a latent image, which developed later appears under the form of a system of very definite lines of reference. These standard reticules are not only of great assistance in micrometrical measures of stellar positions, but they will serve to control the deformation of the gelatine film. We know that for collodion the shrinkage and the deformation of the image which it entails can attain an amount very sensible; it is no greater with gelatine, which adheres very strongly to the glass. This is less according to the micrometric comparisons of an original reticule and several photographic copies, which Mr. Scheiner has recently undertaken; but we cannot answer for the invariability of the plate in each particular case without a special verification; it is particularly necessary to look out for deformations when the copies have been executed with a pencil of slightly convergent light.

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* In the absence of a lens a simple card pierced with a small hole which is held before the eye may be used to examine these images; it is a primitive lens.

II.

The truly invaluable advantage of this intervention of photo-chemistry in the processes of practical astronomy, is that in transporting (so to speak) an authentic image of the firmament into the study of the astronomer, it frees him from obstacles without number which have so long a time trammeled researches the most delicate; the cost of creation and maintenance of an observatory, the difficult handling of great instruments, fatiguing nightly vigils, fogs, and clouds which so often put a stop to observations, the necessity of changing one's hemisphere in order to study certain constellations, etc. Armed with a simple micrometer, he can henceforward explore collections of photographic plates, taken with some years of interval, and make, in his chimney-corner, discoveries which otherwise would demand long struggles, continued during several generations, against the capricious inclemency of the sky.

Indeed celebrated labors come to our mind which have cost in former times long efforts which we shall have no more to renew. These are, firstly, the gauges of the number of stars which William Herschel undertook, a century ago, with the 20-foot telescope after a plan traced by Wright. We know that, setting out with the hypothesis of a nearly uniform distribution of the stars, he admitted for a long time that the relative richness of a region indicated the depth of the heavens in the direction considered, which must conduct to attributing to the visible universe a structure tolerably improbable. Later he changed his method, and occupied himself with sounding the celestial spaces with telescopes more and more powerful, in taking henceforth for a criterion of distances the resolvability of clusters or groups of stars. The two methods are incorrect, in confounding, with the effects of perspective, the inequalities of constitution of different stellar regions, indications of which indeed make us suspect the reality. But however in reserving the conclusions which may be drawn from the gauges or soundings of the sidereal system, it will be necessary sooner or later to return to this grand statistical work, and the photographic chart will singularly tend to facilitate the task of astronomers who will be charged with it.

Shall we speak of catalogues of stars? The most ancient, those of Hipparchus, Ulugh Beigh, Tycho-Brahé, containing a thousand stars; they were made without a telescope. The catalogue, so precious, which Bessel has derived from the observations of Bradley (made at Greenwich about the middle of the last century), and which has, so to speak, inaugurated the astronomy of precision, contained only a little more than 3,000. That which is founded upon the observations of Lalande, executed towards the close of the century at the observatory of the military school, and published, in 1801, in the French Histoire Celeste (the catalogue was not published till 1847), comprises more than 47,000 stars. The Paris observatory has devoted itself, for a long time, to determining them anew with the greatest care, for the purpose of forming a new catalogue, which is being gradually accomplished under the skillful direction of Mr. Gaillot. The first two volumes, comprising 7,245 stars, appeared in 1887, and this brings to view the astonishing precision to which Lalande and his co-workers attained with instruments on the whole very defective.

The catalogue which Weisse has deduced from the "zones" of Bessel contains about 62,000 stars. That of Argelander, founded on the "northern zones" observed at Bonn, contain 324,000, to which Mr. Schoenfeld, the successor of Argelander, has recently added more than 133,000 stars, derived from his "southern zones." The Bonn zones furnish positions, rapidly determined, of stars of the northern sky and of a part of the southern sky, to the ninth or tenth magnitude. We have already said that Mr. Gill has undertaken, at the Cape of Good Hope, to complete this inventory for the remainder of the southern sky, by the means of photography; we have besides also now, for this part of the sky, the zones which Mr. Gould observed at Cordoba (Argentine Republic). To this must be added that in 1867, the International Astronomical Society has taken the initiative in a general revision of the Bonn zones which was distributed among fifteen observatories, and which will furnish the material for a new catalogue. The matters concerned here are careful summaries which do not admit of a very great precision in the observed places; for the stars more brilliant, which do not surpass the eighth magnitude and which are less numerous, we possess a series of catalogues prepared with more rigor and founded upon the mean of frequently repeated observations. It is from these astronomers derive the fundamental stars, stars of reference to which others are referred in order to correct their absolute positions. These vast works, which have cost so much effort and employed so many human lives, is it necessary to believe that they will lose their value when the great photographic chart shall be completed? We do not think thus. Not only the catalogues of high precision, founded upon meridian observations, will remain indispensable for the exact determination of absolute positions; but the zone catalogues will serve to control the relative positions of the stars determined by photography.

The comparison of plates taken at two different epochs will permit the undertaking upon a vast scale of the research of proper motions, which at present can be entered upon only for some thousands of stars. These small progressive displacements which, in the mean, do not exceed one-tenth of a second in the space of a year (in some cases it attains to seven or eight seconds a year) proceed only in part from real movements of the stars, which are thus seen to change in position. These are, in a certain degree, apparent displacements which have for a cause the movement of translation of the solar system, and which permit the determination of its velocity and direction.*

Sometimes the progression, instead of being uniform and continuous, is found to be affected with periodic inequalities which reveal the existence of an annual parallax, that is to say, a sensible effect produced by the change of position of the observer when the earth passes from one extremity to the other of its orbit; the oscillation in the apparent place of a star, which results from it permits the calculation of the distance which separates it from our system. Or indeed the inequality presents a longer period, and the successive positions of the star permit the discovery of an orbit which it describes around a neighboring center of attraction. We deal here with a physical couple; optical couples, where the nearness of position is only the effect of perspective, present independent proper motions.

Researches of this sort will without doubt be facilitated by the application of photography, for the determination of the relative positions upon the negative will be infinitely more convenient than in the field of the telescope, especially when a comparison is desirable between stars of very different brilliancy. In certain cases, indeed, photography offers the only means of obtaining precise measures; how would we attempt directly the measurement of the distances and position angles in a mass of stars such as the cluster in Hercules? Upon the negative this cluster forms a small diffuse spot 2 to 3 millimeters in size; on examining it with a lens we distingush several hundred stars dispersed around a nucleus of a pulverulent appearance, which we may proceed to without doubt resolve in its turn into a multitude of luminous points. We have never attempted to design these groupings, still less to make direct micrometric measures, the eye being dazzled, says Mr. Mouchez, by what appears in the eye-piece as a mass of innumerable and brilliant grains of dust; but the negative placed under the microscope will permit us to draw without difficulty the exact chart of this wonderful corner in the sky. In transmitting it to posterity we will give to our descendants the means of verifying the evolutions, which without doubt are slowly accomplished in the bosom of this agglomeration of suns.

The research of the annual parallax, which permits us to measure the distance of the stars by taking for a base of operations the diameter of the terrestrial orbit constitutes one of the most delicate problems of modern astronomy, for the displacements, which it concerns us to verify, never surpass a few tenths of a second, and are oftener masked errors of observation, whence the irritating discordance of successive determinations of the same parallax effected by astronomers equally skillful with the most perfect instruments. Will photography be more fortunate? Mr. Pritchard at Oxford has attempted to utilize 61 Cygni in the first place for the verification of the parallax of a double star very often observed. According to Bessel and Peters, this parallax scarcely surpasses a third of a second; according to Otto Struve and Anwers, it attains a half second (0″. 52). Mr. Pritchard has photographed the double star in question, during the year with four comparison stars disposed symmetrically,—two in the direction of the components and two in the perpendicular direction. The micrometric measures have given him for each of the two components a parallax of 0″.43, which indicates a distance equal to about 500,000 times that of the sun, a distance over which light leaps in seven and one half years. It has been necessary, however, to reject some negatives on account of accidental deformations of the sensitive film occurring during development. In order to free ones' self from this source of error, it is only necessary to employ plates impressed with a reticule of reference, in accordance with the advice of Mr. Lohse. Since last year Mr. Pritchard has simplified his process of research in confining himself to observing each star during five nights in each of the four periods of the year indicated by the parallactic ellipse, which the star seems to describe in the sky; in this way he hopes to be able to determine the parallaxes of ten to fifteen stars a year. He has commenced the work on several stars of the constellations Cassiopea and Cygnus, whose parallaxes appear to be comprised between 0″.04 and 0″.19. For Polaris Mr. Pritchard has found 0″.07; that is to say, that Polaris is three millions times more distant from us than the sun.

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* See in the *Revue des Deux Mondes* of October 1, 1875, "The Progress of Stellar Astronomy."

It is admitted generally that the most brilliant stars are also the nearest to us; however, among the parallaxes which are known to be sensible up to the present time many belong to stars relatively faint, and nothing prevents supposing that in the number of stars which have not been examined and which will soon be catalogued by photography there will be found those which are even much nearer to our solar system. However, they could not delay being disclosed, since the simple microscopic inspection of the same group photographed at six months' interval would suffice to reveal sensible parallactic displacements however small.

The direct micrometric measures of groups of stars reveal to us only displacements in the direction perpendicular to the visual ray; the spectroscope alone can make us acquainted with movements which take place in a direction the same as the visual ray. For the color of light which comes to us from a star, is slightly modified by the velocity with which the star approaches or removes itself from us, and it follows that rays of the spectrum are deviated a little towards the right or towards the left. (It is for the same reason that the locomotive-whistle seems to us sharper in pitch when the train is approaching than when it is receding.) It is possible by this means to estimate the velocity of translation of a certain number of bright stars whose spectra are not too difficult to observe. However, the eye is fatigued in comparing with the motionless rays of an artificial spectrum the always trembling lines of the stellar spectra, and the deviations thus established rest most frequently upon impressions fading and very uncertain. Mr. Vogel has succeeded in freeing himself from this difficulty, caused by the scintillations, by photographing the stellar spectra at the same time with the spectrum of a gas; the plates which he has published show with a surprising sharpness the deviation of a ray common to several stellar spectra, and which corresponds to the violet ray of hydrogen. It is established thus for example that a certain star of the constellation of Orion recedes from the observer with the velocity of 86 kilometers per second, a velocity which is reduced to 61 kilometers if it is referred to the sun.

At Greenwich, where the spectroscopic study of the velocity of translation of stars has been pursued for fifteen years by the process of primitive observation, contradictory results appear, which proceed without doubt from the small amount of fixity of the images of stellar spectra.* There is reason to hope that the photographic method, in causing this source of error to disappear, will permit the making use of data of this character on the same ground as the proper motions, perpendicular to the visual ray, which modify the apparent positions of the stars. The attempt has already been made to deduce from them the direction and the velocity of the movement of translation of the solar system, and the results agree very well with those which have been obtained by other methods †. Finally, these are the only data for the present which we can make use of in arriving at a more complete knowledge of the orbits of double stars, for the usual observations reveal to us only the apparent orbits in the way they are projected on the celestial sphere. These projections are ellipses, and it is more than probable that the real orbits which we see foreshortened are equally ellipses; but it is not rigorously demonstrated that the principal star occupies one of the foci.

It follows that it can not yet be affirmed in an absolute manner that the law of Newton, the law of universal gravitation, presides also over the motions of double stars, although the generality of this law is extremely probable.‡

The photographic study of stellar spectra is also of a high interest from other points of view, and above all for the comprehension of the constitution of the universe. This is entered upon with ardor in America. At the Cambridge Observatory, where is arranged a generous foundation which the widow of Henry Draper made some years since in memory of her husband, two refractors and two reflectors are devoted to this class of researches. Mr. Pickering,[*] whose energy knows no obstacles, has undertaken a veritable spectroscopic revision of the sky. In the first place a catalogue of the spectra of the stars visible to the naked eye has been commenced. A second catalogue will contain numerous spectra of faint stars, to the eighth magnitude. It is proposed besides to make a detailed study of the spectra of the brightest stars, of variable stars, and in general of all spectra which offer remarkable peculiarities. A first list of 10,875 spectra is finished. This autumn an expedition will be sent to the southern hemisphere, probably Peru, to complete the work to the south pole.

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* The changes in the direction of the velocity of Sirius, if they are real, can be explained by an orbital movement.

† In taking the mean of numerous determinations, taken since W. Herschel, the point towards which the sun is moving is found to be 267° in right ascension, and 31° of north declination. In respect to the velocity of this motion it has been estimated at 25 to 30 kilometers per second; this is a little less than the velocity of the earth in its orbit.

‡ Tisserand, "Treatise on celestial mechanics," t. I, p. 42.

Mr. Pickering hopes also to draw to a successful termination a series of photometric researches which have for an aim the comparison of stellar magnitudes, furnished on the one side by photography and on the other by direct observation by the means of various photometers in use. These researches reach a thousand stars near the pole, an equal number taken in the neighborhood of the equator, and the stars visible in the constellation of the Pleiades, the one of the best known in the northern sky, and which offers the advantage of containing scarcely any but white stars. It is also this constellation that Mr. J. Scheiner has selected for photometric experiments, the results of which he is about to publish.

These classes of researches will give the means of reducing the different scales to a common measure. We know already that the photographic scale is established by the diameters of the stellar disks. For a given time of exposure the differences of the diameters will, in general, be proportional to the differences of magnitude, such as result from direct photometric comparisons. With a little acquaintance the estimation of the magnitudes could be without doubt reached during the micrometric measures of the negatives, as astronomers estimate them during the observation of transits. For a more precise determination all the stars on a negative could be referred to three or four among them, of which the magnitudes could be measured by photometry.

The processes in use permit in general the fixing of the magnitude[†] of a star to about one-tenth, at least, for the first nine or ten magnitudes; this is shown by the agreement of the published determinations by different observers. This is not so when exceptionally bright stars which range above the first magnitude are concerned, or very faint stars below the tenth, and the designations of the fifteenth, sixteenth, seventeenth magnitudes do not have a precise meaning, only by virtue of definition, by such and such an observer. We can, as is done at Paris, define them by the length of exposure necessary to make the images appear, for this time varies in the proportion of 1 to 10,000 from the sixth magnitude to the extreme limit of visibility, and furnishes a scale the most extended. But it is necessary to take account of the variable sensitiveness of the plates; finally it is clear that the process founded upon the estimation of the time of exposure is not favored in present applications as much as the method which consists in the comparison of the disks taken on the same negative. It is then the latter method which is sought to be perfected, for it does not suffer from difficulties when stars gradually fainter are dealt with. The images, then, in forming have dimensions already very appreciable, which increase only very little in the first moments; the comparison of diameters can conduct to erroneous results if it is taken too soon. The progressive increase of the disks has for a cause the irradiation which results from the interior illumination of the translucent gelatine at the point where the image is formed.

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[*] E. C. Pickering, annual reports of the photographic study of stellar spectra.

[†] From one magnitude to another the relative brightness diminishes (in the mean) in the proportion of 1 : 0.42.

III.

The measurers of double stars are subject to a multitude of errors which regard the difference of magnitude of the components, the inclination of the line of the stars, etc., and which render the results obtained by different observers comparable with difficulty. We meet difficulties of the same nature in micrometric measures of satellites, and it is in all these cases that the intervention of photography promises to increase greatly the accuracy and security of the results. The negatives obtained by Messrs. Henry permit the pointings to be made with extraordinary precision. The sensitive plate is not like the eye, dazzled by the vicinage of a bright star; it remains attentive to the faintest gleams. The satellite of Neptune, always visible with difficulty at Paris, can be photographed in all parts of its orbit, even when it is found at only 8 seconds from the planet.

Satellites of new planets, hitherto unknown, will reveal their existence by the trace of their course in the midst of fixed stars. The apparent displacement of a small planet about the epoch of opposition, that is to say at the moment when it approaches nearest to the earth, is in the mean one minute of arc in two hours, or 0′.5 per hour; upon the negatives of the Paris observatory a trace is produced, in one hour of exposure, of one-half of a millimeter. For the planet Pallas, which is the eighth magnitude, this trace is found easily recognizable; but Messrs. Henry think that it would still be appreciable for a planet of the fourteenth or fifteenth magnitude, with a relative brightness four or five hundred times fainter.

The number of asteroids known has increased by several each year; it reaches already 283. Thanks for the intervention of photography, the search, hitherto very laborious, for these little bodies will become so easy that we shall see them multiply too rapidly for the liking of the calculators, and there will not be time enough to select their names. In spite of the insignificance of their masses these humble supernumeraries of the solar cortege interest astronomers in more than one connection; indeed they are watched after almost for the purpose of avoiding losing them after having discovered and inscribed them upon the register of the planetary system. This happens however from time to time, when the first observations have not been sufficiently numerous to fix very securely the elements of the orbit; there are at present a score of these bodies which are wanting at roll-call.

The great diversity in the form and situation of their orbits opens to young astronomers a field for mathematical exercise, and raises at times arduous problems. In order to arrive at a determination of their feeble masses, which we have been able to estimate only from their brightness, it is necessary to be able to establish, for example, the mutual perturbations of two asteroids passing very near one another, so that their reciprocal attraction becomes sensible aside from that of the sun. It is a matter of interest then to predict the coming near to each other or physical conjunctions of the asteroids; but the proximities worthy of being noted are rather rare, or at least they occur only between the orbits and not between the planets.* Perhaps some day the passing of a comet through the belt of asteroids will offer us other means of estimating the power of the attraction of these pigmies. In return, the perturbations which they experience themselves on the part of Jupiter are sometimes very sensible, and they have already served (notably those of Themis and Amphitrite) in verifying the value of the mass of this planet, which represents a little less than a thousandth of the mass of the Sun. The one of the three planets discovered in the month of last October, by Mr. Palisa (it has received the number 279, and the name Thulé), is particularly interesting in this regard, for its mean distance (4.3) surpasses that of all the known asteroids, and permits it to approach near enough to Jupiter to be very strongly disturbed in its course. These are some of the reasons which make us think that photography, in facilitating much the search for small planets, will not serve solely for swelling the statistics of the solar system.

A discovery infinitely more interesting would be, however, that of the trans-Neptunian planet, which has not ceased to haunt the imagination of astronomers. For nothing proves that Neptune must be the last term of the series of planets which gravitate around the sun. We know that Le Verrier, in 1846, had reached a determination of the position of this star by the aid of the errors or residuals of the Tables of Uranus, which amounted to 20″, and which he attributed with reason to the perturbations produced by an unknown planet. The day when he was able to announce to the Academy of Sciences that Mr. Galle had come upon this planet in the indicated place, he added: "This success ought to permit us to hope that after thirty or forty years of observations of the new planet, it may be possible to employ them in their turn in the discovery of that which follows it in order of distance from the sun."*

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* The shortest distance between the orbits of Thetis and Bellona is estimated at 30,000 kilometers; for Clytia and Nemesis, this distance is 115,000 kilometers, and the two planets are found at 950,000 kilometers apart in the month of August, 1889; this is twice and a half the distance of the moon from the earth.

He continues thus: "We will unhappily soon fall upon stars invisible, on account of their immense distance from the sun, but whose orbits will be completed in the course of centuries, by being traced with great exactness, by means of the theory of secular inequalities." More than forty years have elapsed since the discovery of Neptune without realizing the hope of Le Verrier. The fact is that his formulæ represent always with precision not only the observations of Neptune made since 1846, but also some observations much more ancient (Lalande had come upon the planet twice, in 1795, and had entered it in his catalogue as a star of the eighth magnitude). We do not know then on what to rest, to renew the prodigious discovery of Le Verrier, which already itself had been possible only on account of a happy concourse of circumstances. It is this which we can not prevent ourselves from remembering in reading the masterly exposition which Mr. Tisserand has made in the history of the discovery of Neptune in the first tome of his "Treatise on Celestial Mechanics" which appeared a few months since.

The trans-Neptunian planet, if it exists, will be found perhaps at a distance very great, surpassing more than one hundred times the radius of the terrestrial orbit, or else its mass is relatively small, and the action which it exercises will not make accusation against it till after a long period. Let us not forget that Neptune has scarcely traversed one quarter of his orbit since the epoch of discovery. It may be possible even that the action of a mass relatively large may remain for a long time hidden from us, in being confounded with that of the other planets.

There are then few chances for discovering the hypothetical star by virtue alone of the law of Newton. It is necessary rather to count on the happy chance of recognizing it among stars of the twelfth or thirteenth magnitude, among which it may be lost. All these things did not prevent Mr. David P. Todd from constituting himself the prophet of the trans-Neptunian planet, for which he entered upon a search since 1874 by the systematic exploration of certain regions of the sky.† During the winter of 1877-'78 he employed in this exploration the great refractor of the Washington observatory. He closed in placing his hope in photography, which appears called to render this class of researches much more easy. Mr. Todd founds his conviction of the existence of the planet upon the examination of the last residuals of the Tables of Uranus, to which he has applied a very simple graphical process indicated by Sir John Herschel in reference to the perturbations of Uranus due to Neptune. That which has fortified him in this conviction is the very probable accidental agreement of his result with that to which Mr. G. Forbes has been conducted by the consideration of a tendency to grouping of the aphelia of periodic comets, whose distances from the sun coincide more or less exactly with the mean distances of the larger planets. Having found indeed seven comets whose aphelion distances approach 100, and six whose aphelion distances approach 300 (the unit being always the radius of the terrestrial orbit), Mr. Forbes concluded from this that it is possible that there are two trans-Neptunian planets situated respectively at the distances 100 and 300, and whose powerful attraction has acquired these comets for the solar system. The comets thus captured would be able then to inform us of the actual position of the planet to which we owe them, and which has in times past found itself in proximity to their aphelia. But having taken little from the value of these premises, the numerical data upon which the calculations of Mr. Forbes rest do not bear scrutiny. The agreement of the results of Mr. Forbes and Mr. Todd signifies nothing when it is seen how these results have been established. In spite of everything the trans-Neptunian planet may indeed appear some day before our astonished gaze upon one of the negatives which will serve to prepare the general chart of the sky.

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* Galle having proposed for the new planet the name of Janus, Le Verrier replied to him: "The name of Janus would indicate that this planet is the last of the solar system, which there is no reason to believe."

† "Account of a speculative and practical search for a trans-Neptunian planet," 1880. (*Proceedings of the American Academy of Sciences*, 1880-'86.)

Physical astronomy also sees new horizons to open out before it. I will not speak here at length of the photographs of the sun and moon. For a long time we have been able to see those which have great beauty. We know with what success Mr. Janssen, at Meudon, pursues the application of photography to the study of solar phenomena. Researches of the same kind are made at Potsdam, and Mr. Wilsing, depending on a hundred plates for the positions of groups of faculæ, has arrived at this unexpected conclusion, that (contrary to that which takes place with the spots) the velocity of rotation of the faculæ is the same for all the parallels, and equal to that of the equator. The retardation of the motion of the spots explains indeed why those which originate at the foot of a facula proceed gradually in the direction of its parallel as if sown upon its course. It is probable that the unequal velocity which the spots possess is limited by a rather thin layer of the solar envelope, while the great mass turns solidly with the constant velocity of the faculæ.

We know the stereoscopic effects which are obtained with photographs of the moon, taken at two epochs suitably selected. Perfected from time to time, these photographs will be of service in studying more exactly the libration; they will also cause the discovery of changes which are occurring, perhaps, on the surface of our satellite, and which, affirmed by some, contested by others, remain up to the present time very doubtful. On the contrary, the reality of modifications, sometimes rather sudden, appears to-day well verified for some planets. It suffices to recall the mysterious rectilinear canals which Messrs. Schiaparelli and Perrotin have pointed out on the surface of Mars. In reference to sketches sent from Nice, Mr. Janssen has made the statement that he was urgently seeking to obtain, with the aid of our great instrument, photographic images sufficiently perfect to replace these designs. "I know," says he, "that when phenomena are concerned, as delicate as those which have been discovered at Milan and at Nice, photography unhappily can no longer strive with sight; but it is necessary to enter resolutely into this path, to prepare for the future." If in place of designs, we could have photographic images even less detailed, we would already derive from them, in regard to the changes which have occurred on the surface of Mars, notions incomparably more certain than those with which we are obliged to content ourselves.

In order to judge of the difficulty which is experienced in confronting designs of a diverse origin, we have only to pass in review the long series of sketches of the nebula of Orion, made through two centuries by observers such as Huyghens, Mairan, Messier, De Vico, Lamont, J. Herschel, Lassell, O. Struve, the two Bonds, Lord Rosse, Father Secchi. In 1882, Mr. Holden devoted to this nebula a monograph where he gives the results of his own observations, at the same time also copies of the more celebrated drawings of this famous object. These copies, notwithstanding they are very imperfect, cause no less the growth of the conviction that it would be rash to invoke unbiased testimony to prove whether it is true that the appearance varies thus from one sketch to another. Mr. Holden has also reproduced a photograph of the nebula, obtained by H. Draper in 1882. It has been since photographed by Mr. Common, by Mr. Roberts, and by other astronomers. Messrs. Holden and Struve think that the contour of the nebula of Orion has not changed since it has been observed with care, but that the brightness of certain portions has undergone variations which continue to re-produce themselves before our eyes. Photography alone will be able, some day, to give us in this regard a complete certainty, as it permits us already to watch rapid changes in comets, in outlines so variable.

Meanwhile, it has already called up from the bosom of the darkness unknown nebulæ which the human eye had not perceived. Upon a plate of the Pleiades, which Messrs. Henry had obtained November 16, 1885, the star Maïa was shown accompanied with a small cometary tail, very brilliant; it was discovered that this was a nebulosity. It has been found that it also impressed itself upon a negative of Mr. Pickering, which dated November 3; but in America it had been taken for an accidental spot. Once informed, astronomers in possession of very powerful telescopes have been able to verify directly its existence; it has been observed successively at Pulkova, Nice, Vienna, Washington, Geneva, and other places with more or less facility. Since then, Messrs. Henry have continued to perfect their processes, and they repeat each year the negative of the Pleiades, which is well worth the trouble. The impressions of 1888, obtained with very sensitive plates and an exposure of four hours, have revealed with surprising clearness the diffuse mass of cosmical matter which envelopes this constellation, and of which the nebulæ of Maïa and of Merope are only the most luminous parts. A curious and very unexpected peculiarity is a rectilinear filament of nebulous matter which proceeds from the principal mass, over a length of 40′ of arc and a breadth of 3″ to 4″ only; it encounters on its course seven stars which it unites together as beads of a chaplet. A second line, similar but shorter, exists in the midst of the nebulous mass. This new negative contains besides twice as many stars as the first, about 2,000. The chart of the Pleiades of Mr. C. Wolf, which consumed several years of labor, contains only 671.

Mr. Pickering has entered into the same path, and very recently his plates have revealed the existence of five or six new nebulæ in different regions of the sky. Finally, some months since, Mr. Roberts communicated to the Astronomical Society in London photographs of the elliptical nebula of Andromeda, which are indeed a revelation. That which seemed an unformed mass of cosmical matter, traversed by irregular fissures, appeared now as a solar system in embryo; rings are distinguished in it, which are detached from the central mass, as is required by the hypothesis of Laplace, and two satellites in course of formation, whose relative positions must have undergone some changes since the epoch of the observations of Bond. Photography renders thus intelligible a structure which sketches are inclined to conceal.

The success obtained in this field can depend, in a certain measure, upon a particular photogenic power of nebulæ; but it is explained especially by this fact, that the sensitive plate is not dazzled by the vicinage of more brilliant objects. The nebula which surrounds the variable star Eta Argus, was invisible when this star appeared to be the first magnitude, and was discovered only when the star which eclipses it caused it to descend to the fourth order (it is now the seventh magnitude).

There is found to be an advantage of the same order in the application of photography in the registering of phenomena instantaneous or of very short duration, like eclipses, occultations, meridian transits, where the cool-headed sensitive plate shields us from the trouble, and from errors inseparable from a precipitate observation. A great number of total solar eclipses, also the two transits of Venus, 1874 and 1882, have already been observed by this means. The measures of numerous negatives taken by the French expeditions have been confided to a personnel of the gentler sex, under the direction of Mr. Bouquet de La Grye; they are completed, and the calculations are in a very advanced state.

We will limit here this rapid review of the services which photography has rendered to astronomy, or which it is to render to it after a delay which is foreseen, and which is already in some degree discounted. So great result, and so unexpected, acquired in so short a time—is not this the most brilliant guaranty of the future? At the same time telescopes are perfected and attain colossal dimensions. The greatest at the present time is the refractor of 0.90^{m} in aperture which is to be installed on the summit of Mount Hamilton, in California, where stands, 1,300 meters above the sea-level of the Pacific, an observatory founded by James Lick. This old manufacturer of organs, made rich by fortunate speculations, and desirous of perpetuating his name in the memory of men, had for a long time hesitated between a pyramid under which to be interred, and an observatory which should be erected above the clouds. It was said to him that a pyramid, which he wished to be located at the entrance of the harbor of San Francisco, would be taken in case of war for a mark by the enemy, and he decided upon the observatory, where he reposes under the great telescope. There have been spent, in constructing it and in making a road to it, more than $700,000. The bequest is not sufficient for it, and the State has been obliged to intervene. But its atmosphere has a purity unknown elsewhere; it has at least two days of fine weather out of three.

THE LIFE-WORK OF A CHEMIST.*

By Sir Henry E. Roscoe, F. R. S., *President.*

In asking myself what subject I could bring before you on the present occasion, I thought I could not do better than point out by one example what a chemist may do for mankind. And in choosing this theme for my discourse I found myself in no want of material, for amongst the various aspects of scientific activity there is surely none which, whether in its most recondite forms or in those most easily understood, have done more to benefit humanity than those which have their origin in my own special study of chemistry. I desired to show what one chemist may accomplish, a man devoted heart and soul to the investigation of nature, a type of the ideal man of science—whose example may stimulate even the feeblest amongst us to walk in his footsteps if only for a short distance, whose life is a consistent endeavor to seek after truth if haply he may find it, whose watchwords are simplicity, faithfulness, and industry, and whose sole ambition is to succeed in widening the pathway of knowledge so that following generations of wayfarers may find their journeys lightened and their dangers lessened.

Such men are not uncommon amongst the ranks of distinguished chemists. I might have chosen as an example the life and labors of your some time townsman, Joseph Priestley, had not this theme been already treated by Professor Huxley, in a manner I can not approach, on the occasion of the inauguration of the statute which stands hard by. To day however I will select another name, that of a man still living, the great French chemist, Pasteur.

As a chemist Pasteur began life, as a chemist he is ending it. For although, as I shall hope to point out, his most important researches have entered upon fields hitherto tilled with but scanty success by the biologist, yet in his hands, by the application of chemical methods, they have yielded a most bountiful harvest of new facts of essential service to the well-being and progress of the human race.

And after all, the first and obvious endeavor of every cultivator of science ought to be to render service of this kind. For although it is foolish and short-sighted to decry the pursuit of any form of scientific study because it may be as yet far removed from practical application to the wants of man, and although such studies may be of great value as an incentive to intellectual activity, yet the statement is so evident as to almost amount to a truism, that discoveries which give us the power of rescuing a population from starvation, or which tend to diminish the ills that flesh, whether of man or beast, is heir to, must deservedly attract more attention and create a more general interest than others having so far no direct bearing on the welfare of the race.

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* An address delivered to the members of the Birmingham and Midland Institute, in the Town Hall, Birmingham, on October 7, 1889. (*Nature*, October 10, 1889, vol. XL, pp. 578–583.)

"There is no greater charm," says Pasteur himself, "for the investigator than to make new discoveries, but his pleasure is more than doubled when he sees that they find direct application in practical life." To make discoveries capable of such an application has been the good fortune—by which I mean the just reward—of Pasteur. How he made them is the lesson which I desire this evening to teach. I wish to show that these discoveries, culminating as the latest and perhaps the most remarkable of all, in that of a cure for the dreaded and most fearful of all fearful maladies, hydrophobia, have not been, in the words of Priestley, "lucky hap-hazardings," but the outcome of patient and long-continued investigation. This latest result is, as I shall prove to you, not an isolated case of a happy chance, but simply the last link in a long chain of discoveries, each one of which has followed the other in logical sequence, each one bound to the other by ties which exhibit the life-work of the discoverer as one consequent whole. In order however to understand the end we must begin at the beginning, and ask ourselves what was the nature of the training of hand, eye, and brain, which enabled Pasteur to wrest from nature secret processes of disease the discovery of which had hitherto baffled all the efforts of biologists? What was the power by virtue of which he succeeded when all others had failed; how was he able to trace the causes and point out remedies for the hitherto unaccountable changes and sicknesses which beer and wine undergo? What means did he adopt to cure the fatal silk-worm disease, the existence of which in the south of France in one year cost that country more than 100,000,000 of francs? Or how did he arrive at a method for exterminating a plague known as fowl cholera, or that of the deadly cattle disease, anthrax, or splenic fever, which has killed millions of cattle, and is the fatal woolsorters' disease in man? And last, but not least, how did he gain an insight into the workings of that most mysterious of all poisons, the virus of hydrophobia?

To do more than point out the spirit which has guided Pasteur in all his work, and to give an idea of the nature of that work in a few examples, I can not attempt, in the time at my disposal. Of the magnitude and far-reaching character of that work we may form a notion, when we remember that it is to Pasteur that we owe the foundation of the science of bacteriology, a science treating of the ways and means of those minute organisms called microbes, upon whose behavior the very life, not only of the animal, but perhaps also of the vegetable world depends,—a science which bids fair to revolutionize both the theory and practice of medicine, a science which has already, in the hands of Sir Joseph Lister, given rise to a new and beneficent application in the discovery of antiseptic surgery.

The whole secret of Pasteur's success may be summed up in a few words. It consisted in the application of the exact methods of physical and chemical research, to problems which had hitherto been attacked by other less precise and less systematic methods. His early researches were of a purely chemical nature. It is now nearly forty years ago since he published his first investigation. But this pointed out the character of the man and indicated the lines upon which all his subsequent work was laid.

Of all the marvellous and far-reaching discoveries of modern chemistry perhaps the most interesting and important is that of the existence of compounds which while possessing an identical composition (that is, made up of the same elements in the same proportions), are absolutely different substances judged of by their properties. The first instance made known to us of such isomeric bodies, as they are termed by the chemist, was that pointed out by the great Swedish chemist, Berzelius. He showed that the tartaric acid of wine-lees possesses precisely the same composition as a rare acid having quite different properties and occasionally found in the tartar deposited from wine grown in certain districts in the Vosges. Berzelius simply noted this singular fact, but did not attempt to explain it. Later on, Biot observed that not only do these two acids differ in their chemical behavior, but likewise in their physical properties, inasmuch as the one (the common acid) possessed the power of deviating the plane of a polarized ray of light to the right, whereas the rare acid has no such rotatory power. It was reserved however for Pasteur to give the explanation of this singular and at that time unique phenomenon, for he proved that the optically inactive acid is made up of two compounds, each possessing the same composition but differing in optical properties. The one turned out to be the ordinary dextro-rotatory tartaric acid; the other a new acid which rotates the plane of polarization to the left to an equal degree. As indicating the germ of his subsequent researches, it is interesting here to note that Pasteur proved that these two acids can be separated from one another by a process of fermentation, started by a mere trace of a special form of mold. The common acid is thus first decomposed, so that if the process be carried on for a certain time only the rarer lævo-rotatory acid remains.

Investigations on the connection between crystalline form, chemical composition, and optical properties occupied Pasteur for the next seven years, and their results—which seem simple enough when viewed from the vantage ground of accomplished fact—were attainable solely by dint of self-sacrificing labors such as only perhaps those who have themselves walked in these enticing and yet often bewildering paths can fully appreciate, and by attention to minute detail as well as to broad principles to an extent which none can surpass and few can equal. A knowledge of the action of the mold in the changes it effects on tartaric acid led Pasteur to investigate that *bête noire* of chemists, the process of fermentation. The researches thus inaugurated in 1857, not only threw a new and vivid light on these most complicated of chemical changes and pointed the way to scientific improvements in brewing and wine-making of the greatest possible value, but were the stepping-stones to those higher generalizations which lie at the foundation of the science of bacteriology, carrying in their train the revolutions in in modern medicine and surgery to which I have referred.

The history of the various theories from early times until our own day which have been proposed to account for the fact of the change of sugar into alcohol, or that of alcohol into vinegar, under certain conditions, a fact known to the oldest and even the most uncivilized of races, is one of the most interesting chapters in the whole range of chemical literature, but however enticing, is one into which I can not now enter. Suffice it here to say that it was Pasteur who brought light out of darknesss by explaining conflicting facts and by overturning false hypotheses. And this was done by careful experiment and by bringing to bear on the subject an intelligence trained in exact methods and in unerring observation, coupled with the employment of the microscope and the other aids of modern research.

What now did Pasteur accomplish? In the first place he proved that the changes occurring in each of the various processes of fermentation are due to the presence and growth of a minute organism called the ferment. Exclude all traces of these ferments and no change occurs. Brewers' wort thus preserved remains for years unaltered. Milk and other complex liquids do not turn sour even on exposure to pure air, provided these infinitely small organisms are excluded. But introduce even the smallest trace of these microscopic beings and the peculiar changes which they alone can bring about at once begin. A few cells of the yeast plant set up the vinous fermentation in a sugar solution. This is clearly stated by Pasteur as follows: "My decided opinion," he says, "on the nature of alcoholic fermentation is the following: The chemical act of fermentation is essentially a correlative phenomenon of a vital act beginning and ending with it. I think that there is never any alcoholic fermentation without there being at the same time organization, development, multiplication of globules, or the continued consecutive life of globules already formed."

Add on a needle's point a trace of the peculiar growth which accompanies the acetous fermentation and the sound beer or wine in a short time becomes vinegar. Place ever so small a quantity of the organism of the lactic fermentation in your sweet milk, which may have been preserved fresh for years in absence of such organisms, and your milk turns sour. But still more, the organism (yeast) which brings about the alcoholic fermentation will not give rise to the acetous, and *vice versa;* so that each peculiar chemical change is brought about by the vital action of a peculiar organism. In its absence the change can not occur; in its presence only that change can take place.

Here again we may ask, as Pasteur did, why does beer or wine become sour when exposed to ordinary air? And the answer to this question was given by him in no uncertain tone in one of the most remarkable and most important of modern experimental researches. Milk and beer which have become sour on standing in the air contain living micro-organisms which did not exist in the original sound fluids. Where did these organisms originate? Are they or their germs contained in the air, or are these minute beings formed by a process of spontaneous generation from material not endowed with life?

A controversy as to the truth or falsity of the theory of spontaneous generation was waged with spirit on both sides, but in the end Pasteur came off victorious, for by a series of the most delicate and convincing of experiments he proved the existence of micro-organic forms and their spores—or seeds—in the air, and showed that while unpurified air was capable of setting up fermentative changes of various kinds, the same air freed from germs could not give rise to these changes Keep away the special germ which is the incentive to the pathological change and that change can not occur. In the interior of the grape, in the healthy blood, no such organisms, no such germs exist; puncture the grape or wound the animal body and the germs floating in the air settle on the grape juice or on the wounded tissue, and the processes of change, whether fermentative or putrefactive, set in with all their attendant symptoms. But crush the grape or wound the animal under conditions which either preclude the presence or destroy the life of the floating germ, and again no such change occurs; the grape-juice remains sweet, the wound clean.

I have said that every peculiar fermentative change is accompanied by the presence of a special ferment. This most important conclusion has only been arrived at as the result of careful experimental inquiry. How was this effected? By the artificial cultivation of these organisms. Just as the botanist or gardener picks out from a multitude of wild plants the special one which he wishes to propagate, and planting it in ground favorable to its growth, obtains fresh crops of the special plant he has chosen, so the bacteriologist can, by a careful process of selection, obtain what is termed a pure cultivation of any desired organism. Having obtained such a pure cultivation, the next step is to ascertain what are the distinctive properties of that special organism; what characteristic changes does it bring about in material suitable for its growth. This having been determined, and a foundation for the science having thus been laid, it is not difficult to apply these principles to practice, and the first application made by Pasteur was to the study of the diseases of beer and wine.

In September, 1871, Pasteur visited one of the large London breweries, in which the use of the microscope was then unknown. A single glance at the condition of the yeast instantly told its tale, and enabled him to explain to the brewers the cause of the serious state of things by which frequently as much as 20 per cent. of their product was returned on their hands as unsalable—this being that this yeast contained foreign or unhealthy organisms. And just as pure yeast is the cause of the necessary conversion of wort into beer, so these strange forms which differ morphologically from yeast, and whose presence can therefore be distinctly ascertained, are the cause of acidity, ropiness, turbidity, and other diseases which render the beer undrinkable. It is no exaggeration to say that, whereas before Pasteur's researches the microscope was practically unknown in the brew-house, it has now become as common as the thermometer or the saccharimeter, and by its help and by the interpretations we can place upon its revelations through Pasteur's teaching, yeast—of all brewers' materials the least open to rough and ready practical discernment—becomes easy of valuation as to its purity or impurity, its vigor or weakness, and therefore its behavior during fermentation. Thus, while in former days the most costly materials were ever liable to be ruined by disease organisms unconsciously introduced into them with the yeast, at the present day the possibilities of any such vast pecuniary disasters become easily avertable.

Of all industries, brewing is perhaps the one which demands the most stringent care in regard to complete and absolute cleanliness. The brewers' materials, products, and by-products, are so putrescible, there is always so vast an abundance of disease organisms in the brewery air, that the minutest amounts of these waste products lying about in vessels or pipes transform these places into perfect nests for the propagation of these micro-organisms, whence, transferred into the brewings, they inevitably ruin them, however carefully and scientifically prepared in other respects. Without the microscope, any breach of discipline in the way of the supreme cleanliness necessary is impossible of detection; with it we can track down the micro-organisms to their source, whether it be in uncleanly plant, in impurity of materials, or in carelessness of manipulation.

Among the more direct applications of Pasteur's researches, the so-called Pasteurization of beer claims a place. Pasteur showed that temperatures well below the boiling-point sufficed for destroying the disease organisms in alcoholic fluids, and, based on these results, enormous quantities of low-fermentation beers are annually submitted to these temperatures, and thus escape the changes otherwise incident to the micro-organisms which have succumbed to the treatment. This process is however for several intricate reasons, not suited for English beers, but if we can not keep our beers by submitting them to high temperatures, we can foretell to a nicety how they will keep by artificially forcing on those changes which would occur more slowly during storage. The application of a suitable temperature, the exclusion of outside contamination, a microscopic examination of the "forced" beer, and the knowledge which we owe to Pasteur of what the microscopic aspect means, suffice to make each brewing foretell its own future history, and thus suffice to avert the otherwise inevitable risks incident to the storage and export of beer, the stability of which is unknown.

Brewing has thus become a series of precise and definite operations, capable of control at every point. Instead of depending—as it had to depend—on intuition and experience handed down in secrecy from father to son, it now depends upon care, forethought, and the soundness of the brewer's scientific training. This change in the nature of the brewer's operations, and in the persons who govern them, is primarily due to Pasteur. Other men have done much to carry on his work, but it is to his example of ceaseless patience, and to his example of freely publishing to the world all the results of his work, that the brewers of all countries are indebted for the connection of each phenomenon with a controllable cause, and for thus emancipating their industry from empiricism and quackery.

Much the same story has to be told about Pasteur's investigation of wine and its diseases. As with the brewer, so with the wine-grower Pasteur has pointed out the causes of his troubles, and the causes having been ascertained, the remedies soon followed, and the practical value of these researches to the trade of France and other wine-producing countries has been enormous.

The next labor of our scientific Hercules was of a different kind, but of a no less interesting or important character. The south of France is a great silk-producing district. In 1853, the value of the raw silk was represented by a sum of some £5,000,000 sterling, and up to that date the revenue from this source had been greatly augmenting. Suddenly this tide of prosperity turned, a terrible plague broke out amongst the silk-worms, and in 1865, so general had the disease become, that the total production of French silk did not reach £1,000,000, and the consequent poverty and suffering endured in these provinces became appalling. Every conceivable means was tried to overcome the disease, but all in vain. The population and the Government of France—for the evil was a national one—were at their wits' end, and a complete collapse of one of the most important French industries seemed inevitable. Under these circumstances the great chemist Dumas, who was born at Alais, in the center of one of the districts most seriously affected, urged his friend Pasteur to undertake an investigation of the subject. Pasteur, who at this time had never seen a silk-worm, naturally felt diffident about attempting so difficult a task, but at last, at Dumas's renewed entreaty, he consented, and in June, 1865, betook himself to the south for the purpose of studying the disease on the spot. His previous training here again stood him in good stead, and in September, 1865, he was able to communicate to the Academy of Sciences, results of observations and experiment which, striking at the root of the evil, pointed the way to the means of securing immunity from the dreaded plague. This paper was freely criticised. Here, it was said, was a chemist who, quitting his proper sphere, had the hardihood to lay down rules for the guidance of the physician and biologist in fields specially their own. Why should his proposals be more successful than all the other nostrums which had already so egregiously failed?

In order to appreciate the difficulties which met Pasteur in this inquiry, and to understand how wonderfully he overcame them, I must very shortly describe the nature of this disease, which is termed *pébrine*, from the black spots which cover the silk-worm. It declares itself by the stunted and unequal growth of the worms, by their torpidity, and by their fastidiousness as to food, and by their premature death.

Before Pasteur went to Alais the presence of certain microscopic corpuscles had been noticed in the blood and in all the tissues of the diseased caterpillar, and even in the eggs from which such worms were hatched. These micro-organisms often fill the whole of the silk organs of the insect, which in a healthy condition contain the clear viscous liquid from which the silk is made. Such worms are of course valueless. Still this knowledge did not suffice, for eggs apparently healthy gave rise to stricken worms incapable of producing silk, whilst again other worms distinctly diseased yielded normal cocoons. These difficulties, which had proved too much for previous observers, were fully explained by Pasteur. "The germs of these organisms," said he, "which are so minute, may be present in the egg and even in the young worms, and yet baffle the most careful search. They develop with the growth of the worm, and in the chrysalis they are more easily seen. The moth derived from a diseased worm invariably contains these corpuscles, and is incapable of breeding healthy progeny."

This moth-test is the one adopted by Pasteur, and it is an infallible one. If the female moth is stricken, then her eggs, even though they show no visible sign of disease, will produce sick worms. If in the moth no micrococci are seen, then her immediate progeny at any rate will be sound and free from inherited taint, and will always produce the normal quantity of silk. But this is not all. Pasteur found that healthy worms can be readily infected by contact with diseased ones, or through germs contained in the dust of the rooms in which the worms are fed. Worms thus infected, but free from inherited taint, can however (as stated) spin normal cocoons, but—and this is the important point—the moths which such chrysalids yield invariably produce diseased eggs. This explains the anomalies previously noticed. The silk-worms which die without spinning are those in which the disease is hereditary, viz, those born from a diseased mother. Worms from sound eggs which contract the disease during their life-time always spin their silk, but they give rise to a stricken moth, the worms from which do not reach maturity and furnish no silk.

As I have said, these results were but coldly received. It was hard to make those engaged in rearing the worms believe in the efficacy of the proposed cure. Then, seeing this state of things, Pasteur determined to take upon himself the rôle of a prophet. Having in 1866 carefully examined a considerable number of the moths which had laid eggs intended for incubation, he wrote down a prediction of what would happen in the following year with respect to the worms hatched from these eggs. In due course, after the worms from a mixed batch of healthy and unhealthy eggs had spun, the sealed letter was opened and read, and the prediction compared with the actual result, when it was found that in twelve out of fourteen cases there was absolute conformity between the prediction and the observation, for twelve hatchings were predicted to turn out diseased, and this proved to be the case. Now all these "educations" were believed to be healthy by the cultivators, but Pasteur foretold that they would turn out to be diseased by the application of the moth-test in the previous year. The other parcels of eggs were pronounced by Pasteur to be sound, because they were laid by healthy moths containing none of the micrococci, and both these yielded a healthy crop. So successful a prophecy could not but gain the belief of the most obtuse of cultivators, and we are not suprised to learn that Pasteur's test was soon generally applied, and that the consequence has been a return of prosperity to districts in which thousands of homes had been desolated by a terrible scourge.

I must now ask you to accompany me to another and a new field of Pasteur's labors, which, perhaps more than his others, claims your sympathy and will enlist your admiration, because they have opened out to us the confident hope of at least obtaining an insight into some of the hidden causes and therefore to the possible prevention of disease.

In the first place, I must recall to your remembrance that most infectious diseases seldom if ever recur, and that even a slight attack renders the subject of it proof against a second one. Hence inoculation from a mild case of small-pox was for a time practiced, but this too often brought about a serious if not fatal attack of the malady, and the steps taken by Jenner of vaccinating, that is of replacing for the serious disease a slight one which nevertheless is sufficient protection against small-pox infection, was one of the highest importance. But Jenner's great discovery has up to recent years remained an isolated one, for it led to no general method for the preventive treatment of other maladies, nor had any explanation been offered of its mode of action. It is to Pasteur that science is indebted for the generalization of Jenner's method, and for an explanation which bids fair to render possible the preventive treatment of many—if not of all—infectious diseases. It was his experience, based upon his researches on fermentation, that led to a knowledge of the nature of the poison of such diseases, and showed the possibility of so attenuating or weakening the virus as to furnish a general method of protective or preventive inoculation.

I have already pointed out how a pure cultivation of a microbe can be effected. Just as the production of pure alcohol depends on the presence of the pure yeast, so special diseases are dependent on the presence of certain definite organisms which can be artificially cultivated, and which give rise to the special malady. Can we now by any system of artificial cultivation so modify or weaken the virus of a given microbe as to render it possible to inoculate a modified virus which, whilst it is without danger to life, is still capable of acting as a preventive to further attack? This is the question which Pasteur set himself to solve, nor was the task by any means an apparently hopeless one. He had not only the case of Jennerian vaccination before him, but also the well-known modifications which cultivation can bring about in plants. The first instance in which Pasteur succeeded in effecting this weakening of the poison was in that of a fatal disease to which poultry in France are very liable, called chicken cholera. Like many other maladies, this is caused by the presence of a micro-organism found in the blood and tissues of the stricken fowl. One drop of this blood brought under the skin of a healthy chicken kills it, and the same microbe is found throughout its body. And if a pure culture of these microbes be made, that culture—even after a series of generations—is as deadly a poison as the original blood. Now comes the discovery. If these cultures be kept at a suitable temperature for some weeks exposed to pure air, and the poisonous properties tested from time to time, the poison is found gradually to become less powerful, so that after the lapse of two months a dose which had formerly proved fatal now does not disturb in the slightest the apparent health of the fowl. But now let us inoculate a chicken with this weakened virus. It suffers a slight illness, but soon recovers. Next let us give it a dose of the undiluted poison, and, as a control, let us try the action of the same on an unprotected bird. What is the result? Why, that the first chicken remains unaffected, whilst the second bird dies. The inoculation has rendered it exempt from the disease, and this has been proved by Pasteur to be true in thousands of cases, so that whereas the death-rate in certain districts amongst fowls before the adoption of Pasteur's inoculation method was 10 per cent., after its general adoption it has diminished to less than 1 per cent.

We can scarcely value too highly this discovery, for it proves that the poisonous nature of the microbe is not unalterable, but that it can be artificially modified and reduced, and thus an explanation is given of the fact that in an epidemic the virus may either be preserved or become exhausted according to the conditions to which it is subjected. We have here to do with a case similar to that of Jenner's vaccine, except that here the relation between the weak and the strong poison has become known to us, whilst in Jenner's case it has lain concealed. This then is the first triumph of experimental inquiry into the cause and prevention of microbic disease, and this method of attenuation is of great importance, because, as we shall see, it is not confined to the case of chicken cholera, but is applicable to other diseases.

And next I will speak of one which is a fatal scourge to cattle, and is not unfrequently transmitted to man. It is called anthrax, splenic fever, or woolsorters' disease. This plague, which has proved fatal to millions of cattle, is also due to a microbe, which can be cultivated like the rest, and the virus of which can also be weakened or attenuated by a distinct treatment which I will not here further specify. Now, what is the effect of inoculating cattle or sheep with this weakened poison? Does it act as a preventive? That the answer is in the affirmative was proved by Pasteur by a convincing experiment. Five-and-twenty sheep, chosen promiscuously out of a flock of fifty, were thus inoculated with the weak virus, then after a time all the fifty were treated with the strong poison. The first half remained healthy, all the others died of anthrax. Since the discovery of this method, no fewer than 1,700,000 sheep and about 90,000 oxen have thus been inoculated, and last year 269,599 sheep and 34,464 oxen were treated. The mortality which before the introduction of the preventive treatment, was in the case of sheep 10 per cent., was, after the adoption of the method, reduced to less than 1 per cent. So that now the farmers in the stricken districts have all adopted the process, and agricultural insurance societies make the preventive inoculation a *sine quâ non* for insuring cattle in those districts. This is however not the end of this part of my story, for Pasteur can not only thus render the anthrax poison harmless, but he has taught us how to bring the highly virulent poison back again from the harmless form. This may go to explain the varying strength of an attack of infectious disease, one case being severe and another but slight, due to the weakening or otherwise of the virus of the active microbe.

Last, but not least, I must refer to the most remarkable of all Pasteur's reseaches, that on rabies and hydrophobia. Previous to the year 1880, when Pasteur began his study of this disease, next to nothing was known about its nature. It was invested with the mysterious horror which often accompanies the working of secret poisons, and the horror was rendered greater owing to the fact that the development of the poison brought in by the bite or by the lick of a mad dog might be deferred for months, and that if after that length of time the symptoms once make their appearance, a painful death was inevitable. We knew indeed that the virus was contained in the dog's saliva, but experiments made upon the inoculation of the saliva had led to no definite results, and we were entirely in the dark as to the action of the poison until Pasteur's investigation. To begin with, he came to the conclusion that the disease was one localized in the nerve-centers, and to the nerve-centers he therefore looked as the seat of the virus or of the microbe. And he proved by experiment that this is the case, for a portion of the matter of the spinal column of a rabid dog, when injected into a healthy one, causes rabies with a much greater degree of certainty and rapidity than does the injection of the saliva. Here then we have one step in advance. The disease is one of the nerve-centers, and therefore it only exhibits itself when the nerve-centers are attacked. And this goes to explain the varying times of incubation which the attack exhibits. The virus has to travel up the spinal cord before the symptoms can manifest themselves, and the length of time taken over that journey depends on many circumstances. If this be so, the period of incubation must be lessened if the virus is at once introduced into the nerve-centers. This was also proved to be the case, for dogs inoculated under the *dura mater* invariably became rabid within a period rarely exceeding eighteen days.

Next came the question, can this virus be weakened, as has been proved possible with the former poisons? The difficulty in this case was greater, inasmuch as all attempts to isolate or to cultivate the special microbe of rabies outside the animal body had failed. But Pasteur's energy and foresight overcame this difficulty, and a method was discovered by which this terrible poison can so far be weakened as to lose its virulent character, but yet remain potent enough, like the cases already quoted, to act as a preventive ; and dogs which had thus been inoculated were proved to be so perfectly protected, that they might be bitten with impunity by mad dogs, or inoculated harmlessly with the most powerful rabic virus.

But yet another step. Would the preventive action of the weakened virus hold good when it is inoculated even after the bite? If so, it might be thus possible to save the lives of persons bitten by mad dogs. Well, experiment has also proved this to be true, for a number of dogs were bitten by mad ones, or were inoculated under the skin with rabic virus; of these some were subjected to the preventive cure and others not thus treated. Of the first or protected series not one became mad; of the other, or unprotected dogs, a large number died with all the characteristic symptoms of the disease. But it was one thing to thus experiment upon dogs, and quite another thing, as you may well imagine, to subject human beings to so novel and perhaps dangerous a treatment. Nevertheless, Pasteur was bold enough to take this necessary step, and by so doing has earned the gratitude of the human race.

In front of the Pasteur Institute in Paris stands a statue worked with consummate skill in bronze. It represents a French shepherd boy engaged in a death struggle with a mad dog which had been worrying his sheep. With his bare hands, and with no weapon save his wooden *sabot*, the boy was successful in the combat. He killed the dog, but was horribly bitten in the fight. The group represents no mythical struggle; the actual event took place in October, 1885; and this boy, Jupille, was the second person to undergo the anti-rabic treatment, which proved perfectly successful, for he remained perfectly healthy, and his heroic deed and its consequences have become historic. "*C'est le premier pas qui coute*," and as soon as the first man had been successfully treated others similarly situated gladly availed themselves of Pasteur's generous offer to treat them gratuitously. And as soon as this cure became generally known crowds of persons of all ages, stations, and countries, all bitten by rabid animals, visited every day Pasteur's laboratory in the Rue d'Ulm, which, from being one in which quiet scientific researches were carried on, came to resemble the out-patient department of a great hospital. There I saw the French peasant, the Russian *moujik* (suffering from the terrible bites of rabid wolves), the swarthy Arab, the English policeman, with women too and children of every age; in all perhaps a hundred patients. All were there undergoing the careful and kindly treatment, which was to insure them against a horrible death. Such a sight will not be easily forgotten. By degrees this wonderful cure for so deadly a disease attracted the attention of men of science throughout the civilized world. The French nation raised a monument to the discoverer better than any statue in the shape of the "Pasteur Institute," an institution devoted to carrying out in practice this anti-rabic treatment, with laboratories and every other convenience for extending by research our knowledge of the preventive treatment of infectious disease.

For be it remembered, we are only at the beginning of these things, and what has been done is only an inkling of what is to come. Since 1885, twenty anti-rabic institutions have been established in various parts of the world, including Naples, Palermo, Odessa, St. Petersburg, Constantinople, Rio Janeiro, Buenos Ayres, and Havana.

We in England have also taken our share, though a small one, in this work. In 1885, I moved in the House of Commons for a committee to investigate and report on Pasteur's anti-rabic method of treatment. This committee consisted of trusted and well-known English men of science and physicians—Sir James Paget, Sir Joseph Lister, Drs. Burdon Sanderson, Lauder Brunton, Quain, Fleming, and myself, with Prof. Victor Horsley as secretary. We examined the whole subject, investigated the details of a number of cases, repeated Pasteur's experiments on animals, discussed the published statistics, and arrived unanimously at the opinion that Pasteur was justified in his conclusions, and that his anti-rabic treatment had conferred a great and lasting benefit on mankind. Since then His Royal Highness the Prince of Wales, who always takes a vivid interest in questions affecting the well-being of the people, has visited the Pasteur Institute, and has expressed himself strongly in favor of a movement, started by the present lord mayor of London, for showing to Pasteur, by a substantial grant to his Institute, our gratitude for what he has done to relieve upwards of two hundred and fifty of our countrymen who have undergone treatment at his hands, and likewise to enable poor persons who have been bitten to undertake the journey to Paris, and the sojourn there necessary for their treatment. This lasts about a fortnight, it is nearly painless, and no single case of illness, much less of hydrophobia, due to the preventive treatment, has occurred amongst the seven thousand persons who have so far undergone the cure.

Now let me put before you the answer to the question: Is this treatment a real cure? For this has been doubted by persons, some of whom will, I fear, still doubt, or profess to doubt, and still abuse Pasteur whatever is said or done! From all that can be learned about the matter, it appears pretty certain that about from fifteen to twenty persons out of every hundred bitten by mad dogs or cats, and not treated by Pasteur's method, develop the disease, for I need scarcely add that all other methods of treatment have proved fallacious; but bites on the face are much more dangerous, the proportion of fatal cases reaching 80 per cent. Now of two thousand one hundred and sixty-four persons treated in the Pasteur Institute, from November 1885, to January 1887, only thirty-two died, showing a mortality of 1.4 per cent. instead of 15 to 20, and amongst these upwards of two thousand persons, two hundred and fourteen had been bitten on the face, a class of wounds in which, as I have said, when untreated, the mortality is very high; so that the reduction in the death-rate seems more remarkable, especially when we learn that in all these cases the animal inflicting the wound had been proved to be rabid. The same thing occurs even in a more marked degree in 1887 and 1888. In 1887, one thousand seven hundred and seventy-eight cases were treated with a mortality of 1.3 per cent., while last year one thousand six hundred and twenty-six cases were treated, with a mortality of 1.16 per cent.*

Statistics of the anti-rabic treatment in other countries show similar results, proving beyond a doubt that the death-rate from hydrophobia is greatly reduced. Indeed, it may truly be said that in no case of dangerous disease, treated either by medicine or surgery, is a cure so probable. Moreover, in spite of assertions to the contrary, no proof can be given that in any single case did death arise from the treatment itself. And as showing the safety of the inoculation, I may add that all Pasteur's assistants and laboratory workers have undergone the treatment, and no case of hydrophobia has occurred amongst them.

You are no doubt aware that Pasteur's anti-rabic treatment has been strongly opposed by certain persons, some of whom have not scrupled to descend to personal abuse of a virulent character of those who in any way encouraged or supported Pasteur's views, and all of whom persistently deny that anything good has come or can come from investigations of the kind. Such persons we need neither fear nor hate. Their opposition is as powerless to arrest the march of science as was King Canute's order to stop the rising tide. Only let us rest upon the sure basis of exactly ascertained fact, and we may safely defy alike the vaporings of the sentimentalist, and the wrath of the opponent of scientific progress. But opposition of a much fairer character has likewise to be met, and it has with propriety been asked: How comes it that Pasteur is not uniformly successful? Why (if what you tell us is true) do any deaths at all follow the anti-rabic treatment?

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*For further details, see Dr. Ruffer, *Brit. Med. Journ.*, Sept. 21, 1889.

The answer is not far to seek. In the first place, just as it is not every vaccination which protects against small-pox, so Pasteur's vaccination against rabies occasionally fails. Then again, Pasteur's treatment is really a race between a strong and an attenuated virus. In cases in which the bite occurs near a nerve-center, the fatal malady may outstrip the treatment in this race between life and death. If the weakened virus can act in time, it means life. If the strong virus acts first, prevention comes too late,—it means death. So that the treatment is not doubtful in all cases, but only doubtful in those which are under well-known unfavorable conditions. This it seems to me is a complete reply to those who ignorantly fancy that, because Pasteur's treatment has not cured every case, it must be unreliable and worthless.

One word more. I have said that Pasteur is still—as he has always been—a chemist. How does this fit in with the fact that his recent researches seem to be entirely of a biological character? This is true. They seem, but they really are not. Let me in a few sentences explain what I mean. You know that yeast produces a peculiar chemical substance—alcohol. How it does so we can not yet explain, but the fact remains. Gradually, through Pasteur's researches, we are coming to understand that this is not an isolated case, but that the growth of every micro-organism is productive of some special chemical substance, and that the true pathogenic virus—or the poison causing the disease—is not the microbe itself, but the chemical compound which its growth creates. Here once more "to the solid ground of nature trusts the man that builds for aye," and it is only by experiment that these things can be learnt.

Let me illustrate this by the most recent and perhaps the most striking example we know of. The disease of diphtheria is accompanied by a peculiar microbe, which however only grows outside, as it were, of the body, but death often takes place with frightful rapidity. This takes place not by any action of the microbe itself, but by simple poisoning due to the products of the growing organism, which penetrate into the system, although the microbe does not. This diphtheritic *Bacillus* can be cultivated, and the chemical poison which it produces can be completely separated by filtration from the microbe itself, just as alcohol can be separated from the yeast granules. If this be done, and one drop of this pellucid liquid given to an animal, that animal dies with all the well-known symptoms of the disease. This, and similar experiments made with the microbes of other diseases, lead to the conclusion that in infectious maladies the cause of death is poisoning by a distinct chemical compound, the microbe being not only the means of spreading the infection, but also the manufacturer of the poison. But more than this, it has lately been proved that a small dose of these soluble chemical poisons confers immunity. If the poison be administered in such a manner as to avoid speedy poisoning, but so as gradually to accustom the animal to its presence, the creature becomes not only refractory to toxic doses of the poison, but also even to the microbe itself. So that instead of introducing the micro-organism itself into the body, it may now only be necessary to vaccinate with a chemical substance which in large doses brings about the disease, but in small ones confers immunity from it, reminding one of Hahnemann's dictum of "Similia similibus curantur."

Here then we are once more on chemical ground. True, on ground which is full of unexplained wonders, which however depend on laws we are at least in part acquainted with, so that we may in good heart undertake their investigation, and look forward to the time when knowledge will take the place of wonder.

In conclusion, I feel that some sort of apology is needed in thus bringing a rather serious piece of business before you on this occasion. Still I hope for your forgiveness, as my motive has been to explain to you as clearly as I could the life-work of a chemist who has in my opinion conferred benefits as yet untold and perhaps unexampled on mankind, and I may be allowed to close my discourse with the noble words of our hero spoken at the opening of the Pasteur Institute in the presence of the President of the French Republic:

"Two adverse laws seem to me now in contest. One law of blood and death, opening out each day new modes of destruction, forces nations to be always ready for the battle-field. The other a law of peace, of work, of safety, whose only study is to deliver man from the calamities which beset him.

"The one seeks only violent conquests. The other only the relief of humanity. The one places a single life above all victories. The other sacrifices the lives of hundreds of thousands to the ambition of a single individual. The law of which we are the instruments, strives even through the carnage to cure the bloody wounds caused by this law of war. Treatment by our antiseptic methods may preserve thousands of soldiers.

"Which of these two laws will prevail over the other? God only knows. But of this we may be sure, that science in obeying this law of humanity will always labor to enlarge the frontiers of life."

MEMOIR OF HEINRICH LEBERECHT FLEISCHER.*

By PROF. A. MÜLLER, Ph. D.

Translated by Miss HENRIETTA SZOLD.

Were it desirable to single out the rarest and most admirable among the many fine qualities of the great and good scholar to whose memory these lines are devoted, I should not hesitate to name the perfect self-denial which at all times prompted him to place his unparallelled attainments at the disposal of others. Among German orientalists (if Assyriologists be excepted), few will be found who have not profited by his unselfishness; and abroad likewise there are many who are similarly indebted. We all knew where to seek when our meager stores were on the point of giving out, and we stood in need of the gifts with which his treasure-houses were abundantly filled. In dispensing these to great and small, he was untiring, generous, and impartial as God's sun which shines upon the just and the unjust alike. More than a year has passed since his hand has grown numb and his eye dim, but where do they linger who should have hastened to his grave, and wreathed with tributes of gratitude the hillock which nature, slow though her processes are, has twice decked with fresh verdure? I blame, I accuse no one. Many a shrinking soul hides its gratitude in reverential silence rather than parade fine and tender feelings in the market-place. Doubtless there are others who reluctantly find themselves forced by the cares of existence, by daily new burthensome tasks, to deny themselves the fulfillment of a warmly cherished desire. And most probably there are still others, here and there, who, like the writer of these words, are even now, after unavoidable delay, on the point of paying the long-planned tribute of piety. Nevertheless it remains a sad fact that, with the exception of the somewhat business-like though not unsympathetic announcements of the French Institute and of the Bavarian Academy, the brief remarks, accompanying an excellent portrait of Fleischer in the Leipzig *Illustrirte Zeitung*, an article in the *New York Times*, and a barren notice in the London *Athenæum*, only two attempts have up to this time been made to give adequate and becoming treatment to the work of this distinguished scholar: Thorbecke's sketch in the Journal of the German

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* From Bezzenberger's *Beiträge zur Kunde der indogermanischen Sprachen*, Göttingen, 1889, vol. xv, pp. 319–337.

Oriental Society, and the more extended memorial address by Goldziher before the Hungarian Academy. Indeed it will ever be humiliating to German orientalists, that although more than a year has elapsed since Fleischer's death, the only searching analysis published of his great activity as a scholar and a teacher (and such Goldziher's[*] essay obviously is), has been written by an Hungarian in his native language, with which no one of us is conversant.

In fact, the number is not very great of those who may without presumption undertake an exhaustive treatment of the life of so distinguished a scholar. I am far from counting myself among that number, but I believe I have learned enough to enable me to appreciate to a certain extent the great ability of him who acquired such vast learning by means of his own exertions; and I trust I have sufficient judgment to designate at least approximately the rank and position due my deceased teacher in the history of our science. Precisely here I can not permit the motive of modesty to hinder me from attempting this task, for the reader who is interested in the science of Indo-European languages may justly wish to gain an idea of the general attitude of a scholar whose investigations border upon his own sphere. That my task also involves the duty of pointing out the natural limitations of his activity shall not hinder me from carrying out my intention. Next to unselfishness, Fleischer's most prominent trait as a scholar was his love of truth. He himself would be the first to censure me if I were to sketch his personality in white on a white back-ground, according to the latest fashion among painters. Admiration without criticism is valueless. If, feeling the former, I venture to use the latter, no one may charge me with presumptuousness. He is a poor master who trains disciples bereft of the critical faculty; a poor disciple he who leans unquestioningly upon the authority of even a deeply-revered master. I must however refrain from giving a detailed description of the purely human side of his being and life, incomplete though his picture will thus remain. I consider it improper to forestall a full presentation by one more qualified for this task, who can base his assertions upon intimate acquaintance with all the incidents and relations of his private life. I shall confine myself to outlines, the data for which I owe to the kindness of Prof. Dr. Curt Fleischer, of Meissen. They thus may claim reliability on those points in which they disagree with statements published elsewhere.

Heinrich Leberecht Fleischer was born at Schandau on February 21, 1801. His father, Johann Gottfried Fleischer, an officer in the custom-service, died at the age of eighty-nine, on August 24, 1860, at Pirna, enjoying at that time a pension as inspector of customs. His mother, whom he lost as early as August 10, 1825, was the daughter of the parish schoolmaster Unruh, at Prietitz, near Pulsnitz. At Schandau the boy attended the public school, where his talents soon attracted the attention of the principal, Edelmann, who undertook to teach young Fleischer the elements of Latin. Thus his father was enabled to enter him in 1814, as a student at the high-school in Bautzen, where he remained until 1819. Here he was instructed in Hebrew, thus for the first time coming in contact with the Orient. When next he became interested in an Eastern subject it was by chance, and it decided his whole future career. He accidentally found among the wrapping-paper of a cheese-dealer at market sheets of an Arabic grammar, to the study of which he at once applied himself. He became so deeply interested that when he entered upon his course in the University of Leipzig at Easter, 1819, he not only did not neglect his theological pursuits, nor fail to devote himself under the guidance of Gottfried Hermann to classical studies, but he also indulged his love for Orientalia. In fact, after having passed with distinction a theological examination, he spent one more year in the exclusive study of Oriental languages. He soon arrived at the conviction that it was necessary for him to be at Paris with De Sacy. By the assistance of a young French merchant named Bernard, with whom he had become acquainted, he succeeded in obtaining a position as tutor in the household of Mons. de Caulaincourt (under Napoleon Duke of Vicenza). On March 4, he received his degree, and on April 18, he began his journey to Paris. For one year and a half he was in Caulaincourt's house. Later he lived alone, earning a livelihood by giving private lessons. But during the whole time he was zealously occupied with his studies under De Sacy, paying particular attention to Arabic, Persian, and Turkish. The impression made upon him by the great French savant was never obliterated,—neither by the work, nor the success, nor the honors with which his long life was replete. He continued to pay the tribute of love and esteem to his master long after he himself had come to be looked upon as the master Arabist. At the same time he made diligent use of the valuable manuscripts in the library. Thus, the first essay published by him was a review, in the *Journal Asiatique* of 1827, of the first volume of Habicht's edition of the Thousand and One Nights, based upon Galland's manuscript. His editions of Abulfeda and Beidhawi, as well as the essay, *De glossis Habichtianis*, all published later on, are also proofs of his industry in gathering material while at Paris. At the same time he sought the society of Orientals, especially of two Egyptians, a Mohammedan—Refâ'a, and a Christian—Aydé, both mentioned *honoris causa* in the above-named essay. Although the article in the *Journal Asiatique* shows that he was a ripe scholar at that time, he continued to devote himself after his return, in 1828, to private study, partly at his own home and partly at Dresden, where he catalogued the Arabic, Persian, and Turkish manuscripts in the royal library. The catalogue was published at Leipzig in 1831; likewise, his edition and translation of Abulfeda's *Historia ante-islamica*.

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[*] Emlékbeszéd Fleischer Leberecht Henrik a M. Tud. Akadémia kültagja felett. Goldziher Ignácz (a Magy. Tud. Ak. elhúnyt tagjai fölött tarttot emlékbeszédek. Vköt. 4. szám). Budapest, M. T. Ak., 1889, 44 p., 8.

Both publications proved that he had reached the goal which he had been pursuing during a twelve years' preparatory period, entailing constant hard work and manifold sacrifices. In the preface to Abulfeda he deplores the fewness of his notes, and craves indulgence for himself on the plea of being a *homo lectionis paucæ, memoriæ paucioris, otii paucissimi.* But the character of his work is such as to invalidate all but the last of the three excuses. Meantime he had accepted in 1831, a position as teacher in the Kreuz high-school at Dresden. Here he remained until 1835, when, the above-mentioned works having spread his fame, he was offered a professorship at St. Petersburg, later filled by Dorn. He was about to leave for Russia, when, in the nick of time, the offer of a full professorship of Oriental languages, at his own university of Leipzig, reached him. He was elected on October 19, 1835, but did not enter upon the duties of his position until Easter, 1836. At first he was considered a member of the theological faculty, but early in the next decade he was permitted, after active agitation on his own part, to pass over to the philosophical faculty. On September 27, 1836, he married Ernestine Mathilde Jässing, of Bautzen, the daughter of Friedrich Leberecht Jässing, retired brigade judge of the royal Saxon service, who lived at that time in Dresden. He was permitted to celebrate, with his faithful and affectionate wife, who still survives, the fiftieth anniversary of his wedding-day, and was blessed with the joy—marred only by the death of their eldest daughter—of seeing their children occupy positions of honor in the community. Not less happy was his domestic life, than were his official and scientific undertakings. When, in 1846, the Royal Saxon Scientific Society was founded, he at once became an active member. In 1855, he became its assistant secretary, and later, secretary in chief, a position which he filled with his customary scrupulousness until 1883. In 1860, he received an honorable call from Berlin. He refused the offer, remaining faithful to his native land until his death.

When Fleischer entered upon his professorship in Leipzig, in 1836, Arabic-Mohammedan studies had begun to flourish in all parts of Germany. Between 1819 and 1829 there had been published five of the *Mo'allaqât*, with the scholia of Zauzanî, by Kosegarten, Hengstenberg, Rosenmüller, and Vullers; in 1828, the text of the *Hamâsa*, by Freytag; between 1825 and 1831, the first six volumes of Habicht's Thousand and One Nights; and in 1828, Kosegarten's *Chrestomathy;* Freytag's Arabic Poetics (1830), and the first volume of his great lexicon (1830), as well as the beginning of Ewald's *Grammatica critica* (1831), had established the principle that the edition of texts should be prepared with due regard to the laws of the language. Meantime another German scholar—Frähn, in the service of the Russian Government, by his *Recensio* (1826), had laid a scientific foundation for Islamic numismatics. Dorn was beginning to assist the Petersburg investigator, and Hammer-Purgstall, unflagging, continued his magnificent work at Vienna. The last-mentioned scholars prove that the increased interest in Arabic subjects can not be traced entirely to outward causes, but should be connected with the renascence in Germany of philological studies from an historical point of view. On the other hand, the efforts of the first set of scholars depend entirely upon the work of De Sacy, who had been the teacher of Kosegarten and Freytag, and indirectly through the latter, also of Vullers and Hengstenberg. Even Ewald, independent though he was, and striving to master for linguistic purposes, the material bequeathed by Arabian grammarians, had to lean upon De Sacy in the development of the main features of his plan.

The times have been when it was customary, if not with Fleischer himself, at any rate with a few of his disciples, to treat somewhat contemptuously the efforts of men like Freytag and Hammer-Purgstall in behalf of Arabic philology. I myself must confess to the youthful folly of having, in my first very imperfect essay, spoken of Hammer in a way which even vivid remembrance of Ahlwardt's Chalaf could not excuse, certainly not justify. Deservedly, I was at once reproved by a more sensible fellow-worker.* Freytag was also judged unkindly, though perhaps not with equal severity. Even the numberless corrections which had to be made in his lexicon, and will of necessity ever continue to be made, cannot alter the fact that it was an eminent performance at the time of its compilation, and still remains an exceedingly useful work. Naturally, neither Hammer nor Freytag, neither Kosegarten nor Vullers can bear comparison with the master mind at Paris. The last three are docile disciples, who praiseworthy for industry, and estimable for attainments, do no more than follow in the footsteps of their master, without reaching him, even in their happiest moments. Hammer, on the other hand, was never more than a highly gifted dilettante, whose desire for novelties stifled the faculty of maturing ideas. His capacity for work was unbounded; its results however laid down in numerous volumes, but apparently solve the vast problems of history and literature. On his bold excursions, he often paved the way to fields hitherto inaccessible, but keenly discovered by him to be worthy of cultivation. It will always remain his distinction that he made it possible for us to gain a bird's-eye view over such fields, and cursory though it was, it is still valuable on all points in which detailed research has not replaced his superficial statements by more reliable data. Hence it is not astonishing that the Vienna Orientalist enjoyed undisputed fame and exerted great influence in the first third of this century. Thus the danger was imminent, that his virtues being inseparable from his personality, his pupils and imitators might after his death seize only upon his weak points and develop them into a radically false and highly dangerous system. It is doubtful whether any of the German representatives of De Sacy would have been able successfully to combat this method. Despite their merits, not one of the investigators named was distinguished by that combination of wide knowledge and philological accuracy which had marked De Sacy's work, and which—in case his death occurred as early as was feared,* would have to be the characteristics of a successor, whose influence was to counteract Hammer's. Kosegarten approached this ideal most closely, unless we except Rödiger, who if not totally independent of De Sacy, had at least not been trained in his school. But neither devoted himself exclusively to Arabic-Mohammedan philology; and the same objection, to a still stronger degree, holds of Rückert, whose interests were chiefly poetic. Thus it was Fleischer alone in whom the ideal was fully realized. To him therefore naturally fell the task of placing our science upon the same eminence in Germany that it had occupied under De Sacy in France,—a task rendered difficult by the necessity of guiding it so that it might permanently be rescued from the crooked path into which it might have been forced under Hammer's influence.

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* See H. Derenbourg, *Revue critique*, 1869, No. 35, p. 132.

It is impossible for me to judge whether Fleischer, at the time of entering upon the duties of his Leipzig professorship, had conceived his mission as clearly as we can now formulate it after its accomplishment. At all events, the two essays with which he introduced himself upon the arena of his future successes seems to bear unequivocal signs that this was his conscious goal. When he was no more than "Professor-elect of Oriental languages at the University of Leipzig," he published, while at Dresden, his translation of Zamachshari's *Golden Necklaces*, with a preface and notes, containing a sharp attack on Hammer's edition and translation of the same text. Almost at the same time he reviewed Habicht's glosses, in which there is surely no lack of grave mistakes. In this last review his tone was the mildest imaginable, and later, even when dealing with bunglers of the worst sort, he never became vehement. If then in opposing Hammer he made use of more violent language, he must have been actuated by serious and far-reaching considerations. In fact, he states them at the beginning of the preface in these words: "If highly esteemed scholars in possession of every facility, at a time when science has reached its manhood, give thoroughly useless work to the world, what should be the attitude of criticism? It is our opinion that its sharpest weapons should be directed against such abuses, and in this case it should combat even such as are really beneath criticism, in order that their becoming contagious may be prevented." The man, comparatively speaking a novice, who thus met a scholar, universally looked upon as the most eminent orientalist in Germany, must have felt the assurance of victory. The contents of his essay justified his bold language, and a still further justification was furnished by his *Dissertatio critica de glossis Habichtianis in quatuor priores tomos MI noctium*, which appeared in the following year (1836), on the occasion of his entering upon the duties of his chair. On account of the minutiæ of mediæval Muslim life described, the

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* He died in 1838.

"Arabian Nights," require complete mastery of the whole domain of Arabic-Mohammedan life, for a thorough understanding of all the difficulties that grow out of the language and the subject-matter. Of this mastery the essay testifies abundantly, as it does of the unerring philological tact of the critic, whose emendations by no means are the happy suggestions of an ingenious mind, but rather the results of wide linguistic and historical knowledge, and of intimate acquaintance with the habits of copyists and the manner of transmitting manuscripts.

For obvious reasons, it is not easy—and so far as I am concerned it is at this moment not possible—to trace the impression made by the two essays when first published; but their success shows that it must have been deep and lasting. The same complete mastery of the subject is displayed by Fleischer's next works: *Ali's One Hundred Proverbs* (1837), a description of the Arabic, Persian, and Turkish manuscripts in the *Catalogus Librorum MSS. Bibl. Civit. Lipsiensis* (1838), and the completion of Habicht's edition of the Arabian Nights (vols. IX–XII, 1842–'43). But the character of the subjects treated in these works was not calculated to confer controlling influence upon them. Likewise his edition in 1847 of Mirza Mohammed Ibrahim's *Grammar of the modern Persian Language* (2d edition, 1875), merely strengthened the impression derived from *Ali's Proverbs*, that this scholar had as wide an acquaintance with Persian as with Arabic. Thus, directly or indirectly, it must be due to these two short essays that Fleischer, as early as the fourth decade of this century, was freely acknowledged by all, excepting perhaps the immediate followers of Hammer, as the chief of German orientalists. In fact, from that time on for a period of nearly half a century, he became the chosen guide of all Germans and many foreigners, desirous of thorough disciplining in Arabic-Mohammedan philology. The impression created by these two works was so strong, because they are an exemplification of the true philological method for which the Germans, after the death of Reiske, the *vir incomparabilis*, once more had to resort to the Frenchman De Sacy,—a method which is nothing less than the use of common sense, coupled on the one hand with faithful, untiring efforts to attain to the greatest possible completeness and to scrupulous accuracy in the collecting and sifting of the material handed down to us, and forbidding, on the other hand, all arbitrariness, however ingenious, as well as all superficiality, however grandiose. This definition by no means puts an interdict upon cleverness on the part of the philologist thus gifted. It merely provides that cleverness must manifest itself in mastering the details acquired, not in speciously hiding the imperfections of scientific research and then satisfying one's conscience by a perfunctory though minute adherence to a traditional method. That Fleischer realized in himself this ideal of a philologist perhaps best marks his importance in the history of science; his example as well as his precept have made it possible for all to gain a knowledge of the correct method to be used in our department of research. Many of the most prominent scholars of the present day have in other ways arrived at a knowledge of this method, or have been intuitively gifted with it, but its spread to extended circles we owe to Fleischer, and to him alone.

His disinclination to treat philological subjects according to routine methods was shown by Fleischer in his edition of Beidhawi's Coran commentary, which completed his fame, and which, in a way is to be considered the most important work of his life. He, Gottfried Hermann's disciple, who surely knew what elements constitute a "methodical" edition of a work, published this voluminous and difficult text without any variant readings. I have elsewhere* shown what considerations, in my opinion, justly led him to adopt a system unusual even with himself. The character of a Coran commentary is, in every respect, technical. He who would understand and edit it must first have extensive and detailed acquaintance with the contents and technical peculiarities of the theological, juridical, and grammatical system of the Islam. But so enviable a scholar, aided by all available manuscripts and super-commentaries, certainly has the ability in every case to select the correct reading; and superfluous readings serve but to confuse less learned readers. The responsibility incurred by an editor who takes it upon himself to omit customary technicalities is proportionately great. But whoever heard Fleiescher himself interpret his own Beidhawi a single time, was forever delivered from all uneasiness on the score of his power to meet such responsibility. This edition is naturally not purged of every human imperfection. Fleischer himself, in a lecture on one occasion, expressed his vexation that after the feminine والحكاية the expression ليست الّا instead of the correct word ليس الّا had escaped him. (*Cf.* Fell's *Index*, p. 67.) But such instances assuredly are not numerous. On another occasion he related that a copy of his book—(I no longer remember how and when)—had been submitted to the Sheikh-ul-Islâm at Constantinople, and that the latter had considered it beneath his dignity to throw even a superficial glance at an ignorant infidel's disfigurement of the classical work of Mohammedan theology. Finally however he had opened it and glanced at a few lines; then, amazed had eagerly continued to read, at last giving expression to his astonishment that, in the Occident, there could exist a man who apparently understood Beidhawi as well as an orthodox doctor. I quote these remarks of Fleischer, which I heard myself, since I should consider it presumption were I to praise his Beidhawi. He only has the privilege of doing this who is so well versed in Muslim theology, that he might on a proper occasion, criticise it. Whether there are—outside of the Orient—a half-dozen scholars who may venture to make this assertion is doubtful; at all events, I do not reckon myself among them.

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* *Götting. gelehrte. Anzeigen.* 1884, No. 24, p. 961.

Aside from a small edition of the *Hermes trismegistus*, written in 1870, on a special occasion, Beidhawi is the last work published by Fleischer without assistance. Even Beidhawi was not complete when it left the hands of the publisher. For years the *Indices* weighed heavily upon his conscientious mind, until finally in 1878, they were brought out by the helpful aid of Fell. With reference to this unusual delay, Fleischer said in the preface with which he introduces his pupil's work: "*Qui me resque meas norunt, eos me ultro excusatum habere scio,*" and his meaning was evident to all. During the period while he was busy with the Beidhawi text, the claims made upon him from all sides had increased with his growing fame. These claims were put forth chiefly by three parties: his pupils, his co-laborers, and the community at large. His manner of satisfying them illustrates the most admirable traits in his character: extreme conscientiousness, faithful attention to the slightest details, affability and absolute unselfishness.

His conscientiousness and faithfulness were pre-eminently evinced in his academic labors, as I can testify from personal experience in the years 1867 and 1868. He knew Beidhawi thoroughly; daily, at any chance occasion, he excited admiration by his clear explanation of the doctrines of Mohammedan scholasticism, or by his equally correct way of tracing the history of a word and its development in meaning from the Arabic through the Persian to the Turkish,—all this without referring, except in rare instances, to his inter-leaved copy of Freytag, famous on account of its marginal notes, literally covering the text as well as the inter-leaves. Yet he never lectured on Beidhawi without preparation. His students, coming to attend a lecture at his study early in the morning, frequently found him standing by a high desk, the text and a copy of Sheikh Zâde's super-commentary lying before him. In his "Arabic Association," difficult passages in various texts were discussed, opportunity was given to gain practice in the reading of manuscripts, etc. But outside of this, he gave instruction, at the period spoken of, only in the writings of Arabic, Persian, and Turkish authors. The texts selected for reading varied, frequently according to the wishes of the students. But the two lectures a week on Beidhawi were inviolable. In these, he himself translated and explained, frequently cross-questioning his hearers, in order to assure himself that they had grasped his meaning and were making good progress. In the remaining four to six hours a week, Arabic, Persian, and Turkish texts were given to the students to translate, their translations being corrected and elucidated by the professor on the spot. In conducting this exercise his talk wandered from topic to topic, so that in looking back it appears that not the reading of the texts was of prime importance, but rather the wealth of information, relating to the subject-matter, chiefly however of a linguistic nature, which he fairly showered from out of the plenitude of his learning upon the eagerly listening and busily writing members of his class. Aside from the numerous additions to one's knowledge, his apparently irregular and digressive method of instruction possessed the advantage of at once ushering the student into the Mohammedan world of language and ideas, giving him a vivid conception of the wealth and pliability of the Arabic idiom, and most emphatically reminding him at every turn of the necessity of being accurate in the slightest detail. Naturally it was at the same time necessary to pursue private study systematically and unremittingly, and it was pre-supposed as a matter of course. He who could and would work, had to acquire rapidly a knowledge of the languages, and yield with docility to a training in habits of accuracy; the essentials of Arabic, indeed of all philology. The undeniable but doubtless intentional one-sidedness of this method is justified by the necessity of helping the pupils to ground themselves thoroughly in these fundamentals. If I have been correctly informed, Fleischer in earlier years delivered regular courses of lectures, as for instance on the doctrinal theology of the Islam. From this it can be inferred that his later method meant to lay increased stress upon the important and essential points which he had always emphasized. We were charged to acquire Arabic, Persian, and Turkish, and to rid ourselves radically of any tendency to superficiality. Having done that, we were prepared as far as our ability went to do independent and philologically accurate work in whatever special field any one of us might choose. However, even from this point of view, there is one more desideratum, apparently unprovided for in this method, namely, a knowledge of the technical working principles of philology, in any event a highly desirable equipment of the future philologist. But every one had the opportunity of acquiring them while preparing his thesis. For Fleischer's activity as a teacher was by no means at an end when he had appeared in the lecture room eight or ten times a week. His library, his knowledge, his talents were at the disposal of his pupils, and if any one of them in his first attempt at editing a text was perplexed by some difficult passage in the manuscript, he needed but to apply to his ever-obliging teacher to have the difficulty cleared away. Either he might content himself with carrying away the ready explanation or emendation, or if he attended intelligently, he might, in addition, derive the restricted number of principles and tricks of method, which, in fact, can be summed up in the direction to scrutinize carefully the manuscript to be explained and in the observance of the two main injunctions in Lehrs's philological decalogue: "Thou shalt not prostrate thyself before manuscripts," and "Thou shalt not take the name method in vain." Finally, when the time came for the young scholar to leave Leipzig, perhaps soon after receiving his degree, the bond that linked him to Fleischer was by no means severed. Whenever and in whatever way he wished he could apply to Fleischer for a solution of problems and difficulties, In his kindness of heart he was indefatigable in replying and explaining, often himself correcting proof-sheet upon proof-sheet. Each of us was sure to find in him as long as we lived a firm scientific support. I do not care to mar this remembrance of a teacher's touching unselfishness and faithfulness by questioning whether these characteristics were always appealed to with the reserve rendered doubly necessary by so ample a benevolence. He himself never gave this question a thought. He existed for his pupils as long as he supposed them at all interested in science. Therefore no one called him anything but "the Sheikh;" unless led by the exuberant spirits of youth, we translated the Arabic expression by "the old man" (which after all was indicative of our unbounded respect for him), for this Arabic title of honor conveys an idea of the parental relation existing between the teacher and the pupil, which is assumed as a matter of course in the Mohammedan East.

But he was not *our* "Sheikh" alone. Long before I was permitted to become one of his disciples, he had been acknowledged the "sheikh-ush shuyûkh," the master of masters. To a certain extent it was natural that he should have come to occupy this rank. His pupils had developed into co-investigators, and they could not well entertain the idea of supplanting him. But great as was their number, there was still no lack of men, who, having been disciples of Ewald, Rödiger, Freytag, and others, might preserve their independence. In a still higher degree this was true of partial contemporaries, such as Dorn and Rödiger. No one will deny that this state of affairs was salutary for our science. Under all circumstances it is baneful for one school, no matter how excellent its principles or its representatives may be, to exercise autocratic sway in a given domain. In some respects it must be one-sided, and one-sidedness is fatal to science. Now, from what has been said of Fleischer as a teacher, it follows that nothing was further removed from his mind than to force his pupils into a narrow-minded course. If nevertheless any one is disposed to harbor this opinion, let him but read Fleischer's preface to Behrnauer's translation of the *Forty Viziers* to learn differently. But as was natural, his disciples at first had to abandon themselves to his guidance. The necessity was constantly arising to refer them to the Arabic grammar, when once they began to do independent work in the preparation of texts, always of a grammatical nature, since such are easiest for a well-trained Arabist. Thus it had to come about that for a time Arabic grammar seemed to thrive almost too luxuriantly in these circles. Since then it has become apparent that the danger was not very great. It must be conceded however that its complete avoidance was greatly due to the efforts of those scholars who remained independent of Fleischer; that is to say, independent of his instruction, not of his influence. It could not be gainsaid; he was the most learned of the learned, the most accurate of the accurate. It is therefore not remarkable that the recognition of his scientific superiority, readily yielded by all prominent scholars, with one or two exceptions, gradually led to the establishment of personal relations, in which he always gave more than he received. His co-laborers in Germany, as well as in more than one foreign land, by degrees grew accustomed to ask his advice, claim his help, which he granted to strangers as freely as to his own pupils. Thus it happened that for many a year no Arabic text of any importance was printed in Germany without owing to him considerable improvements, and likewise more than one valuable work by foreign Arabists has received similar aid. Sometimes he revised the proof-sheets as they were printed; sometimes, after the appearance of single volumes, he arranged the notes, taken during its careful perusal, so that they might profitably be used in appendices, possibly to be added. There is quite a library of Arabic writings, in the building up of which he has thus participated. Here are some of the important works, selected at hap-hazard: Amari's *Bibliotheca Arabo-Sicula*, Juynboll's *Abulmahâsin*, the *Makkari*, Tornberg's *Ibn el-Athîr*, Wustenfeld's *Jacût*, Flügel's *Fihrist*, Wright's *Kâmil*, de Goeje's *Bibliotheca Geographorum*, Jahn's *Ibn Ya'ish*. This critical work was naturally accompanied by an extensive correspondence, which took the more time as it was conducted with an almost exaggerated conscientiousness. But in no other way could these numberless connections have been maintained so regularly and so steadfastly.

As Oriental studies advanced in Germany the necessity of establishing closer connections between the representatives of the different departments was keenly felt early in the third decade of this century. To effect a union of this kind Ewald, Kosegarten, Rödiger, Rückert, and some others established, in 1837, the *Journal for the Science of the Orient* (*Zeitschrift für die Kunde des Morgenlandes*). From 1838, the philologists' conventions offered place and opportunity for personal intercourse between men in all departments of Oriental research. Thus Rödiger was but giving shape to an idea that had long been entertained when he proposed on the occasion of a visit, in September, 1843, at Fleischer's house, where Pott, Olshausen, von der Gabelentz, and Brockhaus were also present, that German orientalists, as a body, should hold sessions annually in connection with the conventions of philologists. As is well known, this plan was executed in 1844, at the Dresden meeting. The consultations held there resulted in the formation, at next year's meeting, on October 2, 1845, at Darmstadt, of a German Oriental Society, modelled after the Société Asiatique and the Royal Asiatic Society. The Journal of the new society absorbed, in 1847, the *Zeitschrift für die Kunde des Morgenlandes*. From the first, Fleischer displayed zealous interest in the plan. The Dresden council was held under his presidency, and the first draught of the constitution issued from his pen. His certificate of membership was the first conferred, and, up to the time of his resignation from the governing body, in 1880, it may be said, without disparagement to many faithful and deserving men, that he was the soul of the association, unselfishly, as always, devoting his best powers to the common good. His help was given wherever it was needed; he served as editor of the Journal, and again as chronicler of the year's work; he was called upon to pass judgment on works that were to be published under the auspices of the society, and to correct them; and sometimes he had to act as mediator between opposing parties that had sprung up within the society. The society thus became dear to his heart, as does a child that has been raised with care and trouble to man's estate. Nothing (unless it were a falsehood) vexed him so much as an injury done the society, or failure to fulfill punctually the duties imposed by membership. While occupying the position in the governing board of executive in matters relating to the library, he took upon himself the unpleasant task of making a quarterly list of all books and pamphlets that had been sent to him, and inclosing it in the chest of books forwarded to the librarian at Halle. Later, when direct communications between the library and the correspondents of the society were established, this work was no longer necessary. Up to that time, while I was librarian of the society, many a list of that kind passed from his hand into mine. I do not remember ever to have found an error in a single one; but I know that I often wished that he would not waste precious time on unimportant work, for which he might have found dozens of willing hands near him. But he would have eyed with suspicion the man who would suggest that he should transfer to others what he considered his own duty. Undoubtedly he was right, for the society would never have turned out to be such as it is if he had not had so conscientious a conception of duty. He was repaid by the pleasure of seeing it grow and thrive; soon it could fitly range itself by the side of older associations abroad; and among the learned bodies of Germany it occupied a respected position. On one occasion, to be sure, this position caused him much unpleasantness, namely when the directors had to advise the Prussian Government as to the purchase of the Moabite antiquities, which subsequently proved spurious. This is not the place and nowhere is it concern of mine to raise anew the dust under which this unfortunate affair has finally been buried. The proper conception of the province of a business committee is expressed in a resolution, afterwards adopted by the general convention of the German Oriental Society: "In consequence of the position assigned by the constitution to the board of directors of the society, any opinion published by them on scientific and more particularly on disputed points, cannot be construed to express the opinion of the society."* Fleischer may have permitted a fatal mistake to be made, but he afterwards generously assumed more of the responsibility than was necessary.

Surely no one who once more passes in review his extensive and varied work, even in the incomplete survey that I have just made, can find reason to doubt the truth of what Fleischer further says in the above quoted passage from his preface to Fell's *Indices: "Ceteris adsevero otium mihi et vires defuisse, non voluntatem et studium."* Certainly then it was not possible for him to find leisure for the preparation and execution of comprehensive works embodying the results of independent research. The translation of the Coran, the work of many years, was left uncompleted. However, not all his powers were absorbed by his efforts in behalf of his pupils, his colleagues, and the learned world in general. He devoted every leisure moment to his appointed task of maintaining Arabic-Mohammedan philology upon the eminence to which De Sacy had raised it, and if possible of elevating it still higher. He diligently continued up to the last moment the critical work that had opened new paths to science upon his first appearance. For a long time he wrote reviews of new books in the *Hallische Litteraturzeitung*, in Gersdorf's *Repertorium* and in other journals, but afterwards exclusively in the *Zeitschrift der Deutschen Morgenländischen Gesellschaft.* For the readers of the *Beiträge*, special mention may be made of his detailed notices of the re-modelled edition of Rückert's *Poetics and Rhetoric of the Persians*, and of Bacher's edition of Sa'di's short poems, both republished in the third volume of his *Minor Works.* Besides, he contributed extensively to the improvement of the various editions of Arabic texts, especially of Makkari and Abulmahâsin, and wrote a number of short articles on chance topics connected with Arabic, Persian, and Turkish literature, history, and archæology, as they were suggested to him by hints in his correspondence, in his official work for the German Oriental Society, etc. Two great series, by far the most important in a mass of highly instructive material, must be noted: the celebrated contributions (*Beiträge*) to De Sacy's *Grammaire Arabe*, and those to Dozy's *Supplément aux dictionnaires arabes.*

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* *Zeitschrift der Deutschen Morgenlandischen Gesellschaft,* vol. XXXI, p. xv.

"*Grammatici Arabes utilissimi nobis* (*sunt enim thesauri formarum totiusque antiquitatis promi condi*)" was the opinion of Ewald.* It is perhaps De Sacy's greatest distinction that he put Arabic philology upon this basis, and no less deserving of praise is Fleischer for having continued and supplemented this work in a spirit of modesty and life-long devotion to his beloved teacher, aided by the superior knowledge which he had learned how to acquire in the school of De Sacy. Those endowed with unusual talent, and furnished besides with a peculiar gift for the Arabic language, may succeed in understanding Arabic, and in avoiding all the hidden snares in the characters the vocabulary and the syntax, laid for the guileless reader by this most treacherous of all languages with which I am acquainted. But the average scholar is lost, that is to say sinks back upon a lamentably low stage of philological development, unless he masters thoroughly his De Sacy with Fleischer's additions. That diligent and willing students are no longer exposed to this danger of retrograding we owe to "the old man." And the place filled in its time by the worn, interleaved Freytag, or that filled in the domain of grammar by De Sacy with the "*Beiträge*," is occupied, on the field of lexicography, by Dozy's *Supplément*, enriched by Fleischer's corrections and additions. His "Minor Works," covering, together with the others mentioned, 2225 pages, are a legacy, the conscientious use of which will, for a long time, continue to be the prime duty of every scientific Arabist.

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* *Gramm. crit. ling. Ar.*, I, p. iv.

We should use it however not only conscientiously, but also with the most grateful remembrance. We should always bear in mind that Fleischer, in order to become for his pupils and co-laborers what he was to them, refrained from working for himself except by working for others, and this at a time when his powers were at their height and his comprehensive learning in its ripest state.

Possibly many a one has shared the feeling of a prominent and clever co-laborer of mine, who said to me some years ago that it vexed him to think that Fleischer, with his magnificent learning and ability, was deserting from the solution of the highest problems. I can not agree with my nameless friend. Diverse gifts—one mind. Some, venturing fearlessly abroad, are permitted to discover new domains; others secure law and order at home. Not the one by itself, nor the other is the desideratum. The one cannot stand without the other. When Fleischer came upon the stage we stood in need of law and order, which he secured. Now, let the venturesome go forth upon voyages of discovery; the less talented will still do well to remain at home and watch lest law and order be undermined. Certainly it would have been a great achievement if, for instance, our sheikh had built up the edifice of Islamic doctrines for us. But has he not done better in sharpening tools for many generations of workers, so that now they may themselves build, not so quickly and not so high, but on a broad base and with many wings?

"Let me say briefly that from my early youth I have dimly felt the desire and hope to cultivate myself, my whole self, such as it is," writes Wilhelm Meister to his wise friend Werner. Man's duty with regard to his own gifts has never been expressed more pertinently. Fleischer's was a sagacious, acute, and sensible mind. He in nowise sympathized with what is mystic and ambiguous. A critic by nature, he exercised his critical faculties not only upon others, but also and chiefly upon himself. Besides, he was faithful to duty, a lover of truth, benevolent, humbly self-sacrificing, and single-minded. Not one of these natural traits did he fail to cultivate conscientiously, nor did he ever attempt to lay false claims to virtues which he did not possess. A man of this kind could not fail to see that only by means of self-restraint can one succeed in perfectly cultivating one's own nature. In no respect,—neither in his views, nor in his studies,—was he one-sided; but he knew accurately wherein his strength lay, and was too sensible to sin against the proverb: "*Qui trop embrasse, mal étreint.*" "*Il ne faut pas courir deux lièvres à la fois,*" he once wrote to me,—(he often delighted in using the French language, which he had mastered perfectly), and according to this principle he consistently arranged his scientific career. In his preface to the *Golden Necklaces* he says clearly and decidedly: "In Arabic research neither good will, nor diligence, nor penetration of mind, nor ingenuity, nor outside philological attainments, nor anything in the world, can relieve one from the necessity of modestly, faithfully, and diligently studying with the Arabic philologists,—and here in Europe, above all with our master De Sacy; however, I do not mean to imply that Ewald and his compeers will not in time succeed in summarizing the superabundant material of Arabic philology in a more fitting and convenient form, as well as in explaining many facts in a more scientific way." The justification for Ewald's philological methods does not escape his notice, as Ewald in turn admits that the Arabian grammarians are the *promi condi totius antiquitatis.* But Fleischer avowedly limits himself to the purely philological side of the task, for *il ne faut pas courir deux liévres á la fois.* Only once did he deviate from this rule, and then it was done in order to venture upon a neighboring domain that could not well be avoided, that of general Semitic etymology: He that wishes to cast a stone at him on this account may do so after consulting St. John viii, 7. To this wise self-restraint, among other things, he owes his preeminence upon that field of philological research which was chosen by him, or which (if you will) naturally fell to his share. At all events it is hard to believe that any other field would have given the same scope to his natural abilities. The undeviating conformity to law that characterizes the structure of the Arabic language and its perspicuity naturally appeal to him, as on the other hand its boundless wealth and apparent complexity of linguistic phenomena offered welcome problems for his ingenuity to solve. He was thus, by right of birth, the expounder of the Arabic poets and writers, whose peculiarities are analogous to those of their language. This partial affinity (for in other respects, he was a true German with very un-Arabian feelings), together with his linguistic attainments and large information, permitted him to reach the incomparable skill and certainty in the criticism of Arabic texts which for the time at least did more than anything else to shed luster upon his name. Theoretically indeed there is no difference between the proper philological treatment of a Greek or Latin and an Arabic or Persian text. But many external circumstances connected with Mohammedan literature, such as the relatively short period intervening between the original writer and the manuscripts to be studied, the peculiarities of Arabic characters, etc., cause less stress to be put in our specialty upon the *recensio*, if I may be permitted to use technical terms. In some cases the *recensio* is the most essential part of the work; in most however it is very unimportant. With us it is the *emendatio* that taxes the critical faculties to the utmost. Similarly, ours differs from classical philology, inasmuch as conjectures with us are usually either entirely correct or altogether incorrect rarely—*probabilis.* Hence it may be said that, aside from mere copyists' blunders, it is easier to make conjectures in classical philology, but, on the other hand *ceteris paribus* it is easier for us to make correct conjectures or emendations. For both reasons we are not justified in resting satisfied with the mere *recensio*, as our Græco-Roman philologians may sometimes do. It follows that a man like Fleischer may not be disposed of by praising him as the lucky possessor of a talent for conjecturing, and then casting him into the great lumber-room, where the superannuated philologists' apparatus is stored. True, he is the author of thousands of conjectural corrections, but at least two-thirds of his conjectures, if this measure of worth be applicable, are emendations. Whoever admits this, will thereby agree with me in saying that self-forgetful work limited by wise self-restraint, and undertaken with a definite aim in view, is as a matter of course and almost in opposition to the wishes of its author, rewarded with the prize.

It is time to conclude. Fleischer's prominent position in the history of our science is due to these circumstances; by precept and example he made a home in Germany for the scientific study of Arabic Mohammedan philology; he trained generations of scholars with this purpose in view; he similarly influenced his co-laborers in Germany and abroad; he doubled the sum total of all the results reached by De Sacy in the special field of Arabic language and literature, and by his help the work of his contemporaries was raised to the eminence occupied by his own. There was no lack of prominent scholars in his own department, nor of such as took up and supplemented his work outside of the limits he himself had drawn. Still he and no other can be called the true heir and successor of De Sacy. In knowledge and ability he excelled his great teacher. But he himself would have administered a sharp reproof to him who might venture to rank him above his master, in scientific matters: "Honor be to him who leads the way." الفضل للمتقدم

The unstinted recognition yielded to the great scholar on all sides was commensurate with his deserts. The most prominent Orientalists of Germany and others of foreign countries readily acknowledged that his was unequalled knowledge and ability; one learned society after another conferred upon him honorary membership, and to several Saxon orders and the Turkish Medjidié were added the two highest scientific distinctions in the giving of Germany,—the Bavarian order of Maximilian and the Prussian *pour le mérite*. For a long time it seemed as though age itself could not impair the octogenarian's vigor nor destroy his love of work. However, in the spring of 1884, there appeared the first symptoms of an abdominal disorder, which gradually grew. But whoever saw him when he was not troubled with pain, scarcely noticed any change in his appearance,—none whatever in his manner. On October 19, 1885, I enjoyed the privilege of participating with many others in the celebration of the fiftieth anniversary of his official connection with the university, and on October 4, 1886, when I again visited him during his stay at

Neu-Schönefeld, the entry in my diary reads: "Fleischer as bright as ever." But in 1886, he was compelled to avail himself of the permission granted him, on the occasion of his jubilee celebration, to omit the lectures of the summer session, and the physician's orders were constantly limiting the amount of work he did. When again I visited him at Leipzig in October 7, 1887, I felt that I should have to bid him an eternal farewell. In spite of his increasing debility he began a course of lectures for the winter session, and continued them until November 17. But on November 18 he took to his bed, never again to leave it. He bore the pain entailed by his disease with admirable patience; no complaint ever crossed his lips, until on February 10, 1888, a short while before completing his eighty-seventh year, death released him from his suffering.

The prominent features of Fleischer's character were truthfulness, conscientiousness, unselfishness, and punctuality. I was never able to decide how much he owed to nature, how much to the strict self-discipline exercised in early years. But whatever he had acquired by habit had come to be a part of his being. He became indignant nay wrathful, the kindliness that marked his features and sprung from good nature in the best meaning of the word, seemed to leave him,—when he met with falsehood, carelessness, or lack of punctuality. As long as there were no evidences of want of truth on the part of others, he was unsuspicious, sometimes too much so; but whoever shocked his delicate sense of justice, had good cause to fear his anger. Yet there was not a trace of dogmatism in his nature. He may in some instances have chanced to form an incorrect judgment of certain people, but he took the first opportunity to change it most willingly in their favor, unless weighty reasons existed for the contrary. All that he thought and did was characterized by objectivity, pure and simple. In scientific debates he demanded that his conclusions be tested impartially, and on the other hand he accepted instruction from the youngest of his pupils, if he had chanced to find something that had escaped the notice of "the sheikh." His polemics were never of a personal nature except when Ewald accused him, in a manner that even now impairs the reputation of this great man of "being actuated by sordid impulses in science." In a published "statement addressed to Prof. Dr. Ewald of Göttingen," he expresses in plain, though moderate terms, his just indignation. His misunderstanding with Dozy, whom Fleischer had unintentionally offended, was cleared up in a way that reflects credit upon both scholars. He was conscious of his abilities and his achievements, but never boasted of them. To all work done by others, in his or their department, he gladly yielded recognition. Unhesitatingly he subordinated himself in every respect to De Sacy, and to Lane's knowledge of the Arabic, as (in his opinion) superior to his own. He was never ambitious of empty honors, he never sought to assert himself.

What was called Fleischer's school, can scarcely be said any longer to exist as such. Arabic studies, the preponderance of which formed the most distinguishing mark of its unity, have been curtailed in Germany. A cruel fate has prematurely removed the very best philologists of Fleischer's school: Ralfs, Loth, Spitta, and, furthermore Kosut and Huber. Some of us have struck out on new paths; general interest has been diverted to Assyriological research and to comparative philology. The leadership in the Arabic domain is about to pass over to the Dutch school. But it matters not what we do, if only we emulate the example of "our sheikh," and do disinterested, honest, diligent, conscientious, and modest work, in whatever is within the reach of our limited ability.

A MEMOIR OF GUSTAV ROBERT KIRCHHOFF.*

BY ROBERT VON HELMHOLTZ.

Translated by JOSEPH DE PEROTT.

On the 20th day of October of the past year (1887) we bade our last farewells to Gustav R. Kirchhoff in St. Matthew's Cemetery at Berlin. Natural science has lost one of its mightiest promoters, Germany is bereft of one of her keenest thinkers, the youth lament their honored, brilliant master, and his friends mourn over a man who belonged to the best, in the true meaning of this word. While Kirchhoff's works made his name immortal, so that wherever physics is taught he will be mentioned, such were his modesty and simplicity that his own person was hidden behind the object to which he devoted his life, and if we except his colleagues and those who had the fortune to be near him, there were very few who knew more than that Kirchhoff was the illustrious discoverer of spectrum analysis. Let one of his students be permitted to attempt to do what he would never have undertaken himself and what even would have been painful to him while he lived,—to draw a picture of his work not in its pure, abstract form, destitute of all earthly vesture, as he produced it, but rather in connection with his personal life, and as a fruit of his personal genius.

Gustav R. Kirchhoff was a professor of *mathematical physics*. I mention this first, not because it is the main fact which would stand first in a biographical dictionary, but because mathematical physics is a science of which only he who was born to it can become an adept. There are vocations in life, there are branches of science that do not allow us to infer what spirit animates their adepts. In certain regions of abstract science however, whoever wants to penetrate into them, must have faculties and dispositions of definite nature and bias, otherwise he will not even cross the threshold that leads to them.

Pure *mathematics* is such a science. Every-day experience teaches us that only a small proportion of students are endowed with a genius for it. It is more difficult to say on *what* powers of the human mind such a genius rests. Mathematics is logic applied to numbers and extensive magnitudes. It requires accordingly a great power of abstraction and the faculty of intuitive perception of relations of magnitudes. At any

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rate, just because the technics of pure logical thinking have to be developed to a great extent, the perceptive faculty of a mathematician, his judgment and his representation of things are of a peculiar kind.

The natural philosopher requires however another faculty still, I mean the faculty of observation. Every one whose work rests on observation is a student of nature in the widest meaning of this word; the physician, the traveller, the collector. To observe is to notice, and to collect what you have noticed. In proportion however as the collecting of things is done according to higher and higher standards, observation comes nearer to thinking, collecting approaches interpretation, and natural history verges into exact study of nature. The adepts of natural science work not only through the senses by means of observation, but also by means of the logical faculty of drawing inference. They differ from mathematicians chiefly in the material for their thinking being given in the external world and that they must have the talent to find it there, while the foundations of mathematics seem to be given *a priori.* Mathematics is the most convenient instrument in the exact science of nature because it is the tongue in which the latter can express its conclusions in the quickest and most precise way. That is why the exact study of nature becomes more and more mathematical; physics, after astronomy, has made the most progress in this direction; chemistry is about to follow it. Speaking generally, the greatest physicist nowadays will be he who is endowed equally with the gifts of observation and with logical precision of thinking, and has mastered experiment as well as mathematics. According to the pre-eminence of the one or the other faculty the place of each investigator will be nearer to the observers of nature or to the thinkers about nature. Both kinds are necessary, the latter is more seldom met with, there are more good observers than good thinkers. Gustav R. Kirchhoff belongs rather, according to his nature, to the great thinkers, and still his greatest and most celebrated discovery is a discovery of observation. He was one of the greatest natural philosophers just because he was a *mathematical physicist* in the above-mentioned sense.

The life of Kirchhoff was that of a thinker, too. He did not travel all over the world to see nature in the splendid attire of her multifarious productions, like Humboldt or Darwin; he did not work his way to theory through a school of purely practical life, like Faraday or Siemens. No more did he pass his life in the whirlpool of historical or social events. He accomplished his work quietly in the externally serene, but internally the more active, abodes of science,—in the lecture-rooms and laboratories of several German universities. Whoever wants to know him must follow him thither into spheres of thought that lie afar off from the interests of the day.

Gustav R. Kirchhoff, son of the lawyer, was born (1824), brought up, and educated at Königsberg, the "City of Pure Reason." According to a certificate from the Kneiphof High-school, he wished to devote himself to mathematics, and in fact he commenced the study of it under Richelot, and the elder Neumann. The latter, at first a mineralogist, and afterwards gradually becoming one of the great founders of the mathematical physics of our time, had a decisive influence on Kirchhoff. The student took to physics too, and helped to build up his master's structure. While still a student, Kirchhoff wrote, in 1845, an excellent original paper (On the flow of electricity through a circular plate), and was granted a scholarship for a scientific journey to Paris. The disturbances of the year 1848, prevented him from going any farther than Berlin, however. He stopped there and qualified for a professorship in mathematical physics. Strange to say, the first course of lectures of a professor who afterwards attracted hundreds did not take place. Mathematical physics appeared at the time a very remote and abstract subject. In the year 1850, Kirchhoff went to Breslau in the quality of an adjunct professor, and in 1854, as a full professor to Heidelberg, so that he went through the usual career of a German professor.

The bloom of his life was the twenty years he lived and taught at Heidelberg. These years fell into the most brilliant period of the most beautiful of German universities, and Kirchhoff himself contributed much to the increase and preservation of Heidelberg's fame.

Indeed, when Kirchhoff came to Heidelberg, the University of that town held an undisputed rank as the foremost of the German universities, through the renown of its teachers in law and history. A. v. Vangerow exercised an incomparable attraction through his celebrated lectures on the Pandects; at his side worked men like Wittermaier, Renaud, Mohl; the historians Schlossen, Weber, Gervinus, Häusser have a world-wide renown. They raised the level not only of scientific —but even of social life to such a high standard that all who partook of it preserve forever the recollection of those days. A circle arose around Häusser in particular, which took its first beginning from political grounds, but became afterwards the seat of an enchanting and cheerful conviviality. Among the scientists, Kirchhoff's predecessor Jolly, the anatomist Henle, the clinical physician Pfeuffer, all belonged to this circle; and Bunsen, who was already famous when he came in 1852 to Heidelberg, was one of its foremost members.

Robert Bunsen, whose friendship with Kirchhoff became as eventful in the annals of German science as that of Gauss and Weber, made his acquaintance at Breslau. It was through Bunsen's influence that Kirchhoff received a call to Heidelberg.

The large public knew nothing of Kirchhoff at the time his Berlin and Breslau papers could only be appreciated by his fellow-physicists. There was a great surprise at Heidelberg accordingly, when—heartily recommended by Bunsen—there came an unusually young, gentle, shy and modest North German. His fine, spirited talk, his amiable manners full of courtesy to every one, and his keen sense of wit and humor won the hearts of those who came nearer to him. Kirchhoff became accordingly a favorite guest at the cheerful meetings of this circle at Hausser's friends. But it was with Bunsen particularly that Kirchhoff came into a close connection in the first years of his sojourn at Heidelberg. Bunsen was his elder by thirteen years; strong, broad shouldered, of a more vivacious temper and of a more immediate influence, Bunsen struck with awe one and all by the plenitude of his powers. Thus the two men were in exterior very different from each other. It is a fact however that Bunsen and Kirchhoff not only accomplished together their great works, but even spent together their bachelor days as true friends. They took trips together to the magnificient environs of Heidelberg, they travelled together during the summer holidays, and even could often be seen together of an evening at the small Heidelberg theater, an amusement in which Kirchhoff particularly took a great delight from the days of his youth.

They did not part company, as is usually the case, even when Kirchhoff, towards the end of the sixth decade of our century, married the young and charming daughter of his Königsberg Professor Richelot. It was in fact during the years 1859–'62 that the two investigators, starting from a research of Bunsen, made and accomplished together the great discovery of spectrum analysis.

Towards the beginning of the seventh decade Kirchhoff moved, at the same time with my father, to the newly erected Frederick Hall, the first great institution in Germany devoted wholly to the furtherance of resources in natural science. It was an external manifestation of the fact that the center of gravity of the Heidelberg University gradually shifted from law and history to natural science and medicine. The philosopher Zeller, the mathematician Hesse, afterwards Königsberger, the chemist Kopp, the clinician Friedreich, my father as physiologist, all received calls to the institution. The Frederick Hall, became a kind of branch university. In this building I spent the days of my childhood; Kirchhoff's apartments, as well as the apartments of my parents under them, and the whole Frederick Hall, coalesce into one image in my memory. Large lecture-rooms and museums, with enigmatical ——ological names, stuffed animals, chemical and anatomical smells, acoustic sounds, then crowds of students (Russian lady students among them) overflooding at regular intervals the passages and the yards, to the great annoyance of children, while going to hear lectures by their (the annoyed children's) fathers,—these are the impressions that time has left me.

Kirchhoff spent there happy years. His name was already famous through his discovery of spectrum analysis, so that his laboratory and his lectures became the most frequented ones. With his wife, his four children, and his nearest friends, he led a happy life made cheerful through convivial intercourse.

Unfortunately these in every respect pleasant circumstances came to an end already towards the close of the seventh decade. In consequence of a fall on the staircase, he suffered from a sore foot, which compelled him for a long time to move only on a rolling-chair or by means of crutches. It was only at Berlin that he acquired again, after many relapses, his power of locomotion, but even after that he enjoyed his complete health only occasionally. He lost his wife about the same time, so that his family life broke asunder. Some of his friends (Häusser, Vangerow) died; others, like Feller and my father, received calls to Berlin. But accidents to his person could endanger his life, not his work. He continued to perform his task as a teacher and an investigator under the most difficult circumstances and after most severe trials, with a stoical faithfulness to duty and with iron consistency. His own person and his science should have nothing to do with one another.

Afterwards Kirchhoff married, as a second wife, Louise Brömmel, at the time matron of the university clinical hospital for the diseases of the eye. His inexhaustibly cheerful and amiable temper made this second marriage a happy one too, notwithstanding his frequent ill health.

In the year 1875, Kirchhoff received and accepted a call to the University of Berlin, after having refused previously an invitation to become a director of the projected solar observatory at Potsdam.

Whether a life at Berlin is to be considered as an advantage for a scientist may be doubted. The *teacher* acquires a larger, richer field for his activity, but just so much more is there loss of time for the investigator. Kirchhoff, however, owing to his infirm health, suffered but little from the turmoil of the capital. He did his work as usual; he published, as he used to, a paper or so every year in the reports of the academy; he did experimental work too in the laboratory of his friend, G. Hansemann. He it was who, after continuous separation from Bunsen, stood nearest to him as a co-worker and friend.

But the most favorite and admirable work of Kirchhoff at Berlin (in fact unique in its effect) was his course of lectures on mathematical physics. His delivery captivated one and all through its external finish and the precision of exposition. Not a word too little or too much; he never bungled, hesitated, or made himself guilty of a want of clearness. The terseness of his calculus was truly admirable,—a quality not easy to explain to an outsider. The whole subject rose before a hearer in the shape of a highly artistic, classically perfect frame-work, in which every part could be logically deduced from some other, so that it was even an æsthetic pleasure to follow Kirchhoff's deductions. In fact Kirchhoff's lectures though intrinsically they belong to the most difficult, ought to be intelligible to every one—even the less gifted—provided of course he is acquainted with the instrument used, the mathematical language. It may happen, and it happened often indeed, that one was not able to see the arrangement of what was put before him, could not understand why and to what purpose Kirchhoff made such and such a deduction, but to follow the train of his master's thoughts, to think the whole over and to reproduce it afterwards was within reach of every one.

Paradoxical as it may seem, it was not impossible, without ever having understood Kirchhoff, to write his lectures as a first-rate book by means of the notes alone. It is to this quality of Kirchhoff's dialectics (absolute clearness and self-comprehension), that he owed a large part of his success as a teacher. During nine years Kirchhoff was able to deliver his lectures at Berlin without interruption. But it became more and more apparent to us, his hearers, what exertion they required from him and how he was obliged to gather his last strength in order to keep himself on his feet. Nevertheless he was always punctual to the minute, and the excellence of his lectures remained unimpaired. At last (1884) he was prohibited lecturing by the physicians; he took up however this favorite occupation of his once again for a short time. It became apparent however that palsy made him unable to move, and Kirchhoff was reduced entirely to his own home, to the rolling chair, and to the care of his family. In the last two years of his life one would see him always cheerful and amiable, sitting in his arm-chair and preserving a vivid interest in all problems. Never, not even once, did a complaint escape his lips, though he must have been well aware of the decline of his forces. Death, which came unawares during his sleep, delivered him from worse suffering.

We lost in him a perfect example of the true German investigator. To search after truth in its purest shape and to give it utterance with almost an abstract self-forgetfulness, was the religion and the purpose of his life. He loved and furthered science only for her own sake; every embellishment exceeding the limits of what was logically proved, would appear to him as a profanation,—any admixture of personal motives, or grasping at honors or lucre, would seem to him worthy of blame. And in life as well as in science, he carried out what he considered his duty as a man, a citizen, or functionary, with a logical rigor divested of all personal motives. But the knowledge of good alone does not make a man a good one, not even the will or the power to execute it. It was only Kirchhoff's kindness of heart and humaneness, which if not demonstrative and warm in the expression of feelings, were the more pure and genuine, that made of him a true friend, a self-forgetful coworker, the teacher ready to help, the judge ready to acknowledge the merits of others; in short, a man that all of us loved. I have a fine instance lying before me of how friendly and obliging he was, even toward the humblest of his fellow-men. A poor workman—many would have taken him to be insane—applies in a letter to Kirchhoff, for an explanation of pessimistic doubts that torture him. "No physician, no priest, or any other materialistic egotist can help me, but only a man of a truly scientific educational training, an investigator and thinker himself, who does not consider himself too much above any of his fellowmen, placed below him by birth and circumstances, to communicate his conviction free of any compromise. When people tell me I am a workman and must not trouble myself about such mattters, I answer that not all men are alike; that in all classes of men are individuals that have not only material, but also spiritual wants. Not all sciences that are known were developed by scientists alone," etc. Many a one would have simply laid aside the workman's letter. Kirchhoff however wrote to him a well-considered reply, as the minute shows, where among other things we read: "That there are such limits to our knowledge of nature, must be borne with patience by every sound mind whether he be a scientist or a workman. I can only advise you to leave off all impossible aspirations and trying to conceive things that are beyond conception. This requires a struggle, but a struggle is the lot of many men of all professions. The best help is to devote one's self to the task which has fallen to one's lot, and to fulfill the duties of the position in which one is placed." And, in fact, Kirchhoff fulfilled himself the duties of his position. He was really "the truly noble mind, free from all egotistic sham," the workman was looking for. As for us, we are only inclined to ask which to admire more, the greatness of his mind or the strength of his will that lifted him so high above

"The vulgar, which we all, alas, obey!"

We have tried to portray Kirchhoff as he appeared to us, his contemporaries, as a man and as a teacher. His works will outlive him and will be appreciated according to their merit only by posterity. To us, his pupils, falls the task, even if we do not belong to physics, to make apparent what science owes to him. One is apt in such cases to lay the chief stress on the practical results of his works, to adduce their influence on technics and industry. While speaking of Kirchhoff's works one must however keep free from such a bias, first because the chief value of many of his papers lies not in the application but in the method; secondly such considerations would have been antipathetic to his own mind. Kirchhoff never asked himself "What is the use of thy brooding and searching?" What he had to expound he expounded in the way best suited to the thing itself, and in as general a manner as possible, without paying any attention to accessory purposes. "I think I have found such and such a thing, and I take the liberty of giving a demonstration of it in what follows." Such is the beginning of the most of his papers. His writings are less voluminous than might have been expected. His forty papers—product of as many years—are collected into a volume of moderate dimensions. He published besides, a report on his "Researches on the solar spectrum and the spectra of the chemical elements" and a volume of lectures on mechanics, the latter his most mature and perfect work.

What an immense amount of brain work is here condensed into the smallest space possible! Kirchhoff's style, like his delivery, was a model of the most clear and concise diction, absolutely classical in the subject concerned. The words stand as if hewn in stone, each one at its place, the logical comprehension of each duly considered; we find here condensed into a few lines what would have taken others pages to describe; only when the existing words seemed not precise enough, he uses circumlocutions and definitions, and that mostly in mathematical language. He held the highest rank among those who strove to remove from exact sciences all want of clearness, all subjective judgments, all phrases. The influence of such an endeavor will transcend the limits of his particular science.

The most popular of Kirchhoff's works is his spectrum analysis. It had in fact most extraordinary consequences of the most palpable kind, and has become of the highest importance for all branches of natural science. It has excited the admiration and stimulated the fancy of men as hardly any other discovery has done, because it has permitted an insight into worlds that seemed forever veiled for us. It is accordingly the most celebrated of Kirchhoff's discoveries.

But however wonderful the results, what seems to us more admirable still is the truly masterly work itself, the unusually keen and at the same time ingenious and diligent way in which Kirchhoff deduced from the outset, from an accidental observation, a general theoretical law and all the surprising inferences, and demonstrated them with full strictness and certainty. Great men had already held in their hand before him the threads of his discovery without being able to unravel them. The French as well as English brought forward and still produce claims of priority. Kirchhoff repelled them quietly but firmly. All had seen something, made guesses, considered as possible or probable (without Kirchhoff having been aware of it at the time, however). A solid basis, a rigorous demonstration had been given by nobody; it was reserved to the acuteness, thoroughness, and perseverance of a German searcher to elevate the lucky guess to the rank of a sure knowledge.

Spectrum analysis in the narrowest sense, *i. e.*, the "analysis of the chemical elements by means of spectral observations," is due, if we wish to make a distinction, to an idea and a suggestion of Bunsen's. Among the most ingenious performances of Bunsen may be reckoned certain very simple physical methods of qualitative chemical analysis, *i. e.*, the detection and the discrimination of chemical elements. A characteristic re-action of this kind he found to be the coloring of non-luminous flames. Each chemical element vaporized or burned in a non-luminous flame, for instance a blue-burning gas-flame, imparts to it a definite characteristic coloring. We should be able accordingly to recognize each substance by the light its incandescent vapor emits if our eyes had the power to distinguish as many differences of color as there are substances in nature. Kirchhoff and Bunsen helped the eyes however by decomposing the light of flames into its separate colors by means of a prism. This gives rise to the spectrum of the flame. The rainbow is a natural spectrum of the solar light made by the rain-drops. But this spectrum, as well as the spectra of all glowing solid or liquid bodies, offers quite another aspect from the spectra of flames, *i. e.*, incandescent gases. The first consist of known colors varying in a continuous way from one to another; the second consist of different bright lines separated by dark spaces, which bright lines have not only characteristic colorings, but are placed in particular positions and at definite intervals. Just as we recognize the constellation by their configurations and different brightnesses of their stars so do we distinguish the spectrum of iron from the spectrum of copper by the respective distances and coloring of their lines. We could even do without colors; it would be sufficient to measure by means of a scale the intervals between different lines in order to recognize by means of Kirchhoff and Bunsen's tables the element we have before us. It may seem amazing—but it is true—that a color-blind man could know with absolute certainty what colors a flame emits! The greatest advantage of a method in natural science, its independence of all subjective judgment, was bestowed on spectrum analysis by its discoverers. The main part of Kirchhoff and Bunsen's work and their chief merit is however the demonstration of the validity of the method, viz, that the configuration of lines depends only on the chemical nature of the luminous incandescent vapor, not on its temperature or other elements with which it is combined, and not on the nature of the flame in which it glows or other accessory circumstances. Of this a carefully worked out experimental proof was given, and Bunsen was accordingly able long ago to make the perfectly safe assertion that he discovered by means of his spectrum analysis a new element, because the salt from a certain mineral spring showed unknown lines. Nowadays spectrum analysis is the most sensitive chemical method of decomposition. And nevertheless, what is still more astonishing is the further discovery made by Kirchhoff, by means of this method discovered jointly with Bunsen. Kirchhoff happened to let a solar ray pass through a flame colored with sodium and then through a prism, so that the spectrum of the sun and of the flame fell one upon another. It was to be expected that the well known yellow line of sodium would come out in the solar spectrum; but it was just the opposite that took place. On the spot where the bright line ought to have shown itself there appeared a dark line. To Kirchhoff this reversal of the sodium line appeared at once in the highest degree remarkable, and he suspected immediately that some fundamental law was lurking there. The fact had been noticed by others (as was proved afterwards), and that by men of the highest renown. It was reserved however to Kirchhoff's genius to detect and to pick up the treasure of new truths that lay hidden there. Already on the day following the experiment he was able to deduce and to explain the phenomenon from a more general principle which, strange to say, belonged not to optics but to the theory of heat. From a proposition, very remote in appearance, that heat passes only from a body of a higher temperature to one of a lower and not inversely, he deduced by dint of purely logical inferences the fact of the reversal of the sodium line. The middle term in the syllogism was given by the celebrated Kirchhoff's law on the emission and absorption of light and heat by bodies, which says that all bodies absorb chiefly those rays, those colors they emit themselves, and that the ratio of the absorbed and the emitted amount of light is one and the same in all bodies however different. The paper where this law is proved is the most beautiful Kirchhoff ever composed, although there is the smallest amount of mathematics in it. The history of this law might serve as a model for the work of a student of nature; the law is vigorously deduced from well-known general propositions; but says itself something new; it gives the different particular inferences which are to be verified by experiment. It will be the lot of a few only to make such discoveries, but all ought to consider as a model to imitate, the diligence, the conclusiveness, and the care—and not less the great modesty—with which Kirchhoff communicates his discovery to the world: "On the occasion of a research made jointly with Bunsen on the spectra of colored flames, by means of which it became possible to us to recognize the qualitative composition of complex aggregates by inspection of their blow-pipe flames, I made some observations that give an unexpected disclosure as to origin of Fraunhofer's lines, and justify the inference to be drawn from them as to the material constitution of the solar atmosphere, and perhaps of those of the brightest stars." These words show that Kirchhoff himself made the wonderful application of his law. The Fraunhofer's lines to which he alludes are, as is well known, fine dark bands that furrow the solar spectrum, such as it is, even without the help of a flame. The nature of these lines was at first very enigmatic. The just described experiment of Kirchhoff shows however that artificial Fraunhofer's lines may be produced by means of a flame. The inference was near that the natural lines are produced by the same cause as the artificial ones, that they are reversed gas spectra, and that the light of the glowing solar body has already traversed somewhere incandescent gases, before it reached the earth. We may go further, however. When the artificial lines *coincide* with the Fraunhofer's lines, as (for instance), Kirchhoff proved to be the case for iron, sodium, or nickel, one may conclude—taking one's stand on the joint research of Kirchhoff and Bunsen—that these chemical elements are found in those hypothetical incandescent gases. The fact that the sun consists of a glowing liquid nucleus, surrounded by an envelope of luminous vapors, and above all that these vapors contain the terrestrial substances whose line-spectra coincide with Fraunhofer's lines, this fact was inferred, "with as much certainty," says Kirchhoff," "as can be attained in natural science."

It is a characteristic trait of Kirchhoff that he calculated numerically this certainty. It would be possible for the bright lines of iron, for instance, to coincide by mere chance with Fraunhofer's lines; but the probability for such an event was found to be equal to $\frac{1}{1,000,000,000,000}$ (one-billionth), an almost evanescent quantity. "There must be a cause that occasions these coincidences," says Kirchhoff. "An adequate cause can be produced; the observed fact may be explained if it be admitted that the rays of light that make the solar spectrum have traversed vapors of iron and suffered an absorption such as vapors of iron generally produce. It is at the same time the only cause that can be adduced; its adoption seems accordingly necessary."

We may insert here a story that Kirchhoff liked to relate himself. The question whether Fraunhofer's lines reveal the presence of gold in the sun was being investigated. Kirchhoff's banker remarked on this occasion: "What do I care for gold in the sun if I can not fetch it down here?" Shortly afterwards Kirchhoff received from England a medal for his discovery, and its value in gold. While handing it over to his banker, he observed: "Look here, I have succeeded at last in fetching some gold from the sun." As to Kirchhoff's own opinion of the importance of this law, it was quite indifferent to him, as stated above, whether the law admitted of any application to the investigation of the nature of the sun and fixed stars, or had only a theoretical interest. As a characteristic trait of him may be mentioned that in his theoretical lectures he never says a single word about the region to which access was gained through his discovery, and in his collected papers he grants it a place only near the end.

The other papers of Kirchhoff treat various subjects of mathematical physics. Those concerned with electricity are the most numerous. A whole series of them is devoted to the calculation of the paths the electrical current takes in bodies of different shape or in a net-work of conduction. There is a Kirchhoff's law about it too, which is of fundamental importance for the investigation of the distribution of the flow of electricity in complicated conditions of conduction. Another series of papers treats of the distribution of static electricity and magnetism. These were in part celebrated problems on which the greatest of his predecessors (like Poisson), had already tried their forces and had not succeeded in mastering them so completely as Kirchhoff. He was the first to apply the so-called mechanical theory of heat to chemical processes, and by this application he bridged the way to the connection of different branches of natural science by means of mechanical principles. The basis of the mechanical theory of heat, the law of the permanency of work, as Kirchhoff styled it, is according to him undoubtedly the most important accession of knowledge gained in our century in the region of natural science.*

The brilliant, various, and apparently complicated phenomena of light, Kirchhoff deduced in his lectures on optics from the purely mechanical theory of an elastic body. That æther is such a body is a hypothesis which, though enunciated by Kirchhoff's predecessors, was worked out by him in a particularly vigorous way. Nevertheless, all phenomena can not be explained by such a supposition. That Kirchhoff developed this hypothesis and this only, and contented himself with mentioning at the end of his course what circumstances spoke against his hypothesis and, in this way, demolished before the eyes of the students the whole structure, is to be accounted for by his idea of the scope and the limits of the investigation and interpretation of nature.

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* His discourse as rector of Heidelberg University 1865.

On such occasions, I must confess, I asked myself many a time, "What for? Why develop a theory that leads to contradiction with experiment? Is the probing of nature, for Kirchhoff, only the greatest and the most interesting exercise in calculation?"

In answer to such doubts I shall adduce his own words in his discourse delivered in 1865, as rector of Heidelberg University "on the scope of the natural sciences." He says there: "There is a science called mechanics, whose object is to determine the motion of bodies when the causes that occasion them are known. - - - Mechanics is a twin sister of geometry; both sciences are applications of pure mathematics; the propositions of both, as to their certainty, stand on the same level; we have just as much right to ascribe absolute certainty to mechanical theorems as to geometrical." And further: "If we were acquainted with all the forces of nature and knew what is the state of matter at a certain moment of time, we should be able to deduce by means of mechanics its state at every subsequent moment, and to deduce how the various natural phenomena follow and accompany each other. The highest goal the natural sciences must strive to attain is the realization of the just mentioned suppostion, - - - viz, the reduction of all natural phenomena to mechanics. We shall never attain the goal of the natural sciences, but even the fact that it is recognized as such offers a certain satisfaction, and in approximating to it lies the highest pleasure to be derived from the study of natural phenomena."

I must mention besides the famous words with which Kirchhoff commences his Mechanics, published in 1875: "Mechanics is the science of motion; its object may be stated to be to describe in the most *complete and simple way* the motion that takes place in nature." The difference between the first and the last definition of mechanics is worthy of notice. At the former time, and before the large public, Kirchhoff spoke of causes of motion. Now, and in a strictly mathematical book, the word and the notion of cause do not appear. The interpretation of nature is given up; the only thing looked for is the simplest possible description of nature. These introductory words of his Mechanics, and their working out in the book itself are the most consequent, far-reaching expression of Kirchhoff's way of looking at nature. He makes no hypothesis as to the possibility of arriving at a knowledge of things in themselves. He wants only to portray the phenomena in a logically certain form. In relation to the sensible world (according to Kant) we have logical (that is to say, *a priori*) certainty only of the propositions of geometry and mechanics, the last distinguished from the first on account of their requiring, besides the three dimensions of space, the fourth one, time, and the notion of a mobile matter. With these three fundamental notions of space, time, and matter, Kirchhoff tries to make his way to the description of the facts of experience and goes beyond his predecessors by delineating by means of pure geometry the supposed logically fundamental notions of force and mass. Force is to him the acceleration (change of velocity) experienced by a material particle in a unit of time; the knowledge of all these accelerating forces in a given moment of time would suffice to describe the world; experience has shown however that the description gains in simplicity, if we multiply the acceleration by a certain positive constant, called *mass* of the moving particle. I have mentioned this abstract train of thought because it is very characteristic of Kirchhoff. The necessity of looking at natural forces as something *really existing*, or the mass as something *really* constant, remaining equal to itself, he does not recognize. It is only a fact of experience that the movements in nature hitherto observed have taken place in such a way that they seem to be represented in the simplest manner by making those suppositions. We can build up mechanical systems on quite different bases, but it would not help us to describe simply the real movements. The problem of mathematical physics will be solved when the observed phenomena will be described by means of the simplest possible supposition as to the nature of forces and distribution of matter. There is nothing impossible in it; it can be proved in fact that all that men can observe in finite time must be susceptible of being described mathematically.

Even an outsider will not fail to notice, I think, that *something* is not included in Kirchhoff's programme. The simplest description can not produce the conviction that the phenomena, even in future time, shall run in accordance with the description; its equations are, so to say, not *laws*. There exists a stand-point differing somewhat from that of Kirchhoff; it looks for what is in accordance with a law in the change of phenomena. Experience teaches us that nature acts according to laws; because without laws experience would be impossible. Experience is the collecting of what is similar in different particular perceptions. That the laws exist is accordingly an observed fact and not a hypothesis. We feel them acting at every moment independently of our will. We must ascribe to them the same reality as to our will; these two things are opposite to one another, power against power. We designate them accordingly by the names of forces, and forces as *causes* of motion; they have the same reality as the motion itself. Up to this point nature may be said to be intelligible. What a force is we know not; we can only say that it manifests itself in the acceleration it imparts to the mass, and *de facto* accordingly, we do not go beyond Kirchhoff's description of nature. As to results, the search after a law and the endeavor after the simplest description of nature is one and the same thing; the difference lies only in the formulation of the problem and sometimes possibly in the way towards its solution. It follows for instance from Kirchhoff's definition, that it must be permitted (not only upon pedagogical but even upon philosophical grounds) to use hypotheses, even when they are recognized not to be sufficient in all cases, provided they are still the simplest. In fine, only that will appear to us simple which is logically true.

From what precedes one sees how near sometimes mathematical physics approaches to metaphysics. Kirchhoff gave to empiricism in the theory of cognition, its most precise and most consequent expression, and placed himself accordingly at the acme of the whole of modern mathematical physics.

Kirchhoff's endeavor after clearness and truth appears also in his philosophical stand-point, and makes him prefer to give the definition of his own *problem* in the study of nature from a narrow view, rather than to suffer in it even a semblance of a proposition accepted on faith, as nature's conformity to law possibly is. And still he analyzed nature not merely as a critical thinker. His greatest discovery shows that he possessed also the alert introspection, the sympathetic investigation, and the intuitive insight into the working of natural forces, without which no true student of nature can make investigations. We repeat, Kirchhoff was one of the greatest students of nature, because he was a mathematical physicist in the sense explained above.

ON HEREDITY.*

By Sir WILLIAM TURNER.

The subject of heredity (if I may say so) is in the air at the present time. The prominence which it has assumed of late years is in connection with its bearing on the Darwinian theory of natural selection, and consequently biologists generally have had their attention directed to it. But in its relations to man, his structure, functions, and diseases, it has long occupied a prominent position in the minds of anatomists, physiologists, and physicians. That certain diseases, for example, are hereditary was recognized by Hippocrates, who stated generally that hereditary diseases are difficult to remove, and the influence which the hereditary transmission of disease exercises upon the duration of life is the subject of a chapter in numerous works on practical medicine, and forms an important element in the valuation of lives for life insurance.

The first aspect of the question which has to be determined is whether any physical basis can be found for heredity. The careful study, especially during the last few years, of the development of a number of species of animals, mostly but not exclusively amongst the invertebrata, by various observers, has established the important fact that the young animal arises by the fusion within the egg or germ-cell of an extremely minute particle derived from the male parent, with an almost equally minute particle, derived from the germ-cell produced by the female parent. These particles are technically termed in the former case the *male pronucleus*, in the latter the *female pronucleus*, and the body formed by their fusion is called the *segmentation nucleus*. These nuclei are so small that it seems almost a contradiction in terms to speak of their magnitude,—rather one might say their minitude; for it requires the higher powers of the best microscopes to see them and follow out the process of conjugation. But notwithstanding their extreme minuteness, the pronuclei and the segmentation nucleus are complex, both in chemical and molecular structure. From the segmentation nucleus produced by the fusion of the pronuclei with each other, and from corre-

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*Presidential address before the Anthropological Section of the British Association, A. S., at Newcastle, September, 1889. (*Report of the British Association*, vol. LIX, pp. 756-771.)

sponding changes which occur in the protoplasm of the egg which surrounds it, other cells arise by a process of division, and these in their turn also multiply by division. These cells arrange themselves in course of time into layers, which are termed the germinal or embryonic layers. From these layers arise all the tissues and organs of the body, both in its embryonic and adult stages of life.

The starting-point of each individual organism—*i. e.*, of each new generation—is therefore the segmentation nucleus. Every cell in the adult body is derived by descent from that nucleus through repeated division. As the segmentation nucleus is formed by the fusion of material derived from both parents, a physical continuity is established between parents and offspring. But this physical continuity carries with it certain properties which cause the offspring to reproduce, not only the bodily configuration of the parent, but other characters. In the case of man we find along with the family likeness in form and features, a correspondence in temperament and disposition, in the habits and mode of life, and sometimes in the tendency to particular diseases. This transmission of characters from parent to offspring is summarized in the well-known expression that "like begets like," and it rests upon a physical basis. The size of the particles which are derived from the parents (called the male and female pronuclei), the potentiality of which is so utterly out of proportion to their bulk, is almost inconceivably small when compared with the magnitude of the adult body. And yet these particles are sufficient to stamp the characters of the parents, of the grandparents, and of still more remote ancestors on the offspring, and to preserve them throughout life, notwithstanding the constant changes to which the cells forming the tissues and organs of the body are subjected in connection with their use and nutrition.

In considering the question of how new individuals are produced, one must keep in mind that it is not every cell in the body which can act as a center of reproduction for a new generation, but that certain cells, which we name germ-cells and sperm-cells, are set aside for that purpose. These cells, destined for the production of the next generation, form but a small proportion of the body of the animal in which they are situated. They are as a rule marked off from the rest of the cells or of its body at an early period of development. The exact stage at which they become specially differentiated for reproductive purposes varies however in different organisms. In some organisms (as is said by Balbiani to be the case in *Chironomus*) they apparently become isolated before the formation of the germinal layers is completed; but as a rule their appearance is later; and in the higher organisms, not until the development of the body is relatively much more advanced.

The germ-cells after their isolation take no part in the growth of the organism in which they arise; and their chief association with the other cells of its body is that certain of the latter are of service in their nutrition. The problem therefore for consideration is the mode in which these germ or reproductive cells become influenced, so that after having been isolated from the cells which make up the bulk of the body of the parent they can transmit to the offspring the characters of the parent organism. Various speculations and theories have been advanced by the way of explanation. The well-known theory of Pangenesis, which Charles Darwin with characteristic moderation put forward as merely a provisional hypothesis, assumes that *gemmules* are thrown off from each different cell or unit throughout the body which retain the characters of the cells from which they spring; that the gemmules aggregate themselves either to form or to become included within the reproductive cells; and that in this manner they and the characters which they convey are capable of being transmitted in a dormant state to successive generations, and to reproduce in them the likeness of their parents, grandparents, and still older ancestors.

In 1872, and four years afterwards, in 1876, Mr. Francis Galton published most suggestive papers on kinship and heredity.* In the latter of these papers he developed the idea that "the sum total of the germs, gemmules, or whatever they may be called," which are to be found in the newly fertilized ovum, constitute a *stirp*, or root. That the germs which make up the stirp consist of two groups; the one which develops into the bodily structure of the individual, and which constitutes therefore the personal structure; the other, which remains latent in the individual, and forms, as it were, an undeveloped residuum That it is from these latent or residual germs that the sexual elements intended for producing the next generation are derived, and that these germs exercise a predominance in matters of heredity. Further, that the cells which make up the personal structure of the body of the individual, exercise only in a very faint degree any influence on the reproductive cells, so that any modifications acquired by the individuals are barely, if at all, inherited by the offspring.

Subsequent to the publication of Mr. Galton's essays, valuable contributions to the subject of heredity have been made by Professors Brooks, Naegeli, Nussbaum, Weismann, and others. Professor Weismann's theory of heredity embodies the same fundamental idea as that propounded by Mr. Galton; but as he has employed in its elucidation a phraseology which is more in harmony with that generally used by biologists, it has had more immediate attention given to it. As Weismann's essays have during the present year been translated for, and published by the Clarendon Press,† under the editorial superintendence of Messrs. Poulton, Schönland, and Shipley, they are now readily accessible to all English readers.

Weismann asks the fundamental question, "How is it that a single cell of the body can contain within itself all the hereditary tendencies of the whole organism?" He at once discards the theory of pangenesis, and states that in his belief the germ-cell, so far as its essential and characteristic substance is concerned, is not derived at all from the body of the individual in which it is produced, but directly from the parent germ-cell from which the individual has also arisen. He calls his theory the continuity of the germ-plasm, and he bases it upon the supposition that in each individual a portion of the specific germ-plasm derived from the germ-cell of the parent is not used up in the construction of the body of that individual, but is reserved unchanged for the formation of the germ-cells of the succeeding generation. Thus like Mr. Galton, he recognizes that in the stirp or germ there are two classes of cells destined for entirely distinct purposes: the one for the development of the *soma* or body of the individual, which class he calls the *somatic* cells; the other for the perpetuation of the species, *i. e.*, for reproduction. In further exposition of his theory, Weismann goes on to say, as the process of fertilization is attended by a conjugation of the nuclei of the reproductive cells (the pronuclei referred to in an earlier part of this address), that the nuclear substance must be the sole bearer of hereditary tendencies. Each of the two uniting nuclei would contain the germ-plasm of one parent, and this germ-plasm also would contain that of the grandparents as well as that of all previous generations. - - -

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* *Proceedings Roy. Soc. Lond.*, 1872, and *Journ. Anthrop. Inst.*, 1876, vol v.

† Oxford, 1889.

It follows therefore from this theory that the germ-plasm possesses throughout, the same complex, chemical, and molecular structure, and that it would passs through the same stages when the conditions of development are the same, so that the same final product would arise. Each successive generation would have therefore an identical starting-point, so that an identical product would arise from all of them. Weismann does not absolutely assert that an organism can not exercise a modifying influence upon the germ-cells within it; yet he limits this influence to such slight effect as that which would arise from the nutrition and growth of the individual, and the reaction of the germ-cell upon changes of nutrition caused by alteration in growth at the periphery, leading to some change in the size, number, and arrangements of its molecular units. But he throws great doubt upon the existence of such a re-action, and he, more emphatically than Mr. Galton, argues against the idea that the cells which make up the somatic or personal structure of the individual exercise any influence on the reproductive cells. From his point of view the structural or other properties which characterize a family, a race, or a species, are derived solely from the reproductive cells through continuity of their germ-plasm, and are not liable to modification by the action on them of the organs or tissues of the body of the individual organism in which they are situated.

The central idea of heredity is permanency; that like begets like, or as Mr. Galton more fitly puts it, that "like *tends* to produce like." But though the offspring conform with their parents in all their main characteristics, yet, as every one knows, the child is not absolutely like its parents, but possesses its own character, its own individuality. It is easy for any one to recognize that differences exist amongst men when he compares one individual with another; but it is equally easy for those who make a special study of animals to recognize individual differences in them also. Thus a pigeon or canary fancier distinguishes without fail the various birds in his flock and a shepherd knows every sheep under his charge. But the anatomist tells us that these differences are more than superficial,—that they also pervade the internal structure of the body. Intimately associated therefore with the conception of heredity—that is, the transmission of characters common to both parent and offspring—is that of variability,—that is, the appearance in an organism of certain characters which are unlike those possessed by its parents. Heredity therefore may be defined as the perpetuation of the like; variability, as the production of the unlike.

And now we may ask, Is it possible to offer any feasible explanation of the mode in which variations in organic structure take their rise in the course of development of an individual organism? Anything that one may say on this head is of course a matter of speculation, but certain facts may be adduced as offering a basis for the construction of an hypothesis, and on this matter Professor Weismann makes a number of ingenious suggestions.

Prior to the conjugation of the male and female pronuclei to form the segmentation nucleus, a portion of the germ-plasm is extruded from the egg to form what are called *polar bodies*. Various theories have been advanced to account for the significance of this curious phenomenon. Weisman explains it on the hypothesis that a reduction of the number of ancestral germ-plasms in the nucleus of the egg is a necessary preparation for fertilization and for the development of the young animal. He supposes that by the expulsion of the polar bodies one-half of the number of ancestral germ-plasms is removed, and that the original bulk is restored by the addition of the male pronucleus to that which remains. As precisely corresponding molecules of this plasm need not be expelled from each ovum, similar ancestral plasms are not retained in each case; so that diversities would arise even in the same generation and between the offspring of the same parents.

Minute though the segmentation nucleus is, yet microscopic research has shown that it is not a homogeneous structureless body, but is built up of different parts. Most noteworthy is the presence of extremely delicate threads or fibrils, called the *chromatin filaments*, which are either coiled on each other, or intersect to form a network-like arrangement. In the meshes of this network a viscous—and, so far as we yet know, structureless—substance is situated. Before the process of division begins in the segmentation nucleus these filaments swell up and then proceed to arrange themselves at first into one and then into two star-like figures before the actual division of the nucleus takes place.* It is obvious therefore that the molecules which enter into the formation of the segmentation nucleus can move within its substance, and can undergo a re-adjustment in size and form and position. But this re-adjustment of material is without doubt not limited to those relatively coarse particles which can be seen and examined under the microscope, but applies to the entire molecular structure of the segmentation nucleus. Now it must be remembered that the cells of the embryo from which all the tissues and organs of the adult body are derived are themselves descendants of the segmentation nucleus, and they will doubtless inherit from it both the power of transmitting definite characters and a certain capacity for re-adjustment both of their constituent materials and the relative positions which they may assume towards each other. One might conceive therefore that if in a succession of organisms derived from common ancestors the molecular particles were to be of the same composition and to arrange themselves in the segmentation nucleus and in the cells derived from it on the same lines, these successive generations would be alike; but if the lines of adjustment and the molecular constitution were to vary in the different generations, then the products would not be quite the same. Variations in structure and to some extent also in the construction of parts, would arise, and the unlike would be produced.

In this connection it is also to be kept in mind that in the higher organisms, and indeed in multicellular organisms generally, an individual is derived, not from one parent only, but from two parents. If one parent were to contribute a larger proportion than the other to the formation of a particular organism, then the balance would be disturbed, the offspring in its character would incline more to one parent than to the other, according to the proportion contributed by each, and a greater scope for the production of variations would be provided. These differences would be increased in number in the course of generations, owing to new combinations of individual characters arising in each generation. As long as the variations which are produced in an organism are collectively within a certain limitation, they are merely individual variations, and express the range within which such an organism, though exhibiting differences from its neighbors, may yet be classed along with them in the same species. It is in this sense that I have discussed the term variability up to the present stage of this address. Thus all those varieties of mankind which on account of differences in the color of the skin, we speak of as the white, black, yellow races and red-skins are men, and they all belong to that species which the zoologists term *Homo sapiens*.

But the subject of variability cannot, in the present state of science, be confined in its discussion to the production of individual variations within the limitations of a common species. Since Charles Darwin ennunciated the proposition that favorable variations would tend to be preserved, and unfavorable ones to be destroyed, and that the result of this double action, by the accumulation of minute existing differences, would be the formation of new species by a process of natural selection, this subject has attained a much wider scope, has acquired increased importance, and has formed the basis of many ingenious speculations and hypotheses. As variations when once they have arisen, may be hereditarily transmitted, the Darwinian theory might be defined as heredity modified and influenced by variability.

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* The observations, more especially of Flemming, E. Van Bendenen, Strasburger, and Carnoy, may be referred to in connection with the changes which take place in nuclei prior to, and in connection with, their division.

It may be admitted that many variations which may arise in the development of an individual, and which are of service to that individual, would tend to be preserved and perpetuated in its offspring by hereditary transmission. But it is also without question that variations which are of no service, and indeed are detrimental to the individual in which they occur, are also capable of being hereditarily transmitted. This statement is amply borne out in the study of those important defects in bodily structure which pathologists group together under the name of congenital malformations. The commonest form of malformation is where an increase in the number of digits on the hands or feet, or on both, occurs in certain families, numerous instances of which have now been put on record. But in other families there is an hereditary tendency to a diminution in the number of digits, or to a defect in the development of those existing. Another noticeable deformity which is known to be hereditary in some families is that of imperfect development of the upper lip and roof of the mouth, technically known as hare-lip and cleft palate.

These examples illustrate what may be called the coarser kinds of hereditary deformity, where the redundancies or defects in parts of the body are so gross as at once to attract attention. But modifications or variations in structure that can be transmitted from parent to offspring are by no means limited to changes which can be detected by the naked eye. They are sometimes so minute as to be determined rather by the modifications which they occasion in the function of the organ than by the ready recognition of structural variations. [Cases of color-blindness, and of deaf-mutism were then referred to.] - - - Dr. Horner has related a most interesting family history in which color-blindness was traced through seven generations.* - - - Mr. David Buxton, who has paid great attention to the subject of hereditary deafness,† states that the probability of congenital deafness in the offspring is nearly seven times greater when both parents are deaf than when only one is so. In the latter case the chance of a child being born deaf is less than ¾ per cent., in the former the chances are that 5 per cent. of the children will be deaf-mutes.

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* Cited in *Die Allgemeine Pathologie*, by Dr. Edwin Klebs, Jena, 1887.

† *Liverpool Medico-Chirurg. Journ.*, July, 1857; January 1859.

Although a sufficient number of cases has now been put on record to prove that in some families one or other kind of congenital deformity may be hereditarily transmitted, yet I do not wish it to be supposed that congenital malformations may not arise in individuals in whom no hereditary tendency can be traced.

The variations I have spoken of as congenital malformations arise, as a rule, before the time of birth, during the early development of the individual; but there is an important class of cases in which the evidences for hereditary transmission is more or less strong, which may not exhibit their peculiarities until months, or even years, after the birth of the individual This class is spoken of as hereditary diseases, and the structural and functional changes which they produce exercise most momentous influences. Sometimes these diseases may occasion changes in the tissues and organs of the body of considerable magnitude, but at other times the alteration is much more subtle, is molecular in its character, requires the microscope for its determination, or is even incapable of being recognized by that instrument.

Had one been discussing the subject of hereditary disease twenty years ago, the first example probably that would have been adduced would have been tuberculosis, but the additions to our knowledge of late years throw some doubt upon its hereditary character. There can, of course, be no question that tubercular disease propagates itself in numerous families from generation to generation, and that such families show a special susceptibility or tendency to this disease in one or other of its forms. But whilst fully admitting the pre-disposition to it which exists in certain families, there is reason to think that the structural disease itself is not hereditarily transmitted, but that it is directly excited in each individual in whom it appears by a process of external infection due to the action of the tubercle bacillus. Still, if the disease itself be not inherited, a particular temperament which renders the constitution liable to be attacked by it, is capable of hereditary transmission.

Sir James Paget,* when writing on the subject of cancer, gives statistics to show that about a quarter of the persons affected were aware of the existence of the same disease in other members of their family, and he cites particular instances in which cancer was present in two and even four generations. He had no doubt that the disease can be inherited—not, he says, that strictly speaking, cancer, or cancerous material is transmitted, but a tendency to the production of those conditions which will finally manifest themselves in a cancerous growth. The germ from the cancerous parent must be so far different from the normal as after a lapse of years to engender the cancerous condition.

Heredity is also one of the most powerful factors in the production of those affections which we call gout and rheumatism. Sir Dyce Duckworth, the latest systematic writer on gout, states that in those families whose histories are the most complete and trustworthy the influence is

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Lectures on Surgical Pathology, 3d ed., London, 1870.

strongly shown, and occurs in from 50 to 75 per cent. of the cases; further, that the children of gouty parents show signs of articular gout at an age when they have not assumed those habits of life and peculiarities of diet which are regarded as the exciting causes of the disease.

In connection with the tendency to the transmissibility of either congenial malformations or diseases, consanguinity in the parents, although by no means a constant occurrence, is a factor which in many cases must be taken into consideration.* If we could conceive both parents to be physiologically perfect, then it may be presumed that the offspring would be so also; but if there be a departure in one parent from the plane of physiological perfection, then it may safely be assumed that either the immediate offspring or a succeeding generation will display a corresponding departure in a greater or less degree. Should both parents be physiologically imperfect, we may expect the imperfections, if they are of a like nature, to be intensified in the children. It is in this respect therefore that the risk of consanguineous marriages arises; for no family can lay claim to physiological perfection.

When we speak of tendencies, susceptibilities, proclivities, or predisposition to the transmission of characters, whether they be normal or pathological, we employ terms which undoubtedly have a certain vagueness. We are as yet quite unable to recognize, by observation alone, in the germ-plasm any structural change which would enable us to say that a particular tendency or susceptibility will be manifested in an organism derived from it. We can only determine this by following out the life-history of the individual. Still it is not the less true that these terms express a something, of the importance of which we are all conscious. So far as man is concerned, the evidence in favor of a tendency to the transmission of both structural and functional modifications which are either of disservice or positively injurious, or both, is quite as capable of proof as that for the transmission of characters which are likely to be of service. Hence useless as well as useful characters may be selected and transmitted heriditarily.

Much has been said and written during the last few years of the transmission from parents to offspring of characters which have been "acquired" by the parent, so that I cannot altogether omit some reference to this subject. It will conduce to one's clearness of perception of this much-discussed question, if one defines at the outset in what sense the term "acquired characters" is employed; and it is the more advisable that this should be done, as the expression has not always been used with the same signification. This term may be used in a wide or in a more restricted sense. In its wider meaning it may cover all the characters which make their first appearance in an individual, and which are not found in its parents, in whatever way they have arisen:—

(1) Whether their origin be due to such molecular changes in the

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* I may especially refer (for a discussion of this subject) to an admirable essay by Sir Arthur Mitchell, K. C. B., "On Blood-relationship in Marriage considered in its Influence upon the Offspring."

germ-plasm as may be called spontaneous, leading to such an alteration in its character as may produce a new variation; or,

(2) Whether their origin be accidental, or due to habits, or to the nature of the surroundings, such as climate, food, etc.

Professor Weismann has pointed out with great force the necessity of distinguishing between these two kinds of "acquired characters," and he has suggested two terms, the employment of which may keep before us how important it is that these different modes of origin should be recognized. Characters which are produced in the germ-plasm itself by natural selection, and all other character which result from this latter cause, he names *blastogenic*. He further maintains that all blastogenic characters can be transmitted; and in this conclusion, doubtless most persons will agree with him. On the other hand, he uses the term *somatogenic* to express those characters which first appear in the body itself, and which follow from the re-action of the *soma* under direct external influences. He includes under this head the effects of mutilation, the changes which follow from increased or diminished performance of function, those directly due to nutrition, and any of the other direct external influences which act upon the body. He further maintains that the somatogenic characters are not capable of transmission from parent to offspring, and he suggests that in future discussions on this subject the term "acquired characters" should be restricted to those which are somatogenic.

That the transmission of character so required can take place is the foundation of the theory of Lamark, who imagined that the gradual transformation of species was due to a change in the structure of a part of an organism under the influence of new conditions of life, and that such modifications could be transmitted to the offspring. It was also regarded as of importance by Charles Darwin, who stated,* that all the changes of corporeal structure and mental power cannot be exclusively attributed to the natural selection of such variations as are often called spontaneous, but that great value must be given to the inherited effects of use and dis-use, some also to the modification in the direct and prolonged action of changed conditions of life, also to occasional reversions of structure. Herbert Spencer believes,† that the natural selection of favorable varieties is not in itself sufficient to account for the whole of organic evolution. He attaches a greater importance than Darwin did to the share of use and dis-use in the transmission of variations. He believes that the inheritance of functionally produced modifications of structure takes place universally, and that as the modification of structure is a *vera causa* as regards the individual, it is unreasonable to suppose that it leaves no traces in prosterity.

On the other hand, there are very eminent authorities who contend that the somatogenic acquired characters are not transmissible from parent to offspring. Mr. Francis Galton (for example) gives a very qualified assent to the possibility of transmission. Professor His, of Leipsig, doubts its validity. Professor Weismann says that there is no proof of it. Mr. Alfred Russel Wallace in his most recent work* considers that the direct actions of the environment (even if we admit that its effects on the individual are transmitted by inheritence) are so small in comparison with the amount of spontaneous variation of every part of the organism, that they must be quite overshadowed by the latter. Whatever causes (he says) have been at work, natural selection is supreme to an extent which even Darwin himself hesitated to claim for it. There is thus a conflict of opinion amongst the authorities who have given probably the most thought to the consideration of this question.

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* Preface to 2d edition of *Descent of Man*, 1885; also *Origin of Species*, 1st ed.

† "Factors of Organic Evolution," *Nineteenth Century*, 1886.

In the first place I would however express my agreement with much that has been said by Professor Weismann on the want of sufficient evidence to justify the statement that a mutilation which has affected a parent can be transmitted to the offspring. It is I suppose within the range of knowledge of most of us that children born of parents who have lost an eye, an arm, or a leg, come into the world with the full complement of eyes and limbs. The mutilation of the parent has not affected the offspring; and one would indeed scarcely expect to find that such gross visible losses of parts as take place when a limb is removed by an accident or surgical operation, should be repeated in the offspring. But a similar remark is also applicable to such minor mutilations as scars, of the transmission of which to the offspring, though it has been stoutly contended for by some, yet seems not to be supported by sufficiently definite instances.

I should search for illustrations of the transmission of somatogenic characters in the more subtle processes which affect living organisms, rather than those which are produced by violence and accident. I shall take as my example certain facts which are well known to those engaged in the breeding of farm-stock or of other animals that are of utility to, or are specially cultivated by, man. I do not refer to the influence on the offspring of impressions made on the senses and nervous system of the mother, the first statement of the effects of which we find in the book of Genesis, where Jacob set peeled rods before the flocks in order to influence the color and markings of their young; though I may state that I have heard agriculturists relate instances from their own experience which they regarded as bearing out the view that impressions acting through the mother do influence her offspring. But I refer to what is an axiom with those who breed any particular kind of stock, that to keep the strain pure, there must be no admixture with stock of another blood. For example, if a short-horned cow has a calf by a highland sire, that calf, of course, exhibits characters which are those of both its parents. But future calves which the same cow may have when their sires have been of the short-horned blood, may, in addition to short-horn characters, have others which are not short-horned but highland.

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* *Darwinism.* London, 1889. P. 443.

The most noteworthy instance of this transmission of characters acquired from one sire through the same mother to her offspring by other sires, is that given in the often-quoted experiment by a former Lord Morton.* An Arabian mare in his possession produced a hybrid, the sire of which was a quagga, and the young one was marked by zebra-like stripes. But the same Arabian had subsequently two foals, the sire of which was an Arab horse, and these also showed some zebra-like markings. How then did these markings characteristic of a very different animal arise in these foals, both parents of which were Arabians? I can imagine it being said that this was a case of reversion to a very remote striped ancestor, common alike to the horse and the quagga. But to my mind no such far-fetched and hypothetical explanation is necessary. The cause of the appearance of the stripes seems to me to be much nearer and more obvious. I believe that the mother had acquired during her prolonged gestation with the hybrid, the power of transmitting quagga-like characters from it, owing to the interchange of material which had taken place between them in connection with the nutrition of the young one. For it must be kept in mind that in placental mammals an interchange of material takes place in opposite directions, from the young to the mother as well as from the mother to the young.† In this way, the germ-plasm of the mother, belonging to ova which had not yet matured, had become modified whilst still lodged in the ovary. This acquired modification had influenced her future offspring, derived from that germ-plasm, so that they in their turn, though in a more diluted form, exhibited zebra-like markings. If this explanation be correct, then we have an illustration of the germ-plasm having been directly influenced by the soma, and of somatogenic acquired characters having been transmitted.

Those who uphold the view that characters acquired by the soma can not be transmitted from parents to offspring undoubtedly draw so large a check on the bank of hypothesis that one finds it difficult, if not impossible, to honor it. Let us consider for a moment all that is involved in the acceptance of this theory, and apply it in the first instance to man. On the supposition that all mankind have been derived from common ancestors through the continuity of the germ-plasm, and that this plasm has undergone no modification from the *persona* or *soma* of the succession of individuals through whom it has been transmitted, it would follow that the primordial human germ-plasm must have contained within itself an extraordinary potentiality of development; a potentiality so varied, that all the multiform variations in physical structure, tendency to disease, temperament, and other characters and dispositions which have been exhibited by all the races and varieties of men who either now inhabit, or at any period in the world's history have inhabited, the earth must have been included in it. - - -

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* *Philosophical Transactions*, 1881; also Darwin's *Animals and Plants under Domestication*, 1st ed., 1868, vol. I, p. 403.

† See for facts and experiments *Essays* by Professors Harvey and Gusserow and Mr. Savory; also my *Lectures on the Comparative Anatomy of the Placenta*, Edinburgh, 1876.

Let us now glance at the other side of the question. All biologists will, I suppose, accept the proposition that the individual soma is influenced or modified by its environments or surroundings. Now, if on the basis of this proposition, the theory be grafted that modifications or variations thus produced are capable of so affecting the germ-plasm of the individual in whom the variation arises as to be transmitted to its offspring (and I have already given cases in point), then such variations might be perpetuated. If the modification is of service, then presumably it will add to the vitability of the individual, and through the inter-action between the soma and the germ-plasm, in connection with their respective nutritive changes, will so affect the latter as to lead to its being transmitted to the offspring. From this point of view the environment would, as it were, determine and regulate the nature of those variations which are to become hereditary, and the possibility of variations arising which are likely to prove useful becomes greater than on the theory that the soma exercises no influence on the germ-plasm. Hence I am unable to accept the proposition that somatogenic characters are not transmitted, and I can not but think that they form an important factor in the production of hereditary characters.

The morphological aspect of organic structure is undoubtedly of fundamental importance. But it should not be forgotton that tissues and organs—in addition to their subjection to the principles of development and descent—have to discharge certain specific purposes and functions, and that structural modifications arise in them in correlation with the uses to which they are put, so as to adopt them to perform modified duties. It may be difficult to assign the exact force which physiological adaptation can exercise in the perpetuation of variations. If the habit or external condition which has produced a variation continues to be practiced, then in all probability the variation would be intensified in successive generations. But should the habit cease or the external condition be changed, then although the variation might continue to be for a time perpetuated by descent, it would probably become less strongly marked and perhaps ultimately disappear. By accepting the theory that somatogenic characters are transmitted we obtain a more ready explanation, how men belonging to a race living in one climate or part of the globe can adapt themselves to a climate of a different kind. On the theory of the non-transmissibility of these acquired characters, long periods of years would have to elapse before the process of adaptation could be effected. The weaker examples (on this theory) would have had to die out, and the racial variety would require to have been produced by the selection of variations arising slowly and requiring one knows not how many hundreds or thousands of years to produce a race which could adapt itself to its new environment.

It may perhaps be thought that in selecting the subject of Heredity for my address, and in treating it as I have to a large extent in its general biological aspects, I have infringed upon the province of Section D (that of Biology). But I am not prepared to admit that any such encroachment has been made. Man is a living organism with a physical structure which discharges a variety of functions, and both structure and functions correspond in many respects (though with characteristic differences) with those which are found in animals. The study of his physical frame cannot therefore be separated from that of other living beings; and the processes which take place in the one must also be investigated in the other.

The physical aspect of the question, although of vast importance and interest, yet by no means covers the whole ground of man's nature, for in him we recognize the presence of an element beyond and above his animal framework. Man is also endowed with a spiritual nature. He possesses a conscious responsibility which enables him to control his animal nature, to exercise a discriminating power over his actions, and which places him on a far higher and altogether different platform than that occupied by the beasts which perish. The kind of evolution which we are to hope and strive for in him is the perfecting of this spiritual nature, so that the standard of the whole human race may be elevated and brought into more harmonious relation with that which is holy and divine.

ANTHROPOLOGY IN THE LAST TWENTY YEARS.*

By Dr. Rudolph Virchow, *of the University of Berlin.*

Translated by Rev. C. A. Bleismer.

Nearly twenty years ago the foundation of our present union meeting was laid on Austrian soil. A few men attending an association of naturalists, at Innspruck in 1869, formed themselves into a separate section, which held its session in a small auditorium of the university.

Of that number my countryman, Koner, has since died, but the rest are still living, among them Karl Vogt, Professor Semper (first general secretary of the German Anthropological Society), Professor Seligman, of Vienna, and some others.

And as I see with us Count Enzenberg, the secretary of that section, there are here at least two representatives of that memorable day.

Every member of that little gathering was fully convinced that Germany and Austria ought to be united in anthropological matters and that only through united work could any success be expected in anthropological investigations. A call was published for the establishment of a General German Anthropological Society, which should unite all German workers, including the German Swiss and the Germans in Austria.

At a subsequent meeting held in Mayence, in May, 1870, for the purpose of drafting a constitution, a number of Austrians participated and the articles were purposely framed in such a manner as to include German Austrians. But circumstances are frequently more powerful than the intentions of men.

The current of opinion during the period following this meeting was contrary to our purpose, which represented ideas based upon an unprejudiced consideration of events. Previously, in 1869, there had been formed an Anthropological Society at Berlin, the first one in Germany, also a separate society at Vienna, but only the Berlin society became a branch of the General German Society. It seemed impossible for some time to find any direct point of contact with the society at Vienna, although there was no variance between them and us. Individual members—as I most gratefully admit, (among whom our president, Baron von Andrian, is one,) often expressed their regret at our lack of union.

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* Opening address delivered before the twentieth general meeting of the German Anthropological Association (of Germany and Austria) in Vienna, August 5, 1889. (From the *Correspondenz-Blatt der deutschen Gesellschaft für Anthropologie, Ethnologie und Urgeschichte*, xx. Jahrgang, No. 9, September, 1889, pp. 89-100.)

In 1881, the first attempt to bring about a union was made when the German and the Austrian anthropologists held their general meetings successively at Regensburg and Salzburg, both attending each other's session. Since that time the idea of union gained strength until it was realized in our present joint meeting; and may a sentiment of union be developed that shall complete the work which we began.

You all understand that this question of nationality is a very important one in an anthropological sense.

We must always start from what is known; our question is that of habitat. And here we differ from the zoologist, who is only to a limited degree concerned about this question. Not until we know whence a person came and where he lived is he a legitimate subject of anthropological investigation. This holds true also with respect to a human skull. An unknown skull may be momentarily of some interest, but from a scientific stand-point it is of no importance until its habitat has been determined.

Τὶς πόθεν εἶς ἀνδρῶν is a question which not merely concerns our every day life, but is an important one for the anthropologist. It is a very difficult matter to make collectors of skulls understand that not merely skulls, but skulls of persons or tribes are needed, that can be identified as regards their habitat. Then only are they of any anthropological value to the investigator.

A skull *per se* is of very little account to us, but when its nationality is known it begins to exist, so to speak. We must not forget moreover that our conceptions of nationality are largely based upon our present relationships, and that these become of less value the farther back we go, and that they are of no value at all when we reach the period in which clearly defined nationalities are not known.

Every evidence of nationality ceases in pre-historic times; it is then a mere abstraction. There nationality has to be made up and a nomenclature adopted which can be at best only a designation for a certain period, valueless in itself and unintelligible to future times. To be sure, to talk about a race of Cannstatt or of Cro-Magnon may sound very learned, but I hope that ere long such a phraseology will be discarded.

At present questions of nationality can be settled only with great difficulty. We may be sure of being tolerably successful, if we select some island of the Pacific Ocean. There nationality is fully developed and its people are tangible; every one of them is easily recognized as belonging to a distinct nationality, and our experience is similar to that of the geologist who can construct a whole species from a single, or at most only from a few skulls of animals, or who at any rate can determine from a single skull the craniology of the whole species. It would be very pleasant to be able to trace the history of a whole tribe whenever a skull is found, but unfortunately we are too often confronted with such complicated variations that we lose all data for making out the nationality. But in an island of the Pacific Ocean, which possesses much more scientific interest than political importance, we find an analogy to animal races, viz: races of men, developed in circumscribed surroundings, with definite characteristics, easily pointed out, who represented a distinct type. Much to our regret this can be done only very infrequently in the case of continental tribes or nations. To determine the question of nationality with regard to a European would take many days.

Permit me to emphasize right here that we as anthropologists have little right to thrust into the foreground the idea of nationality, in a narrow sense of the word. We know that every nationality, take for instance, the German or the Slavonic, is of a composite character, and that no one can say, on the spur of the moment, from what original stock either may have been developed. We usually call the Germans blonde and the Slavonians brunette, yet just as great variations in this respect can be found among the Germans as among Slavonians. Indeed, northern, southern, eastern, and western groups of either nation present such a large number of variations that it is just as difficult to assert that the Germans came from a common stock, as is the case with the Slavonians. Consanguinity and heredity have been urged an explanation of these differences, but it has been proven that certain Slavonic groups are more nearly related to the Germans than to their own Slavonic brethren. If we compare the blonde element among the Poles and Galicians with the brunette Slavonians of the south, it is found that they not only differ with respect to color of skin, of eyes, and of hair, but also in a very marked degree in the structure of their skull; so much so indeed that the former show a greater affinity to our German tribes than to the Slavonian. In Northern Germany matters are still more intricate. There, in some of the old burial fields, skulls are found which might be called Germanic, were it not that they clearly possess Slavonic added character, so that for the present at least these fields must be considered Slavonic burial places. To make the case still stronger, there are found in the famous grave-rows (Reihengräber) of the period of the Franks or of the Merovingians, with their characteristic ornaments and weapons, skulls which very distinctly present the peculiarities of the Germanic type. Corresponding to these in an anthropological sense, a large number of graves have been opened in the east of Germany where similar types of skulls are found; but these are lacking in Frankish peculiarities and are characterized by Slavonic marks. Greater contrasts than these can not be imagined.

It is at present an impossibility and probably will be for all times to trace back to a common type either the Slavonic or the German tribes. When we compare the short and thick skulls of our Alemannic brethren with the long and low skulls of the Frisians and Hanoverians, it is evident that they differ more from each other than is the case with skulls of certain Slavonic or German tribes. Consequently we must give up the idea of an original consanguinity in respect to each one of the historic nationalities. We do not possess as yet any known conclusive series of observations by means of which it can be demonstrated that from dolichocephalous families there have been developed brachycephalous individuals, such as we find among Slavonic or Germanic tribes. It may be possible by means of cross breeding to develop in process of time from a dolichocephalous family a brachycephalous one; but actual proof of this has not as yet been produced. Hence we are compelled to adopt as a solution the theory of "mixed races." A mixed race is one whose elements are people of different blood, not of one blood: it is one which can not appeal to a common origin but which in the course of time was made up of elements of different original races. This theory causes us, as you easily see, to attach but little importance to nationalities as such existing at present. It will be our task to determine the localities of the original elements of this mixture, and to ascertain whence came these brachycephalous and these dolichocephalous peoples. Somewhere there must be a starting point for each of these categories, since upon an anthropological map these distinctions are marked with geological clearness. This difficulty not only exists in Germany or Austria but also in Russia. What are now called Russians are made up of a very composite mass of elements, derived from the farthest parts of Asia, from Turanian and Mongolian stocks. Hence our colleagues in the East are in no less a quandary than ourselves. They too meet wide differences between north and south, east and west.

In the popular mind these questions are very easily considered to be concerned merely with a single nationality, but we must not only try to solve them in respect to one nation, but for the whole of Europe. In attempting to do this our investigations carry us further and further from a consideration of their special relation to individual nations. I may be permitted to say right here, that we are all especially interested to see such investigations carried on in this Austrian Monarchy; for Austria in its peculiar development has preserved in greater purity the remnants of old nationalities, than any other state in Europe. Everywhere else the change of former environments has gone on to a larger extent, the remains of antiquity have been crowded back so far that at present it is very difficult to make collections of the very oldest remains.

We are now occupied with the establishment of a museum in Berlin for German costumes and domestic utensils; we intend to preserve in it everything that can yet be saved from destruction. In some localities the very last relic has been secured for our museum. Here and there we meet with lingering recollections of primitive days, but these can not be compared with the living realities in so many districts of Austria.

A reference to dead and living languages will make plain this contrast referred to. For while a dead language may indeed be studied, the investigation of a living language secures to a greater degree a comprehension of its fundamental elements, than the mere study of authors, each one of whom expresses his own individuality. So that we lose sight of the fact that this individuality of the author studied can not be the portrait of the thoughts of the people to whom he belongs. On this account we notice with especial gratitude the efforts along that line, which are gradually spreading throughout all Austria and of which the late Crown Prince Rudolf was the acknowledged leader.

Extensive labors were carried on under his direction and by reason of his personal participation in them promised to yield rich returns of trustworthy reports taken from life concerning the nationalities of Austria. To-day the place is vacant in which he hoped to stand; at the throne we were considering the establishment of this congress; and it is fitting that I should voice the sorrow of all on account of the loss which this great country has sustained in him who seemed to be one of the most humane princes of this century. We trust that the idea bequeathed to us in his words will not be lost, but prove a precious heritage to Austria, which will be carried on by her to completion. It will be our aim to do all in our power to foster a spirit of union with our neighbors, which is so essential to the success of such an undertaking.

In the department of archæology, you have made large advances during the last few years, completely over-shadowing the rather slow progress of former years, which caused at times a little feeling of impatience in the bosom of your superintendent. Those of us who saw yesterday your new buildings and your finely arranged collections were obliged to ground their weapons. We can not longer keep up our rivalry in view of such magnificence and completeness. Such a palace of science as your Imperial Natural Historic Museum can be found nowhere else, and we too though strangers, must praise most highly the beneficent plans of His Majesty the Emperor, as well as of the Government, which have been executed in such an admirable manner. Here we find revealed the incredible riches of pre-historic materials belonging to Austria. Scarcely can there be found anywhere else a museum surpassing this one.

We are always sure to see in Austria, every possible effort made to put into execution any views which have fully gained ascendancy. I hope therefore that under the direction of Mr. von Hauer, with the assistance of such accomplished investigators as are here to be found, a further development of the pre-historic archæology of Austria may take place and reach such a degree of perfection that the different branches of local types will be arranged into a comprehensive whole.

Several years ago we differed widely concerning the interpretation of certain local finds. At that time the most noted Austrian investigators thought that the original seat of European civilization must have been in the mountains of Austria, while the Germans contended that the starting point must be looked for farther south. I myself, although recognizing the importance of this local development, was in favor of the German theory. It seems to me that every day the bonds are made stronger of a definite connection among the nations of the north and south as regards their civilization.

My own travels in places of ancient human civilization as well as a study of recent literature convince me that the numerous finds made in Egypt and Babylonia prove conclusively that the origins of our civilization are to be found only to a small degree in our own country, or that they have arisen out of individual necessities, but that on the contrary there exists a connection with the pre-historic times of those nations of ancient civilizations, and that from them there have been derived our present lines of culture. I will not say anything further concerning this point, only to call your attention to a publication of investigations in our Berliner Zeitschrift für Ethnologie concerning old weights and measures. These investigations demonstrate again the fact that our present weights and measures existed in all their details in remotest antiquity and were at that time in common use, that our modern measures correspond to the old as far as one-tenth of a gram, and that we therefore have not made any advancement in respect to them since 4000 B. C.

I have stated elsewhere that only a few people can be called inventors. At times it happens indeed that similar inventions are made at the same time in different places, and that the same ideas make their way in different directions, and it is said at such times "these things were in the air." But it is not in the air but in living human beings where such things exist. Yet if at times two men arrive at the same thing, a closer study proves that after all there is a difference. Everywhere, whenever we can follow the history of human culture in individual things, we find that *it was not the work of the masses which determined the great lines of civilization, but the work of individuals, or of individual tribes, or of individual nations, if you please.*

Not only in our study but in other matters however we met with numerous contradictions which for a long time impeded the discovery of the true direction of civilization in general, and the connections of the civilization of different countries. This difficulty is so great because first of all a mass of antiquated traditions remaining until the present must be discarded in order to determine this question aright. There are in Europe, perhaps three or four museums in which Caucasian antiquities are more richly represented than anywhere else, and among them your Imperial Museum here in Vienna occupies a prominent place.

Until a very recent period when these collections came to Europe it was a rigid dogma of philologists and archæologists that the bronze culture had its origin in the Caucasus. Its impossibility has now been proven, for we do not find bronze of a primitive form or mixture in the

Caucasus, but of the same composition as that found in Greece and Italy, and at the same time in an advanced state of development that clearly proves it to be an importation. Whether single articles were imported or only patterns and a knowledge of the art of making bronze matters little. At any rate the invention must have been made in another place.

By examining different countries and nations we succeed in narrowing the territory until by keeping on, we may find the point of beginning of bronze manufacture. We shall probably be unable to find the original inventor, but we shall learn the steps which mankind has taken in its advance regarding bronze manufacture.

It may be mentioned at this point, that just such considerations as these enable us to cast a retrospective glance upon the last twenty years, and to exhibit the progress made by us in the science of archæology. The science of pre-historic archæology twenty years ago had reached in but few places its full development. At that time the museum at Copenhagen was so far ahead of all others that it was considered as an unattainable prototype; next to this was the one at Lund, and later on the one at Bergen. Here there was exhibited a seemingly circumscribed field of civilization which was called for brevity the Scandinavian. The Scandinavians indeed went so far as to believe that their remote ancestors had invented these things, and that only at the time of the Romans had there taken place an influx from without. The aged Nilsson with his Phœnician hypothesis stood all alone. Matters have changed considerably since then. Many Scandinavians to be sure still defend the old view, by pointing out the great development which the older bronze exhibits in the north, but none of them seriously believe that the invention of bronze was really a northern achievement, even though the manufacture in bronze shows numerous northern peculiarities. We take in like manner Chinese patterns and copy them, but although by modifications, the style may be called at last German or Austrian, the Chinese origin never disappears entirely. Among us scarcely any one believes in the Scandinavian origin of bronze. At present we may assume that our Scandinavian friends are convinced that bronze came to them as a finished thing. The formula of its composition was invented before it came to the north. Although spècial peculiarities have been developed and although the art of bronze manufacture seemed to flourish more independently in the north than in the south, nevertheless they must admit that their ancestors were not the inventors of bronze. Here I think lies the main difference between the former and the present theory. Formerly it was thought that the secret lay concealed in the north, that there the origin of our metallurgical art was to be found and that there had lived the original smith from whom our people had inherited their technique. During the last two decades another view has found much favor, and for many good and strong reasons it is called the Indo-German or Aryan theory. Interesting investigations were made to prove how the Indo-Germans in their immigrations from the east and from the central parts of the mountains of Asia, had brought with them on their advance towards Europe, all sorts of things and formulæ, not only the knowledge of the smelting of bronze, but also precious stones like nephrite and jadeite. But this Indo-Germanic theory has received lately some very damaging blows and none more destructive than from the quarters of pre-historic archæology.

In spite of much care, we have not as yet succeeded in finding any patterns in the supposed Asiatic home of bronze. I myself have made strenuous efforts to find original Indian bronzes, but have not obtained types which would justify the statement that this importation alluded to ever took place. Not even sufficient proof can be found for saying that the classic formula of 90 parts of copper and 10 parts of tin was in use in India. This formula remained as constant as the measures of weight and length. Both facts present a good argument for the existence of a continuous communication of knowledge from one generation to the next.

Indian bronzes are zinc bronzes, like mixtures found in our country belonging to the time of the Roman empire. There are no authentic specimens of them found in Europe dating before the Christian era. Prehistoric archæology therefore at the present offers the poorest kind of testimony for the Indo-Germanic origin of bronze. Moreover, the routes of migration of the Indo-Germans are mapped out differently. Some authors put them northward of the Aral and Caspian Sea, others to the south. The northern route must be considered an entirely arbitrary hypothesis, for there have never been found any Aryan tribes in those regions. On the other hand, we find along the supposed southern route of the Indo-Germans mainly a population of brachycephalous peoples, which fills the Caucasus and the Armenian highlands, Thrace, and Illyria. All these differ materially from those inhabiting the north, especially from the Scandinavians. This Indo-Germanic hypothesis is attended with still another difficulty. Existing races in this region not only differ among themselves in their physical composition and are crossed in various ways, but they also diverge widely in many of the conditions of life.

Archæological researches have nowhere led to the beginning of a common civilization in an indisputably Aryan territory. Of course this does not necessitate an attempt to locate the origin of the Ayran race in Germany or Belgium, as has been proposed in the case where the race of Cannstatt or of Neanderthal (a dolichocephalous people) is said to represent the original central stock.

The pre-historic theory of the much abused skull of Cannstatt has been much shattered; it does not fit into that far off period into which our French neighbors place it. Too little attention has been paid to the proposition: *that international intercourse is a more important factor archæologically considered than we are wont to think.* With an increasing conviction of its truth a greater value will be attached to proofs which show that there has been a transmitting of culture from one race to another. Nothing has given me greater joy than the discovery of those large burial fields in the most southern parts of the Austrian Alps, along the coast and in Istria for which we are indebted to the energy of Messrs. De Marchesetti and Szombathy. A number of new links have thus been welded into the chain of the old system of transmission, and the result of these researches will doubtless be embodied in a series of papers, and given to the public.

Let me emphasize right here that these finds are most valuable because they prove a pre-historic international intercourse (not migrations, for this can not be established); and because they exhibit the directions which civilization has taken. They will also beget in our international intercourse a little more modesty and amiability than seems to exist at times on account of a too great sensitiveness about this idea of nationality.

If different races would recognize one another as independent co-laborers in the great field of humanity, if all possessed a modesty which would allow them to see merits in neighboring people, much of the strife now agitating the world would disappear.

A far greater revolution than that which took place in the sphere of archæology has been brought about in anthropological science. At the time of our coming together twenty years ago, Darwinism had just made its first triumphal march through the world. My friend Karl Vogt, with his usual vigor, entered the contest and through his personal advocacy secured for this theory a great adherence. At that time it was hoped that the theory of descent would conquer not in the form promulgated by Darwin, but in that by his followers;—for we have to deal now not with Darwin but with Darwinians. No one doubted that the proof would be forthcoming, demonstrating that man descended from the monkey and that this descent from a monkey or at least from some kind of an animal would soon be established. This was a challenge which was made and successfully defended in the first battle. Every body knew all about it and was interested in it; some spoke for it, others against it. It was considered the greatest question of Anthropology.

Let me remind you however at this point that natural science, as long as it remains such, works only with real existing objects; a hypothesis may be discussed, but its significance can only be established by producing actual proofs in its favor, either by experiments or direct observations. This Darwinism has not succeeded in doing. In vain have its adherents sought for connecting links which should connect man with the monkey; not a single one has been found. The so-called pro-anthropos which is supposed to represent this connecting link has not as yet appeared. No real scientist claims to have seen him; hence the pro-anthropos is not at present an object of discussion for an anthropologist. Some may be able to see him in their dreams, but when awake they will not be able to say that they have met him. Even the hope of a future discovery of this pro-anthropos is highly improbable, for we are not living in a dream, or an ideal world, but in a real one.

At our meeting in Innspruck it looked as if it might become possible to demnstrate amid the excitement, the descent of man from the monkey or some other animal. At present to our regret we do not even possess the means to prove a descent of the individual races from one another. It was not known at that time how difficult it is to prove that all men are brethren, nevertheless laborious attempts were made to show the unity of mankind.

There was an inclination to single out individual skulls and skeletons found among the remains of men in caves, as for instance in the caves of the "Maasthal",) as representative types, and from them make up the races of primitive ages. Some claimed the original race to have been Mongoloid, others contended that the first man was Australioid. It all depended on the question whether the Mongolians or the Australians were the lowest race. The first European must have looked like one of them, it was said. But the first European has not yet been found. At present we know that judging from his remains, primitive man did not resemble a monkey any more than do men of to-day. The ancients were well formed, they bore the same characteristic marks which we find in men of our times; not a single one was so poorly developed as to justify us in saying that he possessed the lowest form of skull.

Twenty years ago little was known of the skull forms of the lowest primitive nations. This accounts for hasty judgments passed; the wildest ideas were afloat about the make-up of the lowest tribes. No one possessed any exact idea concerning the physical construction of the Eskimos, Patagonians, etc. To-day there is scarcely upon our earth a tribe which might be called entirely unknown. There is only one place where there is some possibility of new discoveries,—I mean the peninsula of Malacca;—but even in this place we have an energetic agent at work. Its inhabitants, according to the results of the researches of some, seem to satisfy most nearly the demands made for a lowes trace. Aside from these we know them all,—Patagonians, Eskimos, Bushmen, Veddas, Laplanders, Australians, Polynesians, Melanesians; about many of them we really know more than of European nations.

If for instance you take the case of individual islanders and compare them with Albanians, I may say that more investigations have been made concerning the Polynesian natives than concerning separate groups of Albanians. All these uncivilized nations, which stand so low in their mental development, are becoming gradually known to us. Of most of them we have in Europe good typical examples, concerning whom the most exact observations in respect to their whole organization have been made. Not a few of these died in Europe, and on that account were more especially noticed. We possess greater knowledge concerning the brains of a Patagonian than about the brains of the civilized nations of Asia.

Not one of these examples resembled the monkey any more than (if indeed as much as) it does our own. Now the systematic naturalist determines the lines separating genera and species in the following manner: Whenever he finds that the totality of points of one group equals that of the other, he separates both from related genera or species. If however the respective sums of their points are equal he draws a line between them and makes of each a separate genus or species. Such a dividing line is drawn between man and monkey. Every living race of men is as yet purely human; not one has been found which might be called pithecoid, or which might be considered an intermediate race between man and monkey.

I must however admit that there exists a series of peculiarities found among men which are called pithecoid, and these can not be explained as mere disturbances or hindrances of their normal development. Let me illustrate: The higher apes exhibit frequently an especial development of the skull in the region of the temples. Just as in the case of man, several bones join in the depression beneath the muscles covering this part. From below, the upper edge of the great wing of the sphenoid bone joins the parietal bone; the squamous portion of the temporal bone to which the ear is attached touches this spot from behind, and the frontal bone is joined anteriorly to the other three bones just mentioned. These four bones come together in such a manner that the parietal and the sphenoid bone, joining each other, keep the frontal and the temporal bone apart by being thus unitedly wedged in between them. Now in the skull of the monkey a long process of the temporal bone is frequently found wedging itself in as far forward as the frontal bone, thus separating the parietal and the wing of the sphenoid bone. This constitutes a marked difference of great value, since this does not occur in man, as a rule, but there exist isolated cases where this same peculiarity is found.

As we examine large collections of skulls and formulate the result, we find that certain races show these peculiarities more than others. So far as we can tell, three races especially exhibit them. We find them first of all among the Australian and African, *i. e.*, the black races; then among the yellow in the Malay Archipelago, especially on that chain of islands which connects New Guinea with Timor, and to which are joined the Molucca Islands in the north and Australia in the south.

I lectured only a little while ago* concerning a number of skulls of Alfuros, of Tenimber, among which this peculiarity was noticeable. At the same time another characteristic was observed, namely, the enormous development of the jaws, as shown in a greatly projecting ridge of

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* *Vide:* Transactions of the Berlin Anthropological Society, 1889, page 177.

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the arch of the jaw and of the teeth. Associated with this prognathism there is found an inward curving of the nosé, together with the extreme flattening, as if somebody had sat on it. In this case sometimes the nasal bones grow into one, which scarcely ever takes place in other races of man. These forms also are especially characteristic of catarrhine apes. Hence this catarrhine nose is a kind of pithecoid element (Thermorphy). In certain localities this occurs more frequently than in others, and there may have existed a greater propinquity of relation with apes. It is not without importance to remember that among the anthropoid apes, the gorilla and chimpanzee are found in Africa, and the orang and the gibbon in the Indian Archipelago. But if you inquire farther, may not the Australian and the African blacks or the Malay and the Alfures be the sought-for connecting links which bridge the chasm between man and the ape? No one can answer with an absolute no. It might be possible, but possibility is a great way from reality. For temporal processes, catarrhine noses, and prognathous jaws, do not make an ape; a number of other characteristics are necessary to produce a monkey.

Hypothetically from every piece of skin a monkey may be constructed; no anatomist ever doubted this. But the differences between man and monkey are so wide that almost any fragment is sufficient to diagnose them. Much is still lacking for a demonstration of the theory of descent.

How necessary it is then as we may look at the problems of the future, to make still more far-reaching researches in this particular branch of science which has to do with the earlier developments of the human race. Especially should there be made careful investigations concerning pre-historic man in Australia, and also in Indonesia. If anthropologically-trained physicians would stay there continuously and make researches, perhaps essential and important proofs might be found.

At present they are still lacking, and we can study the early state of man only by means of what old graves, a few caves, and lake-dwellings, and what the present can furnish us. I would not pass over in silence the fact that from all these sources mentioned only specimens of man have been discovered of which we need not be ashamed and whom we may fully acknowledge as brethren. Through the kindness of Swiss colleagues, I was enabled to make comparative examinations of nearly all the existing skulls of the lake-dwellers. It became evident to me that even in those times difference existed between tribes which probably came upon the scene of action one after the other. None of them however was constructed in such a manner as to lie outside of the physical form of our present nationalities.

Again, it can not be said that all races have descended from a single human pair. This matter does not lie within the province of natural science proper. Everybody may decide that to suit himself. Those who, on account of their religious convictions, need a first pair, will encounter no objection from us. A possibility exists, we acknowledge, that all races and tribes may have sprung from a single pair by means of transmutations. But no one has actually demonstrated that negroes descended from white parents, or *vice versa.* Whenever a black tribe is found the naturalist supposes that there were negroes before, and where a white tribe is located that such a tribe always has been white. Of course, all this is likewise a mere supposition, which can not be established. In short, every proof is lacking to show that a nation or a tribe is capable of a total transmutation. This is seen in Egypt. I thought that I could find by means of comparative examinations of the living and the remains and pictures of the dead some points establishing a change of ancient Egyptians into Egyptians of historic times, but I have returned with the conviction that ancient Egypt and its neighboring countries have not essentially changed during all these periods. If Menes really existed, there were in his time negroes, since quite old mural paintings show negroes with all their peculiarities. Nor do the native Egyptians offer any data to speak of. The Egyptian of to-day possesses still the forms of the ancient one. Unfortunately for us, Egyptian skulls and skeletons are not as ancient as we might wish. There has never a skull been seen belonging to the three oldest dynasties. Hence there is no possibility of a continuous list. But anyhow the register goes as far back as 3000 B. C. with positive certainty, which gives us in all some 5000 years. During this long time only one difference has been noticed, namely: An appearance of brachycephalous men in the old kingdom in contrast with dolicho- and meso-cephalous people of the new kingdom. At any rate, definite proof is not wanting that since the beginning of the new kingdom, 1700 B. C., no noteworthy change of type has taken place. A permanence of type accordingly during thirty-five centuries is established.

It does not look unreasonable to assume a certain influence of climate and occupation. In this respect both the straitest orthodoxy and the purest Darwinism agree. Their thesis is the same. The former go as far back as the first human pair, the latter beyond it to the first pair of animals; aside from this they both accept the transmutation of a primitive race into different races. Those can not sustain scientifically their position in the case of man, and these as regards the monkey. If you should ask me whether the first pair was white or black, I must confess I do not know. We have no foundation upon which to base any decision. It can not be supposed that there lived, *e. g.*, in France at the time of the troglodytes all negroes with woolly heads and that from these sprung white and straight-haired people. For other reasons moreover it is not clear to me how or where this could have happened. The very oldest remains show already differences. It sounds very plausible that the north made man light complexioned. But in America where similar conditions exist we do not find any blonde natives. The primitive Germans as well as the Finns of Mongolian origin are blonde, why these should be thus, while the rest of the Mongolians became black or deeply brunette is a question which we can not answer. It must not be forgotten that language does not stand in correlation with outward physical phenomena. On the contrary, they are related to each other in a similar manner as a process of the forehead which may appear as a single mark without its necessitating a corresponding similarity in all the rest of the given characteristics; nor can we say that underneath a light skin there is always one and the same arrangement of internal organs. It may be entirely different.

In this particular direction I have tried from the very first appearance of Darwinism to modify the doctrine of heredity. I recognize as truth the law of heridity, but I ever emphasized, and do so again today, that heredity in man is only a partial one. Man is not subject to a general heredity by means of which all peculiarities are developed in him from generation to generation. If botanists have begun upon a basis of local variations to establish subdivisions, and in that way have instituted within the same genus individual sub-genera or variations with hereditary character, it is a very easy matter to form out of these sub-genera new genera. But the fact that within the same genus there occur individual variations which appear to be hereditary, only proves that the same individual may be the possessor of different hereditary peculiarities.

It is indeed well known that one may inherit peculiarities from both father and mother and thus unite in himself a double heredity; or he may even exhibit characteristics which belonged to his grandparents while at the same time marks may be present which were inherited from his parents. In the same individual may unite then the aggregate of partial heredities, which are more or less limited. There may be many of these parts, but that can not be established. Only in the case of twins it sometimes happens they can not be distinguished without much painstaking observations; whenever they can be distinguished it is done by means of marks peculiar to each one of them. Hereditary characteristics under some circumstances may appear with such prominence that the resulting shape actually differs from the type.

Often people are born with six fingers and six toes. These transmit this peculiarity and whole families of this description come into existence. If this peculiarity were cultivated by in-breeding one might get a whole tribe with six fingers. Something like this exists in the dynasty of Hadramaut in Southern Arabia where only six-fingered descendants have any right to the crown. Certainly these are peculiar formations, but it can not be said on that account that in primeval times all mankind had six fingers. The negroes in the neighborhood of the Congo River have often web-membranes between their fingers and since fishes have not only five but many more single rays in their fins, between which there is found such membrane, while the rays show, also, articulation, the thought suggests itself that web-membranes of the negro must have been produced by a kind of retrograde movement. There are to be found such retrograde movements whether we believe it or not. If for instance a child has the nose of his grandfather we say that atavism is clearly existing, and everybody is satisfied with it. But if the six fingers are traced back to the six rays of the fins of a ray it is looked upon as an imputation. There are great difficulties connected with this subject which can be overcome only by means of heroic effort. I refer especially to the relation between atavistic peculiarities and those acquired by external circumstances. Acquired peculiarities are not atavistic, even when they prove to be hereditary.

During recent years a subject has been very popular which I would recommend for further study, viz, the tailless cats. On the island of Man there is found a race of cats without tails. It has not as yet been explained whether these cats are indebted for their taillessness to a fault of their original parents and by reason of acquired characteristics are propagated in this way or whether there has intervened a disturbance in their development. As to the fact of this taillessness there is no doubt, for we find very frequently similar occurrences at other places, *e.g.*, in Scotland, but how this heredity has taken its rise is entirely unknown. Perhaps the original mother was run over by a wagon and in this way lost her tail and then brought forth tailless cats!

We do not even know how far this law of heredity extends. On account of this uncertainty the question becomes very complicated in its relation to human circumstances. Climate and life may influence human development, although at present no convincing reasons can be given which show such a change in respect to human beings living in our age either in their totality or as individuals through the influence of local climate prevailing at their homes. In these particulars then we are deficient to-day in our knowledge. You may possibly say that it is a strange thing to have gone backward and to know less than people knew twenty years ago.

We know indeed less, but it is our pride that we have our knowledge in such a shape that we really know what we claim to understand. Twenty years ago many things were supposed to be known when people were really ignorant of them. We have made this supposed knowledge the object of scientific tests and natural science has now really taken possession of its wide domain, and we can now say that much that was formerly asserted to be true is no longer admissible. It was supposed by faith, but it never belonged to science. Now the question before us is whether it is not possible with all the auxiliaries to observation and experiment to discover a kind of plan in the natural history of man. Whether we shall ever get to a point where we can show that the home of the negro was the submerged land, which according to English zoologists was the original home of man, the so-called Lemuria, or that this place was the river Rhine, where some claim to have found the most ancient remains of primitive man;—all this we leave for our successors to decide after another twenty years shall have passed.

I can only say to day we have no debts; we have not borrowed from any hypothesis-framer; we do not go about oppressed by a fear that the things to which we hold will be overturned. What we now determine has stability and will prove a foundation for further researches. We have levelled the ground so that succeeding generations may make as much use as possible of these means furnished them by us. It is our confidence based upon the recognition given us by our rulers and the sympathy of the people that in the future there will be no lack of material for work.

Gentlemen, it is now our duty to go to work unitedly and with more zeal than ever before, so that all these questions may be solved which are of such importance to man for his understanding of self, and for his social and political development. Let us take hold then so that real and abiding progress may be ours.

I would propose as our aim to be attained in the coming twenty years that we obtain such an insight into the anthropology of European nations as to be able to present some valuable points concerning the connection of European tribes and to succeed in showing the reasons for existing differences among them.

This much I wanted to say to-day. I beg pardon for speaking so long.

Anthropology is surrounded by a dense fog of traditions, a large number of them useless. Much labor is necessary to bring out its nucleus, just as it is the case with many of our fruits, whose little living kernel is surrounded by thick woody coverings. These germs are to be found in the field of anthropology and they must be opened up in coming days.. May they find as much appreciation from a circle of such interested hearers as I see before me.to-day.

SCANDINAVIAN ARCHÆOLOGY.*

By M. INGWALD UNSET.
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Translated by Prof. L. D. LODGE.

Pre-historic studies made their appearance in Scandinavia before they were broached in any other country. That is easily explained. The pre-historic times of Scandinavia are only separated by a few centuries from present times and extend to the introduction of Christianity into that country about the year 1000 of our era. The Roman legions never set foot upon Scandinavian soil, and the ancient authors have only left us some very enigmatical passages upon the countries of the north. Nor do the Scandinavian traditions shed much light upon the epochs which preceded the introduction of Christianity. On the other hand, Scandinavia possesses an unusual number of pre-historic remains. It is then easy to understand that there should have been developed a peculiar science, founded upon empirical studies of the antiquities themselves, in the north rather than in other countries.

DENMARK.

Passing in silence the unsuccessful attempts of past centuries, the first decade of our century must be considered as the epoch of the birth of a pre-historic science in the north, whose beginnings appear in Denmark. The study of national history received in that country a strong impetus in consequence of the sentiment of nationality which awoke at that epoch in all the Germanic world. It was then that men began in Denmark to direct their attention to the national remains and to regard them as things worthy of study.

In the first rank in this road must be mentioned Prof. Rasmus Nyerup, who published in 1806, an epitome of the national remains of antiquity (*Oversigt over fædrelandets mindesmærker fra oldtiden*), in which he proposes a plan for the establishment of a national museum. At the same time he began to make a collection of national antiquities at the library of the university of which he was the librarian. This was the germ of the pre-historic museum of Copenhagen, a museum now so vast and so famous. The state itself a short time afterwards took charge of the interests of this museum. On May 22, 1807, the King signed a resolution constituting a royal commission of which Professor Nyerup was named secretary. This commission was charged with forming a museum for national antiquities, with watching over the preservation of the remarkable remains still existing in the country, and finally with making known to the public the value of ancient objects which are found in the soil, in order to put an end to their daily destruction.

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* From the *Revue d'Anthropologie*, May 15, 1887, 3d series, vol. II, pp. 311–332.

Under the active influence of this commission the collection originally founded by the private exertions of Mr. Nyerup, became so extensive that it soon gave birth to a new special science, that of pre-historic archæology.

The historian Vedel-Simonsen was not a member at first, but he was one of the most zealous collaborators; he undertook for the commission several tours into the country, in order to collect antiquities and to excite interest in favor of the National Institute recently founded. In this way he had many opportunities of seeing the finds taken from the soil and the tumuli. Relying upon his own experience, he was the first to establish a fundamental principle for the classification and distribution of the chaotic mass of antiquities, the first to propose as a scientific theory the division of pre-historic times into the great paleo-ethnologic periods,—*that of stone, that of bronze, and that of iron.*

In his work entitled: *Udsigt over Nationalhistoriens oldste og mærkeligste Perioder* (Epitome of the most ancient and most remarkable periods of national history), the first volume of which was published in 1813, there is a chapter on the first settlement, the most ancient inhabitants, and the primitive history of the North. He discusses (pages 73–76) the tools and arms of the most remote times, and rejects the opinion then common, that the stone objects are only sacred objects. On the contrary, he pronounces them tools and arms of an epoch in which metals were still unknown, and he fortifies his opinion by citing for comparison the information about the savages of the present time who still use stone tools, and by referring to his observations during his tours undertaken for the new museum. At page 76, he gives a résumé of his ideas in the following very remarkable passage: "The arms and utensils of the most ancient Scandinavians were in the beginning of stone and of wood. These Scandinavians then learned to work copper and even to harden it; so that there result copper axes found in the soil, and lastly (as it seems) iron. So from this point of view *the history of their civilization might be divided into an age of stone, an age of copper, and an age of iron.* These ages were not however separated from one another by limits so exact that they do not encroach upon one another. Doubtless among the poor they continued to use stone tools after the introduction of copper ones, and copper tools after the introduction of iron ones; the same case has arisen in our day with vases of clay, of pewter, and of porcelain. The arms and utensils of wood have naturally decomposed, those of iron have been oxidized in the soil, those of stone and of copper alone have been preserved."

This proposition, already clearly enunciated in 1813, and now recognized everywhere as a fundamental truth of pre-historic archæology, was not however generally accepted at once in Denmark; the materials accumulated in the museum were not yet numerous enough for the truth to be obvious to the eyes of all. It was only in Sweden that some authors admitted the theory of Mr. Vedel-Simonsen; in Denmark his ideas were for a long time only a sort of prophecy of what everybody was going to accept. The man who was to draw from the archæological finds, and from the antiquities themselves, the incontestable proofs of this theory and to secure its recognition throughout the entire world was Christian Jurgensen Thomsen, for fifty years the director of the Pre-historic Museum of Copenhagen, which he raised to the rank of the first institution of that kind in Europe; he has been called the father of the pre-historic archæology of the North. In 1816 he succeeded Mr. Nyerup as secretary of the archæological commission and as director of the museum, a position which he held until his death, in 1865. This remarkable man was truly self-taught,—originally a merchant without erudition, and for that matter little enough attached to books,—but he had very extraordinary natural gifts, an observing mind, and a very delicate perception of objects of art and of antiquities. He was an excellent numismatist and a good connoisseur in art. His trained eye and his fine perception of the style and of the characteristic details of ancient objects permitted him to arrive at a more profound knowledge of pre historic antiquities. For him the aim was no longer to seek to determine and to illustrate pre-historic objects by the interpretation of traditions. Through him, as well as through the young men who attached themselves to this acute connoisseur and to his rich museum, pre-historic archæology became a study of the antiquities themselves; they understood that a knowledge of the very remote times to which these antiquities ascend is only obtained from these contemporary remains by the empirical path and by an inductive method.

Mr. Thomsen mainly exerted his influence by his labors inside of the museum. It was in 1819, that he began to open the latter to the public for a few hours each week; he was always there himself to instruct visitors. In this way he succeeded by degrees in conquering for the Archæological museum a place in the national interests. In the classification and exposition of antiquities he was ever making progress. Very early there began to form in him the knowledge of the three great periods of the development of civilization, and of the way in which an archæological museum should be arranged conformably to this principle. What Mr. Vedel-Simonsen had declared on that subject ten years before does not seem at once to have convinced him. From 1825, however, he expounded to Professor Keyser, of Christiania, his ideas upon the classification of a pre-historic collection on the basis of this chronological principle. We know that he had already, in 1830, realized this method in the museum of Copenhagen, when Mr. B. E. Hildebrand studied there under him. As we have already said, Mr. Thomsen has left only a very few printed works. In his memoirs of 1831 and of 1832, he already puts forth the theory of the three periods; but it was only in his book of 1836, *Ledetraad til nordisk Oldkyndighed* (Manual for the learning of northern antiquity,—a German edition in 1837, afterwards an English one also) that he developed it more at length, and presented it as valid not only for all the North but for all Europe. He expresses himself in the following terms upon the age of bronze, of which he as yet knew scarcely any remains in countries not Scandinavian, but which he thought had prevailed in the rest of Europe (p. 59): "It seems that a very ancient civilization, anterior to the introduction of iron, had spread over a large part of Europe, and that its products have had a very great resemblance in countries very distant from one another. In studying the arms and cutting tools of bronze and the inferences from the discoveries as a whole, one will doubtless be more and more convinced that they have a very high antiquity, and that (especially in the countries of the South) they are exceedingly ancient. If it is admitted that the objects of this sort which are found upon Scandinavian soil are imitations of those which have been imported thither, it is clear that they have been used once in the countries from which they come. If on the contrary the relations ceased there, where they only existed by the migrations of the peoples, one can understand that the inhabitants of the North, having once received from southern countries the knowledge of the most ancient inventions, should—because of the great distance and the interruption of communications, have remained ignorant of the progress and subsequent discoveries made by the most civilized peoples. What exists in the North will thus be able doubtless to instruct us about the similar objects which must have existed in countries where the development entered into the full light of history long before it did in the North."

The Royal Society of Antiquaries of the North, founded by Mr. Rafn in 1825, and directed by him for forty years, had also at this time commenced to enter into more intimate relations with the museum. In the beginning the society expended its activity in editing the literary remains existing in the ancient Norwegian-Icelandic tongue; afterward it devoted itself also to the occupation of collecting and describing the antiquities and of examining the archæological remains of the country. The archæological commission had in 1812–'27, published *Antikvariske Annaler* (Archæological Annals), four volumes; from 1832, it united with the royal society for the publication of *Nordisk Tidskrift for Oldkyndighed* (Periodical of the North for the investigation of antiquity), three volumes of which appeared by 1836. From that year the society began to publish the *Annaler for nordisk Oldkyndighed*, twenty-three volumes of which appeared by 1863; in the same time appeared seven other volumes; *Antikvarisk Tidskrift* (Antiquarian Periodical), 1843–'63, containing also some memoirs treating of ethnographic subjects. To render the most important works accessible also to foreigners a series was printed in French: *Mémoires de la Société Royale des Antiquaires du Nord* (Memoirs of the Royal Society of Antiquaries of the North), 1840–'60, in all three volumes.

Under the direction of Mr. Thomsen a number of young men began then to devote themselves to the study of pre-historic antiquities, working at the museum and undertaking excavations in the country, for example Sorterup Strunk Herbst, but before all Worsaae (1821–'85). By an excellent little book, *Danmarks oldtid oplyst ved Oldsager og Grav høie* (the Antiquity of Denmark, elucidated by the tumuli and finds), which appeared in 1842, he places himself immediately at the head of the archæological authorities of Denmark. It is a statement comprising some conclusions possible to be drawn from the materials amassed and from the facts established up to that time. This book showed the public at once how this new science of pre-historic antiquities could extend and had already extended the horizon of our knowledge. For more than forty years Mr. Worsaae continued to be the chief of the Danish archæologists; he has enriched the science with numerous archæological and archæologico-historical works, and he has opened to the studies new paths.

Pre-historic studies in Denmark made a very considerable stride forward by the discovery of the *kjøkkenmøddings* (heaps of kitchen refuse). It is the illustrious zoologist Japetus Steenstrup, (of whose last memoir the *Revlse d'Anthropologie* gave an account in its last number,) who has the honor of having discovered, examined exactly, and interpreted ingeniously these remarkable relics of the earliest antiquity of Denmark. It was in 1837, that he observed, in some heaps of oyster and other shells which were found in the elevated places of the Danish coasts, some evident products of human industry, incontestable proofs that the formation of these heaps must have taken place after the habitation of the country. As some bones of animals were also found in these heaps, he came to include these formations in the circle of his special studies upon the ancient flora and fauna of the country. It was by examining the turf-pits of Denmark that he discovered that the flora of the country formerly had been altogether different from that of our day, and that he there established the existence of several successive periods of vegetation. Some bones of animals found in the different layers of these turf-pits had also furnished him with the materials for the history of the fauna of the country. Upon the initiative of Mr. Steenstrup, the Royal Academy of Copenhagen in 1848, appointed a committee composed of Mr. Forchhammer as geologist, Mr. Worsaae as archæologist, and Mr. Steenstrup himself as zoologist, to examine these shell heaps. This last gentleman in reality took charge of the labors of the committee. In the reports of the Academy of Copenhagen he gave (1848–'55) a series of famous memoirs upon his admirable studies. The true nature of these heaps was soon discovered by the committee; their formation was evidently due to man; the shells and the bones of animals were remains of things eaten; these large heaps were remains of meals, and that is why Mr. Steenstrup called them *kjøkkenmøddings*, a word afterward naturalized in all languages as a scientific term designating the similar remains found in the most distant countries in the world. By acute studies upon the formation and upon the contents of these heaps, Mr. Steenstrup discovered that they originated among a population of hunters and of fishers who were as yet unacquainted with metals, a population which had lived in a remote age; and that the climate of Denmark, and in consequence the flora and the fauna, were then altogether different from what they are now. That climate was colder; the forests consisted of firs; many animals which have now disappeared from the fauna of Denmark were numerous there; for example, the great European carnivores, the *Bos primigenius*, the *Tetrao urogallus*, etc.; of special interest is the presence of a northern bird, now extinct, *Alca impennis*. The studies of Mr. Steenstrup not only enriched, by their results, Danish paleo-ethnology; indirectly they were also very important by the influence which they exerted upon the severity and the exactness of the method of the natural sciences which was subsequently adopted by the pre-historic archæologists in Denmark.

In this time Mr. Worsaae had already extended the circle of his prehistoric studies beyond the limits of his native land, undertaking journeys into Germany, France, England, and Ireland; he had already published some works containing the results of studies made during these journeys, and had just created thus the comparative method in prehistoric science. Several of his works had been published in foreign languages and had likewise exerted an influence upon the beginning of paleo-ethnologic studies in other countries.

From 1850 to 1870, the museum of Copenhagen was greatly enriched; it increased especially from the materials coming from the systematic excavations of the antiquaries. One event of great importance for the prosperity of the museum and of paleo-ethnology occurred: the reigning king, His Majesty Frederick VII, became warmly interested in paleology; he even made his appearance as an author on the subject. New collaborators were added to the museum to those who were already working there under the direction of Mr. Thomsen, among others Mr. Boye and Mr. Engelhardt who, until 1863, was the director of the museum of Sleswick at Flensburg.

But relying thus upon materials whose number was increasing and upon ever-extending explorations, progress ought to have been made. First an effort was made to sub-divide the three great ages established by Vedel-Simonsen, and Thomsen, and to discover their chronological limits. It is always Mr. Worsaae who marches at the head. In 1854, he published an atlas of illustrations: *Afbildninger fra det Kgl. Museum for nordiske Oldsager* (Illustrations of the royal museum of northern antiquities), an important work, of which the second edition especially, under the title: *Nordiske Oldsager* (Northern Antiquities) 1859, circulated in all Europe, and which, still frequently quoted, has served as a model for several similar paleo-ethnological atlases in different countries. In this work appeared for the first time the sub-division of the age of iron into two periods. About 1840, moreover, few remains of the age of iron were known in Denmark, while those of the age of bronze were numerous; investigators inclined to the opinion that the age of bronze extended there down toward the year 700 of our era. In 1853, Mr. Worsaae, with the co-operation of Mr. Herbst, discovered a first age of iron, characterized especially by numerous imported Roman pieces, and whose duration he determined as from the year 1 to about 500 of our era. In a memoir of 1859, Mr. Worsaae also proposed sub-divisions of the age of stone and of the age of bronze. As to the age of stone, he wished to establish a first period, comprising essentially the *kjøkkenmøddings* with their ground flint, and corresponding to an epoch of transition from the paleolithic age to the neolithic age in the west of Europe; then a second period, characterized by the dolmens and the ground and polished flint; but this sub-division was very energetically contested by Mr. Steenstrup. In the reports of the Academy of Copenhagen, 1859–'62, this question was earnestly discussed by Mr. Worsaae and Mr. Steenstrup; the latter wished to maintain the contemporaneity of the *kjøkkenmøddings* and the dolmens.

Among the most considerable publications of Danish paleo-ethnology about 1870, should be noticed moreover Mr. Engelhardt's descriptions of some great discoveries of the first iron age, or more exactly of the lower Roman epoch, which had been made in some marshy meadows in Sleswick and in Fionia.

The Swiss Morlot contributed much to make Danish paleo-ethnology and its results known abroad. In 1858, he studied a long time at the museum of Copenhagen and on his return to his native land, published several memoirs upon the pre-historic labors of the savants of the North.

SWEDEN.

In Sweden also attention had been directed, in past centuries to prehistoric remains and objects. Beginning with the year 1666, the Government had established a college of antiquities which was charged with forming a collection of the ancient treasures which might be found in the soil. Although that is to be considered as the germ of the archæological museum of Stockholm, only a few objects nevertheless were collected there. When in 1786, King Gustavus III founded the Royal Academy of belles-lettres, of history, and of antiquities, that institution was charged with the care of the remains of the country and with the custody of the collection which had already existed for a century. The Swedish historians often mentioned at this period ancient objects, but it was only in our century that the special study of national antiquities began in Sweden.

At the beginning of this century, Sjöborg, professor of history, at Lund, deserved credit for his activity in investigating the remains of the country, in order to make up its archæological topography and statistics; in 1805, he obtained from the government a decree ordering the preservation of pre-historic remains. The fruits of his labors are published in several memoirs, but especially in a work entitled *Samlingar for Nordens fornælskare* (collections for those interested in northern antiquity), I–III, 1822–'23. The *Götiska forbündet* (Gothic Union), a literary society, founded in 1811, by some learned and patriotic young men, has also done much to spread the knowledge of the national antiquities; its literary organ, *Iduna*, must be considered as the first periodical publication of Swedish archæology. At this time many private collections of antiquities were founded, the most of which have since been acquired by the museum of Stockholm.

The theory of the three ages of civilization enunciated in 1813, by the Dane Vedel-Simonsen, was accepted by Magnus Bruzelius, at Lund (*Specimen antiquitatum borealium*, 1816). The illustrious Geijer approved it in his work, *Svenska folkets historia* (The History of the Swedish People), 1832.

In 1830, Dr. B. E. Hildebrand, of Lund, went to Copenhagen to study there under Mr. Thomsen, numismatics and northern antiquities; on his return he was appointed chief of the archæological collection at Lund, which he classified according to the system of the three periods communicated to him by Mr. Thomsen. In 1833, Hildebrand was called to Stockholm to arrange the numismatic collection and the old museum of antiquities, hitherto however without any importance. Hildebrand thus became the true founder of this museum; in the beginning he classified it according to the ideas of Mr. Thomsen. In 1837, appointed antiquary of the kingdom, he had during a long energetic administration the opportunity of being very active for the enlargement of the museum, so that in 1879, he was able to commit it into the hands of his son and successor, as an institution of the first rank.

Another illustrious man is also to be named at the beginning of paleo-ethnological studies in Sweden in our century. As the introduction to a new edition of his work *Skandinaviens Fauna*, the celebrated zoologist Sven Nilsson, at Lund, in 1834, published a remarkable memoir: *Udkast til Jagtens og Fiskeriets Historie i Skandinavien* (outline of a history of hunting and fishing in Scandinavia). He there sketches the life of the first inhabitants; they were as yet unacquainted with metals, and lived as hunters and fishers; they had only tools of stone and of wood. He gives detailed descriptions of the different kinds of tools found, investigates their use, and compares them with those of peoples still savage, particularly with those of the Greenlanders and Australians. Then after having visited the museums of Copenhagen and several ethnographic collections abroad, he published in 1838–'43 his famous work *Skandinaviska Nordens Urinvanare.* (The

First Inhabitants of the Scandinavian North). In this work, which is of the greatest importance, upon the age of stone, one sees introduced for the first time the methods of comparative ethnography; it is a book which assures forever to its author an elevated place among the founders of pre-historic science. In this first edition, the age of bronze is only treated in the last chapter; the opinion is there maintained that the introduction of that civilization is due to the immigration of a Celtic tribe. It was not until later that he put forth his well-known theory that the age of bronze in Europe is due to the Phœnicians, those commercial mariners of antiquity; this theory has been developed in detail in the second part, published in 1862-'64, of a new edition of his work. In the same way a new edition of the first part, upon the age of stone, was published in 1866. This work, translated into German, French, and English, excited the greatest attention in all Europe, and still enjoys, and with reason, the greatest reputation, although his Phœnician theory perhaps no longer counts any adherents.

Principally under the influence of B. E. Hildebrand, the Swedish Academy of belles-lettres, history, and antiquities, from the year 1856, directed its activity more and more toward archæological topography and the statistics of the remains of the country, the extension of the museum, and of the systematic excavations, and the publication of the results; a throng of able men took part in these labors. During the course of 1860, local societies of antiquaries were founded everywhere in the provinces; these societies did much to spread archæological knowledge, excite interest in its favor, and collect and preserve materials; a series of provincial museums were organized, depending in a certain degree upon the National Museum of Stockholm. The Swedish Archæological Society, founded in 1869, has become the common center of these local societies.

The most of these private societies have published private periodical collections; the principal organ of Swedish archæology appears under the auspices of the Academy of Antiquities, *Antikvarisk Tidsskrift för Sverige* (Antiquarian Journal for Sweden). Among the most important memoirs of this journal must be cited the work of B. E. Hildebrand, published in 1869, upon the carvings on rock, where he first gives the incontestable proofs that these remarkable remains date from the age of bronze. From 1860 to 1870, commenced also the labors of two men still the most celebrated to-day among the Swedish pre-historians: Hans Hildebrand (the son of B. E. Hildebrand), who published in 1866, an important book entitled *Svenska folket under hednatiden* (The Swedish People during the Time of Paganism), in which he treats especially of the relations between the two periods of the age of iron in Scandinavia; and Oscar Montelius, whose work, *Fra jernaldern* (On the Age of Iron), which appeared in 1869, has laid the solid foundations of a chronological classification of the finds dating from the age of iron, by giving detailed descriptions of all those of the northern iron age, accompanied by imported foreign coins.

NORWAY.

In Norway it was the Royal Society for the good of Norway which founded at Christiania the first collection of national antiquities; in 1811, it appointed a commission to form this collection. After the re-establishment of the independence of Norway, by the separation from Denmark in 1814, the society ceded the collection to the university recently founded, where it became the basis of the pre-historic museum, now the most considerable in the country. It was not until 1828, however, that a director of the museum was appointed,—Keyser, a professor of history. Already, in 1825, Keyser, during a visit at the house of Mr. Thomsen in Copenhagen, had learned of the classification into three periods adopted by him in his museum; at Christiania the same principle was adopted from the beginning. This museum grew constantly and rapidly in a subsequent period. To Mr. Keyser, in 1862, succeeded Mr. O. Rygh, who is still the director.

In 1825, another archæological museum was founded in Norway. At that time a number of private citizens, patriotic and interested in the sciences, established at Bergen a museum for the west of Norway, whose collections, especially the archæological, increased rapidly. There also, in 1833, was begun the first Norwegian archæological journal, Urda, of which down to 1846, two volumes and a part of a third were published. It was above all Christie, and the bishop Neumann, who displayed the most activity in the founding and the development of this museum of Bergen.

In 1844, at Christiania, was created the *Forening til norske fortidsmindesmærkers bevaring* (The Society for the Preservation of the Ancient Remains of Norway), which formed a new centre for archæological labors. The society has affiliated members at Trondhjem and at Bergen, and counts members throughout the country; it is subsidized by the state, and its president is always the antiquary of the kingdom, appointed by the government,—at present Mr. Nicolaysen. The society has done much for the preservation and description of the remains; it has undertaken explorations and excavations, and has published a series of works. Since 1815, it has published *Aarsberetninger* (Annual Reports), with plates; among its other publications must be named the work of Mr. Nicolaysen, *Norske fornlevninger* (Norwegian Archæological Materials), 1866, containing information upon all the archæological remains and materials known up to that time in Norway.

About 1870, at Trondhjem, a provincial museum was also organized by Mr. K. Rygh, which has now acquired some importance. In the south of the country Mr. Lorange has formed at Fredericksbald a private collection of special interest, because almost all the materials which are there preserved come from his own excavations.

The most important archæological publications which appeared in Norway from 1860 to 1870, are some memoirs of Mr. O. Rygh, especially one published in 1868: *Den ældre Jernalder i Norge* (The First Age of Iron in Norway). The first period of iron stands indicated for the first time and distinctly characterized in the Norwegian finds. It is a model work as to soberness and soundness of method.

We have followed the development of pre-historic studies in the north down to 1870; here it is proper for us to stop and to conclude this first period.

It is already a long time since the new science pushed itself likewise into the other countries of Europe. The surprising discoveries of Mr. Boucher de Perthes, which formed an epoch, have long since been generally accepted; energetic labors have commenced, especially in geological paleo-ethnology; great attention is directed at once upon prehistoric times and upon proto-historic times. In Germany, particularly in the north of that country, local investigators early began work in the same way as the antiquaries of the north. Let us mention among them and in the first rank Mr. Lisch, at Schwerin. In 1852, a central museum was established at Mayence under the direction of Mr. Lindenschmit. A few years afterwards, in Switzerland, Mr. Keller discovered the palaffites; finally about 1860, Mr. Gastaldi founded the study of paleo-ethnology in Italy.

In 1866, the international congress of anthropology and pre-historic archæology held its first session. It is fitting to end this first section by mentioning the fourth session of this congress, which took place at Copenhagen in 1869. Scholars from all the countries of Europe assembled there to learn the results attained by Danish paleo-ethnology during more than fifty years. The rich museums, the archæological and the ethnographic, both lasting monuments of the aged Thomsen, deceased a few years before, excited the admiration of all. The labors of the congress presented much interest. Foreigners were especially interested in a discussion between Messrs. Worsaae and Steenstrup upon the sub-division of the stone age and upon the chronological characters of the *kjøkkenmøddings*.

II.

About the year 1870, a new phase opens in the history of Scandinavian archæology. Under the ægis of the preceding scholars there is formed a phalanx of young men who bring with them new ideas, tendencies, and methods.

SWEDEN.

The first to be pointed out in this country, at this period, is Mr. Hans Hildebrand, the son (already mentioned) of B. E. Hildebrand. While still young he had the opportunity of making, during several years, long tours to the most important archæological museums of Europe, and thus acquiring a profound knowledge of all archæological materials. The results of these studies have been recorded especially in a memoir the principal parts of which appeared in 1872–'73, in the *Antikvarisk*

Tidsskrift för Sverige: Studier i jämnförande fornforskning. Bidrag til spännets historia. (Studies in comparative archæology: materials to be used for the history of the fibula.) In this excellent work the author classifies for the first time the principal groups of finds of the bronze and the iron age in central Europe; he describes them in their peculiarities and their geographical extension and insists particularly upon the two great groups of the pre-Roman iron age, which he designates under the names of the Hallstadt group and the Zène group, after the most celebrated localities of these finds. Though this work has never been translated in its entirety into a foreign language, it has nevertheless been of great importance in the development of the science, and has formed an epoch in comparative prehistoric archæology. We have seen how Mr. Worsaae, many years before, had already undertaken to compare the archæological data of the north with those of other countries; but it is Mr. Hildebrand who possesses the merit of having sought, almost the first, to give a *systematic* epitome, a *complete* classification of all the material pertaining to a certain archæological age, in this case the one nearest the beginning of historic times in southern and central Europe. Geologically speaking, it is the principal stages and the most remarkable formations of the tertiary period of pre-historic time, which are separated and characterized in this work for the first time. The principal conclusions of this memoir, whether they be essentially modified by the increase of materials or not, will always be of great importance as the point of departure of a new phase in the progress of this pre-historic science. Another memoir of Mr. Hildebrand has the same tendency: *Sur la division du Nord de l'Europe en provinces archéologiques pour l'âge de la pierre polie.* (On the division of the north of Europe into archæological districts for the age of polished stone.) Report of the Congress of Brussels, 1872. In 1873–'80, he published a great work: *De förhistoriska folken i Europa* (the pre-historic peoples in Europe), in which—exhibiting vast erudition—he treats of all the paleoethnological materials then existing in Europe. Among his other works must be mentioned here a memoir upon *les Cassitérides et l'étain dans l'antiquité* (the cassiterides and tin in antiquity). In the *Antikvarisk Tidsskrift*, 1878, two works upon the "Finds discovered by Mr. Schliemann in Troas" (Stockholm, 1878) "and at Mycenæ" (*ibidem*, 1882); then two memoirs treating of comparative ethnology: *Folkens troom sina döda* (the ideas of peoples about their dead; Stockholm, 1874), and *De lägre naturfolkens konst* (art among primitive peoples). The latter, which is concerned especially with the sculpture and carving on bone of the aurochs, the Eskimos and the men of the quaternary period, forms a part of the work of Mr. Nordenskjöld: *Résultats de mes voyages dans le haut Nord* (results of my travels in the far north). In 1879, Mr. H. Hildebrand was appointed antiquary of the kingdom of Sweden. He has shown himself an energetic administrator. This is not the place to dwell upon the dispositions of the Government with regard to the museums and remains, nor to speak of the works which he has published upon the civilization and the arts of Sweden in the middle ages, studies with which he seems to have been most occupied in the last years.

A worthy colleague of Mr. Hans Hildebrand is Mr. Oscar Montelius, whom we have also mentioned already; he is to-day first curator of the museum of Stockholm. By numerous works he has contributed to the knowledge of the antiquities of the North and of other countries of Europe. In 1872, '73, he published (in the *Antikvarisk Tidsskrift*) an extended memoir on the relics of the age of bronze, discovered in the northern and central parts of Sweden, with comparative dissertations upon the bronzes of the North and those of central Europe. Since then, he has continued his studies upon the bronze age in the North, and has published a series of them, seeking to throw some light upon that remarkable epoch by profound researches upon the bronze age in central and southern Europe. Recently (in 1885), he has published his definite conclusions upon this pre-historic age in a great work which our next article will discuss in detail. Among his numerous other works we must mention his atlas: *Svenska fornsaker*, 1872–'77, a French edition of which appeared in 1873–'75—*Antiquités Suédoises, arrangées et décrites par O. Montelius* (Swedish antiquities, arranged and described by O. Montelius), with 658 figures; of the text corresponding to this atlas there has appeared only the first part, *Stenalderen* (the age of stone) 1874. An abridged collection of the results of Swedish archæology has been given by him in his book on *Pre-historic Sweden*, Stockholm, 1874. (A French translation from the Swedish original, of 1873. A German edition, much enlarged, appeared in 1885, at Berlin, under this title: *Die Kultur Schwedens in vorchristlicher Zeit.*) In the *Antikvarisk Tidsskrift*, v, he published, in 1880–'82, a great comparative study: *Spännen från bronsalderen*, etc. (Fibulæ of the age of bronze and of the first age of iron); a study which is the result of extended travels, and which treats in a very detailed manner especially of the Italian fibulæ *del prima etá del ferro* (of the first age of iron). A succinct résumé of this memoir is inserted in the *Matériaux pour l'histoire de l'homme* (Materials for the history of man, 1880, pp. 583–589). In the works of Messrs. Hildebrand and Montelius a peculiar method of research, *typology*, with which we shall occupy ourselves in our next article, plays a prominent part.*

Other archæologists attached to the museum of Stockholm are to be mentioned,—Messrs. Stolpe and Eckhoff. Both have made valuable investigations and have published reports of them, Mr. Stolpe especially, upon the famous findings of Björkö, of the second age of iron. Mr. Eckhoff, since 1880, has published the description of the antiquities of the Bohuslän, a model of the archæological topography of a country.

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* We cannot refrain from calling attention to a review of Mr. Hildebrand's Scandinavian archæology, which appeared in the *Revue d'anthropologie* in 1873, p. 523.

Among the men who, without being attached to the museum, have contributed to the pre-historic archæology of Sweden must be mentioned Mr. Wiberg, director of the lyceum of Gefle. From 1861 to 1873, he published several studies upon the relations of the ancient peoples of the Mediterranean to the ancient inhabitants of Scandinavia; then upon the influence of the Greeks and Etruscans on the age of bronze in the north of Europe (1869). Moreover, numerous local societies have also displayed great activity and have given proofs of it in their periodicals. Important contributions to archæological literature have also been furnished by private individuals; for example, a work on "*Les antiquités de Wärend* (The antiquities Wärend), a district of southern Sweden, by Mr. Wittlock, 1874; then a ceramic monograph on the clay funeral vases found in Sweden by Mr. Strale, 1873. The great illustrated work of Mr. Baltzer, of Götheborg, on the glyphics upon the rocks of Bohuslän, of the age of bronze, commenced in 1881, is not yet finished.

The Swedish periodical publications which we have mentioned above, page 320, have been continued in this period; since the year 1872, the Academy of Antiquities has added to them a new *Manadsblad* (monthly bulletin) containing less extended memoirs and especially information on recent finds and excavations. In 1873, the Academy also commenced the publication of a grandly conceived work, *Tekningar ur Statens historiska museum* (Illustrations of the National Archæological Museum). This magnificent work is destined to comprise several volumes of figures on the most important series of the museum of Stockholm.

In 1874, the International Congress of Anthropology and Archæology, having held its seventh session at Stockholm, the Messrs. Hildebrand, father and son, together with Mr. Sven Nilsson, did the honors of Swedish archæology to their most illustrious colleagues from all the countries of Europe. In 1879, Mr. B. E. Hildebrand, the true founder of the museum of Stockholm, resigned the direction of the museum and his position as antiquary of the kingdom; he was succeeded by his son, Hans Hildebrand; he was still living five years ago. In 1883, Sven Nilsson also died at Lund, almost a centenarian.

DENMARK.

Mr. Worsaae, during all this period, has continued to be the chief of the pre-historic archæologists there. Among his numerous works we must notice an important memoir which appeared in 1872, in the *Aarböger* on the archæology of the countries situated to the east of Scandinavia, a French edition of which appeared in the *Memoirs* of 1873-'74 under this title: *La colonisation de la Russie et du Nord scandinave et leur plus ancien état de civilisation* (the settlement of Russia and of the Scandinavian North and their most ancient state of civilization). He demonstrates that the theory of the immigration of the Scandinavian peoples from the East finds no support in archæological facts. In the *Aarböger* of 1879, he publishes a study embracing vast territories: *Des âges de pierre et de bronze dans l'ancien et le nouveau monde* (on the ages of stone and of bronze in the Old and the New World):—French translation in the *Memoirs*, 1880. In this memoir he advances the opinion that these archæological ages have existed in eastern Asia and in America, and that the system of the three periods has thus a certain value in the whole world. He draws thence the conclusion that in the developments of civilization, there is not only a parallelism, but also a true relation, even between the most distant races. In his book, *Nordens Forhistorie*, 1878, second edition, 1881 (the pre-history of the North), he seeks to give a complete epitome of the results of northern archæology. In his last years he occupied himself with researches by which he believed that he could open new horizons upon our knowledge of pre-historic civilization; comparative studies of antiquities from the point of view of their forms and of their ornamentation, which, according to him, must almost all have derived their origin from religious symbols; and detailed observations on the usages and rites according to which antiquities are deposited he thought that he could even form conjecttures about the mythology and the religious life of pre-historic times.

In his book, The industrial arts of Denmark, London, 1882 (South Kensington Hand-Book), he has published some conclusions on this subject.

By the side of Worsaae the men whose names have been mentioned above—among them Mr. Engelhardt especially—have also been active at the museum of Copenhagen during this period. His works treat of the age of iron. We call attention to one of his articles of 1871, *Romerske Gjenstande fundne i Norden* (Roman objects found in the North), a French résumé of which exists in the *Memoirs*, 1872; then to two memoirs on the tombs of the iron age in eastern Denmark (in the *Aarböger*, 1877, cf. *Memoirs*, 1878–'79); and in Jutland (in the *Aarböger*, 1881). Constructed according to a larger plan is his work of 1875, *Klassisk Industris og Kulturs Betydning for Norden* (Influence of classic industry and civilization upon those of the North in antiquity in the *Memoirs*, 1875–'76). Mr. Engelhardt died in 1881.

In Denmark also a new generation of archæologists has formed at the museum in the period of whose history we are giving a summary. Mr. Sophus Müller has published there several admirable works on comparative archæology, relying not only upon Danish materials, but also upon a profound knowledge of all the riches existing in foreign lands, where he has several times visited the most important museums. In a study published in the *Aarböger*, 1877, he succeeded in sub-dividing the age of iron in Denmark in a more detailed manner than any one had done before. In 1876, appeared his work on "The periods of the age of bronze," in which he seeks to demonstrate that the two groups of the northern age of bronze, established by Mr. Worsaae, and defined in a more complete manner by Mr. Montelius, rest upon differences rather in topography than in chronology. In 1880, he published an excellent work on "Decoration with animal designs in the northern age of iron" (*La décoration avec des motifs animaux dans l'âge du fer nordique*) and in Europe during the epochs of the migrations of the peoples. In the *Aarböger* of 1882, is contained his important memoir, *Sur les origines et le premier développement de l'âge du bronze en Europe, éclairi par les trouvailles dans le sud-est de l'Europe* (on the origin and first development of the age of bronze in Europe, elucidated by the finds in the southwest of Europe). There the author treats of the finds discovered by Mr. Schliemann and of the materials derived from southern Russia and Caucasus. These three works have also passed through German editions.

Among the young men connected with the museum of Copenhagen Mr. Henry Petersen ought also to be named. In the number of his publications must be mentioned his memoir of 1875, *Helleristninger i Danmark* (a résumé in the *Memoirs*, 1877; "Notes on the sculptured stones of Denmark"); and further, *Stenalderens gravformer og deres chronologie* (the different kinds of tombs during the age of stone in Denmark and their chronology, *Aarböger*, 1881); the remains of northern Germany are also here treated. Otherwise, the works of this author are mainly taken up with the archæology of the middle ages.

Among the other archæologists who have been active during this period in Denmark we must name also Mr. Boye, previously mentioned, who has published several memoirs upon national antiquities; then Mr. Zink, who in the *Aarböger*, 1871, published a very important memoir *Sur les tombeaux de l'âge du bronze et leurs relations avec ceux de l'âge de la pierre* (on the tombs of the age of bronze and their relations to those of the age of stone.) Mr. Vedel, the governor of the island of Bornholm, who in the *Aarböger*, 1869, 1870, 1872, 1878, and 1883, (*cf. Memoirs*, 1872, 1878, 1879,) has given the report of his researches on the pre-historic antiquities of this island, researches so excellent, that there is not perhaps in northern Europe any territory so correctly explored from the point of view of its antiquities as the island of Bornholm.

The able artist Mr. A. P. Madsen, published in 1868–'76, a splendid illustrated work: *Afbildninger af Danske Mindesmärker og Oldsager* (Illustrated Danish remains and antiquities), three volumes in 4to, containing 125 engraved tables. Another Danish artist, Mr. Magnus Petersen, has also devoted himself to the illustration of Danish antiquities.

A proof of the ardor with which national archæology in Denmark was cultivated also by private individuals is afforded by the two magnificent works published in 1878 and 1884, by Mr. de Sehested. In the vicinity of his château of Broholm, on the island of Fionia, this gentleman had for a series of years investigated the archæological remains with minute care, and undertaken considerable excavations, the reports of which are contained in the two works mentioned. Of very special interest are the descriptions in the second work (posthumous) of the long series of technical experiments undertaken by the author during several years upon the making and the utility of stone implements; for example, he had a wooden house constructed, using only stone tools.

In the same way after the year 1870, Mr. Steenstrup continued his studies on the pre-historic flora and fauna of Denmark. For example, his memoir included in the Reports of the Academy, 1872, on the contemporaneity of the *Bos primigenius* and the forests of fir in Denmark, in which mention is made of some flint chips buried in animal bones, as proof of the hunts conducted against the deer during the stone age, is important.

Quite an interesting episode in the development of Swedish archæology is the vehement controversy with the German archæologists which took place from 1876 to 1880. Before that time the system of the three periods had already been vigorously contested in Germany, where the archæological materials were scattered in a multitude of small collections, and where (owing to the lack of great deposits of objects) one could with difficulty understand the principal phases of the development of civilization. Mr. Worsaae had been obliged several times to repulse the German attacks against northern archæology, especially those of Mr. Lindenschmit. In 1876, Mr. Hostmann, in the *Archiv für Anthropologie*, made a furious assault against the theory of the three archæological ages, and disputed especially the existence of an age of bronze. On the northern side M. Sophus Müller entered the lists; others joined the two champions; the controversy continued for several years. If the attacks have in no respect been able to overthrow the system of the three periods or annihilate the age of bronze, it will be found perhaps that northern archæology has received a salutary influence from the criticisms of German scholars.

During these fifteen years (1870–'85), the periodical publications before mentioned have been continued by the Royal Society of Antiquaries of the North at Copenhagen. In *Aarböger for nordisk Oldkyndighed* (Annals for the study of northern antiquities), and in the *Mémoires de la Société royale des Antiquaires du Nord* (Memoirs of the Royal Society of the Antiquaries of the North) new series were commenced in 1866. Moreover, investigators have worked energetically during this epoch to complete the archæological exploration of Denmark, and numerous excavations have been undertaken by the scholars connected with the museum. In 1874, Worsaae, then minister of religion, procured an annual subsidy from the revenue of the state for the investigation of the antiquities of the country. Every summer an archæologist and an artist associated have since 1875, travelled through several districts parish by parish, writing lists and detailed descriptions of the known remains and findings, and taking the measure and the designs of all the ancient remains still existing. When these labors some years hence shall have been finished, there will be in Europe no country archæologically so well explored and known as Denmark.

NORWAY.

The archæological society of this country, under the direction of Mr. Nicolaysen, the antiquary of the kingdom, has continued since the year 1870, its investigations and its excavations, the reports of which are found in the annals of the society. In 1875, was established at the University of Christiania a chair of pre-historic archæology (perhaps the first ordinary chair for this science in any university in Europe?) to which was appointed Mr. O. Rygh. The administration of the museum of Christiania was attached to this chair. This period saw the most active Norwegian archæologists: First, those already mentioned, then Mr. Lorange, the director of the museum of Bergen since 1874, Messrs. Bendixen and Undset. Among the publications must be mentioned a memoir of Rygh on the *Deuxième âge du fer en Norvège* (Second age of iron in Norway), which appeared in the Danish *Aarböger*, 1877, and above all a large and splendid atlas, which appeared in 1880–'85, *Norske Oldsager* (Norwegian antiquities), 732 wood cuts, with the text in Norwegian and in French.

Among the publications of Mr. Lorange must be noticed, *Om spor af romersk Cultur i Norges ældre Jernalder*, 1873 (The traces of Roman civilization in Norway during the first age of iron). A remarkable work, *Langskibet fra Gokstad* (The ship of Gokstad), by Mr. Nicolaysen, was published in 1882, with a number of plates and figures, the text in Norwegian and in English, in 4to. He there describes a large ship of the epoch of the Normands (about the year 900) which he was able to dig out of a tumulus and have transported to Christiania, where this unique relic is now preserved in the archæological museum, of which it constitutes the principal ornament. Mr. Undset, connected with the museum of Christiania, has for his part contributed to comparative archæology by works founded upon studies made during extended travels. Reference may be made to a memoir by him, *Fra Norges oldre Jernalder* (On the first age of iron in Norway, published in the Danish *Aarböger*, 1880);* then a book entitled, *Études sur l'âge du bronze de la Hongrie* (Studies on the bronze age of Hungary), Christiania, 1880, in which he has treated of the relations between the bronze age of central Europe and that of the Scandinavian North. In 1881, appeared his great work: *Jernalderens Begyndelse i Nordeuropa* (the beginnings of the iron age in northern Europe. A German edition of it was published in 1882, under the title, *Das erste Auftreten des Eisens in Nordeuropa*. In this book he speaks of all the materials of central and especially northern Europe, which date from the epoch of the transition from the bronze age to the age of iron.

I conclude this retrospective review of pre-historic archæology in

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* The circumstance that Norwegian archæologists often publish their memoirs in a Danish periodical journal is explained by the fact that Denmark and Norway have the same literary language.

Scandinavian countries, which has become essentially a history of its developments in the North, with a single remark. It is possible that some persons may be surprised that under the title of pre-historic archæology I include works treating of Roman antiquities or dating even from a later period. It is because the Scandinavian countries were plunged in pre-historic darkness until nearly the year 1000 of our era. The knowledge of northern doings and developments before that epoch should be sought there principally in an empirical manner, by the inductive study of all the archæological materials, whatever they may be. The word pre-historic has a signification altogether relative. The conditions of France, for instance, about the year 2000 before our era, are absolutely pre-historic, while the civilization of the valley of the Nile, having followed its course for many centuries, was already in the full light of history.

PROGRESS OF ANTHROPOLOGY IN 1889.

By Prof. OTIS T. MASON.

INTRODUCTION.

Merely for the convenience of bringing together those subjects that are most akin, and not to draw hard and fast lines in a vigorously growing science, the same method will be pursued here as in last year's summary. The order of presentation will be: General or Encyclopædic Anthropology, Biology, Psychology, Ethnology, Language, Technology, Archæology, Sociology, Philosophy, Folk-lore and Religion, and Hexiology.

Under the heading of Encyclopædic Anthropology, the following classific concepts cover the entire ground:

(1) General treatises, annual addresses, courses of lectures, dictionaries, encyclopædias, general discussions, classifications of the science.

(2) Societies, their organization, scope, enterprises, history, and lists of their publications.

(3) Journals, proceedings and transactions, the organs of associated bodies.

(4) Periodicals, like L'Anthropologie, devoted wholly or in part to anthropology.

(5) Annual assemblies, caucuses, congresses, general meetings of a national or international character.

(6) Laboratories for general study.

(7) Museums and collections, public and private, their scope, contents, methods, catalogues and history. Expositions.

(8) Albums, galleries, portfolios, methods of illustrating anthropology.

(9) Libraries on anthropology, catalogues, bibliographies, check-lists, and devices for ready reference, classification of books.

(10) Instructions to collectors.

I.—GENERAL ANTHROPOLOGY.

(1) Each year some distinguished anthropologist brings together the results of his lifetime work in a general treatise upon the natural history of man. In accordance with this unwritten law the historian

calls attention to the works of Sergi and Turner in the current year. Characteristic addresses were delivered by Galton and Virchow, the former before the annual meeting of the Anthropological Institute, the latter before the general meeting of German anthropologists in Vienna. The volumes of the Bibliothèque Anthropologique continue to make their appearance. This series is designed to give expression to the ripest thoughts of the French Société d'Anthropologie. The series of Smithsonian Annual Reports now embraces two volumes instead of one, as formerly. Part I contains general papers; while in Part II will be found only such as are based on material in the National Museum collections.

(2) Happily for the diffusion of knowledge, innumerable societies and organizations are now to be found in every land, studying mankind. It would be well to enumerate them all. The best collection of titles will be found in the very last Smithsonian list of foreign and home correspondents. Scudder's catalogue has already become antiquated by the death of many societies and the birth of others. Indeed the anthropological part of it was never full. Nothing is more desirable, and the suggestion is here made with the hope of stirring up an interest in the subject. In the bibliography appended to this report most of the great national societies are noticed, especially in connection with their journals and proceedings. The personnel of the American local societies is generally represented in the American Association. The same is true of England and France. The leading spirits of local organizations are to be seen in the British Association and the French Association. It is only in Germany that a general anthropological annual meeting is held, in which the sole topic considered is the natural history of man. The national organization of Germany is most complete in this regard. Every meeting publishes a stenographic report in Correspondenz-Blatt.

(3) What is true of societies is true of their journals. A full list can not be given. If the following should be carefully studied, nearly all that is good will be found reviewed or at least catalogued by author and by title.

The American Anthropologist, Washington; *Archiv für Anthropologie*, Braunschweig; *Archivio per l' Antropologia*, Firenze; *Bulletins de la Société d'Anthropologie de Paris; Journal of the Anthropological Institute of Great Britain and Ireland*, London; *Journal of the Royal Asiatic Society of Great Britain and Ireland*, London; *Mittheilungen der Anthropologischen Gesellschaft, in Wien; Verhandlungen der Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte*, Berlin.

(4) The most gratifying statement to be made in this summary is the fact that every popular magazine, weekly or daily newspaper, and every course of lectures for the people, contains a great deal of the very best anthropological material. It is frequently said nowadays to the publishing committees of technical and scientific journals, "We can not

afford to have our papers appear in our society organ because the subscription periodicals offer good prices for them." This fact marks an epoch in the history of anthropological literature and invites the societies to explore new fields to which the general reader has not arrived. Indeed it is impossible to chronicle all the periodicals of purely scientific character that lend their pages to our pens.

If the following journals be scrutinized in their original papers, reviews, and book-lists, little that is desirable will escape the reader:

Academy, London; *American Antiquarian and Oriental Journal*, Mendon, Ill.; *The American Naturalist*, New York; *L'Anthropologie*, Paris; *Athenæum*, London; *Ausland*, Stuttgardt; *Internationale Archiv für Ethnographie*, Leiden; *Nature*, London; *The Popular Science Monthly*, New York; *Science*, New York.

5. The four events of national interest each year are, the British Association for the Advancement of Science, the American Association for the Advancement of Science, the Association Francaise pour l'Avancement des Sciences, and the Allgemeine Versammlung der deutschen Gesellschaft für Anthropologie. During the year 1889, the first named met in Newcastle-on-Tyne, the second in Toronto, the third in Paris, in connection with the Exposition, the fourth in Vienna.

The programme of the anthropological section of the British Association was as follows: Marks for bodily efficiency in examination of candidates for public service, Francis Galton. Early failure of pairs of grinding teeth, W. W. Smith. Development of the wisdom teeth, Redolfo Livi. Left-leggedness, W. K. Sibley. Occasional eighth true rib in man, D. J. Cunningham. Proportion of bone and cartilage in the lumbar section of the vertebral column in apes and in men, *id.* Model of the head of a man said to be one hundred and six years old, *id.* Head and shoulders of a young orang, *id.* European origin of early Egyptian art, J. Wilson. African airs and musical instruments, Governor Maloney. The Vikings the ancestors of English-speaking nations, P. B. du Chaillu. Origin of the Aryans, Isaac Taylor. Ethnological significance of the beech, Isaac Taylor. Right of property in trees on another's land, Hyde Clarke. Report of committee on the tribes of Asia Minor. Report of committee on anthropological notes and queries. Report of committee on anthropological measures taken at Bath. New anthropometric instrument for the use of travelers, F. Galton. Instruments for measuring re-action time, *id.* The Smithsonian Institution in relation to Anthropology, T. Wilson. The study of ethonology in India, H. H. Risley. Former beliefs and customs of Torres Straits islanders, A. C. Haddon. Notes collected at Morvat, New Guinea, Edward Beardmore. The British race in Australia, Dr. McLaurin. Color of the skin in certain Oriental races, T. Beddoe. Temperature of negroes and Europeans in tropical countries, R. W. Fellcin. Sensibility in Europeans and in negroes, *id.* The Esquimaux, Fridtj of Nansen. Northumberland in prehistoric times, G.

Rome Hall. Implements of stag's horn with whales' skeletons in the Carse of Stirling, Sir William Turner. The origin of human faculty, G. J. Romanes. Brain functions and human character, B. Hollander. Topography of the brain in relation to the surface of the head, Professor Fraser. Classification of sociology, G. Weddell. Fire-making in North Borneo, S. B. J. Skertchley. The tribes of South Africa, T. Macdonald. Report on occupation and employments, their effects on the development of the human body. Report on the northwestern tribes of Canada, six plates, pp. 797–893. Report on an archæological map of the British Isles.

Colonel Mallery, of the Bureau of Ethnology, opened the session of the anthropological section of the American Association with a striking paper entitled Israelite and Indian. The following is the programme of Section H: Aboriginal fire-making, Walter Hough. Shinto, the religion of the Japanese, Romyn Hitchcock. Siouan terms for "mysterious" and "serpent," J. O. Dorsey. Gens and sub-gens in four Siouan languages, J. O. Dorsey. Principles of evidence relating to the antiquity of man W. J. McGee. Evolution of ornament, W. H. Holmes. Mounds of North Dakota, Henry Montgomery. Iroquois white-dog feast, W. M. Beauchamp. Missions Indians of California, H. W. Henshaw. Successors of paleolithic man in the Delaware Valley, C. C. Abott. Winnepeg mound region, George Bryce. Artificial languages, David R. Keys. New linguistic family in California, H. W. Henshaw. The Parsee towers of silence, Mrs. R. Hitchcock. Seega, an Egyptian game, H. C. Bolton. Onondaga Shamanistic masks, De Cost Smith. Gold ornaments from Florida, A. E. Douglas. Alphabet of the Winnebago Indians, Miss Alice C. Fletcher. Great medicine society of the Ojibwa, W. J. Hoffman. Algonkin Onomatology, A. F. Chamberlain. Indian personal names, J. O. Dorsey. Huron-Iroquois of the St. Lawrence and lake region, D. Wilson. Gesture language of the Blackfeet, J. McLean. The African in Canada, J. C. Hamelton. Indian burial in New York, W. M. Beauchamp. Portrait pipe in Central America, A. E. Douglas. Government of the Six Nations, Oji-ja-tek-ka. Ancient Japanese tombs and burial grounds, R. Hitchcock. Explorations around the serpent mound, Ohio. Aboriginal monuments in North Dakota, Henry Montgomery. Little Fall quartzes, Franc E. E. Babbitt. A Mississaqua legend, A. F. Chamberlain. Places of gentes in Siouan camping circles, J. O. Dorsey. Onomatopoeia interjections, etc., J. O. Dorsey. Ancient pit-dwellers in Yezo, R. Hitchcock. Steatite ornaments from Susquehanna River, A. Wanner. Eskimo of Cape Prince of Wales, Hudson St., F. F. Payne. Contents of children's mounds, H. H. Ballard. The Accads, Virginia H. Bowers.

The programme of the French Association was made far more interesting by the illustrative collections in the Paris Exposition. The following subjects were discussed: The Svastika and the cross as religious emblems, Abbe Rochon. Anthropometric characters of the French, A. Bertillon. Stone Age in Denmark, Valdemar Schmidt. Pre-Roman times in Lorraine, F. Barthelemy. Stone disks found in neolithic cemeteries, Chauvet. Ethnic energy, J. Laumonier. Stone chests in the dolmen of Grulennec, F. Gaillard. Digging of a mardelle in Gard, Delort. Photographs of Mexican antiquities, Boban Duverge. Existence of semi-domestic herds in the Magdalenian epoch, Piette. Age of bronze in the Gironde, Dr. Berchon. Prehistoric right and left hand, G. de Mortillet. Anthropometry of a series of Algerians, Dr. Manouvrier. Skulls from the sepulchral grotto of Masdfrech, Dr. Prunieres. Form of the thumb, Dr. A. Block. Minerals used in making prehistoric objects, Thomas Wilson. Paleography among the Arabs, Tarry. Physical characters of the Japanese, Verrier. Stone disks found in the neolithic sepultures, Chauvet. Histologic researches as a complement to morphological studies in the brain.

The address of Dr. Virchow before the German society was a review of twenty years in anthropology. The papers read were as follows: General meeting of the German anthropological society in Vienna, from August 5 to 10, 1889, with adjourned meeting to Buda Pesth, August 11 to 14. Inspection of the pre-historic exhibition and collections in the Royal Natural History Museum. Preliminary report upon the adoption of a common scheme for anthropometry and nomenclature of the brain. Anthropology in the last twenty years, R. Virchow. The present state of pre-historic studies in Austria, Moriz Hoernes. The protection of pre-historic antiquities, von Tröltsch. Report of the central commission on art and historic monuments, M. Much. Pre-historic times in middle Europe and their relation with neolithic times, J. N. Woldrich. Contemporaneity of mammoth with diluvial man in Mahren, J. Maska. Stone boring in ancient stone implements, Theodor Ortvay. Bronze age in Bavaria, J. Naue. Similarities of form in bronzes, at home and abroad, Gundaker Graf von Wurmbrand. Alphabetic characters on Dacian finds, S. von Torma. Engraved bones and antlers in the caves of Kulna and Kostelik, Martin. Report of the committee on the same, Kriz. Daggers in women's graves of the bronze age, Julia Mestorf. Archæological finds, gold and silver, Grempler. Report on physical anthropology; the measurements of recruits, J. Ranke. Physical characters of the peoples of the Austrian Alps, Zuckerkandl. Human molars, Zuckerkandl. Present knowledge of crania, Schaaffhausen. Crania Americana ethnica, R. Virchow. Positions of the ear on the head, J. Ranke. Placenta in men and apes, Waldeyer. Diluvial finds from Mahren; bronze finds in Austria, Szombathy. Cemetery of St. Lucia in Kusterlande, Marchesetti. Finds and their positions in Lengyel, Wosinsky.

The tenth session of the International Congress of Anthropology and Pre-historic Archæology was held in Paris, at the College of France, from August 19 to 26, under the presidency of Prof. A. de Quatrefages.

The following is the programme: (1) Erosion and filling of valleys. Filling of caverns in relation to the antiquity of man. (2) Periodicity of glacial phenomena. (3) Art in the alluvium and in the caverns. Value of palæontological and archæological classifications for the quartenary epoch. (4) Chronological relations between the civilizations of the stone, the bronze, and the iron periods. (5) Relation between the civilizations of Hallstadt and other Danubian stations on the one hand and on the other that of Mycenæ, Tirhyns, Hissarlik, and the Caucasus. (6) Examinations of the quaternary skulls and skeleton parts found during the last fifteen years. Ethnic elements belonging to the different ages of stone, bronze and iron, in Central and Western Europe. (7) Ethnographic survivals which can throw light on the condition of primitive peoples in Central and Western Europe. (8) How far do the analogies of archæology and ethnology authorize the hypothesis of relationship, or that of pre-historic migration.

The enormous advantage of having the Congress in Paris during the Exposition was apparent in the large attendance and in the frequent visits which were made to the anthropological museum of Paris, and to the many sections of the Champs de Mars, and the Esplanade des Invalides under the very best of guidance.

6. The model workshops of anthropologists is the Laboratoire d'Anthropologie in Paris. Even here the counting, weighing, measuring of capacity, surface, distance, and angulation is confined to the human body. Galton's laboratory in London and Wundt's psycho-physical establishment should be added, with the assistance of the vital statistician and the census director to make the whole complete. The system employed by Alphonse Bertillon for the measurement and identification of criminals in the Palais de Justice in Paris, is being adopted in many cities in our own country. Under the names of Benedikt, Galton, Hitchcock, Rollet, Topinard, and Virchow in our bibliography will be found titles of publications on this branch of anthropology.

7. Kristian Bahnson, of Copenhagen, has rendered a lasting benefit to the student in his pamphlet on ethnographic museums, first published in Denmark and translated in the Mittheilungen der Anthropologischen Gesellschaft, in Wien. Museum history is the subject of an elaborate paper by Dr. Goode, of the Smithsonian Institution. Dr. Bastian, in the transactions of the Berlin Anthropological Society, writes on American collections. Reinach, on the museum of the Emperor Augustus, should not be overlooked.

The Peabody Museum in Cambridge publishes carefully prepared reports of its explorations and accessions each year. New zeal and activity have characterized the management of the American Museum in New York, and of the collections in Philadelphia. In the reports of the National Museum in Washington will be found detailed statements of work done and of the accessions.

The memorable event in our science was the Paris Exposition, and especially that portion of it called "Exposition retrospective du travail et des sciences anthropologiques." The design of this portion of the world's great fair was to trace in outline by means of specimens, re-productions, authentic documents and villages inhabited by native peoples, the steps of human genius from their first trace to the present moment. Associated with this exhibition of invention were the specimens of man himself, shown in the savage just as he came from the hands of nature, and in other races as he has improved with time. Finally, the cabinets of skeletons and the soft parts of the body in plaster and papier-maché were so installed as to exhibit man associated with his inventions; the skull and the brain, laboratory of thought and discovery; the skeleton and its contents, articulated machine to execute the conceptions of the central office.

A building entitled Palais des Arts Liberaux was devoted to the serial display of the history of invention, a large space on the Esplanade des Invalides was covered with villages of Africans and natives of south-eastern Asia. The whole Exposition was filled with the climaxes of modern thought in every land, the Champs de Mars was fringed with structures which enabled the student to grasp in a *coup d'œil* the history and the natural history of architecture.

The arrangement of objects embraced the following classes: (1) Anthropology and ethnography, anatomy and races of mankind. (2) Liberal arts. (3) Arts and trades. (4) Transportation. (5) Military arts.

In addition to this, a very interesting conception was that of showing also the organized machinery for the study of anthropology in Paris. To this end the Société d'Anthropologie, the École d'Anthropologie and the Laboratoire d'Anthropologie, under the regime of public instruction, made a display in the Palais des Arts Liberaux. The institutions of Paris, united more or less for studying the natural history of man, are the following:

(1) *Société d'Anthropologie*, founded May 19, 1859, publishes bulletins and memoires. The collections of the society are styled, since 1880, the Musée Broca, and the literary collections, Bibliothèque de la Société d'Anthropologie. Two prizes, the Godard and the Broca, furnish an effective stimulus to thorough work.

(2) *Laboratoire d'Anthropologie* (École des hautes Études), founded by Broca in 1878. During the period from 1878 to 1889 the Laboratoire published 378 separate titles.

(3) *École d'Anthropologie*, first authorized in 1876. This school is an annual course of lectures by the most distinguished men in France upon the different branches of anthropology.

The following foundations are accessory to the three above named.

(1) *Société d'Autopsie mutuelle.*—In 1876 a group of members of the Société d'Anthropologie formed a fraternity, the object of which is to

conduct a scientific autopsy upon the members as they die. Each one signs a will conveying his cadaver to the society, to be used in the furtherance of the science to which he has devoted himself while living.

(2) *Réunion Lamarck.*—In 1884, the admirers of Lamarck formed a union for the erection of a monument to the great naturalist, and they brought together in the Exposition his works and other testimonials of his greatness.

(3) *Bibliothèque des Sciences Contemporaines.*—This is a series of works on anthropology conducted by M. M. Hovelacque, Issaurat, Andre Lefévre, Letourneau, Mortillet, Thulie, Veron. The list as now made up is as follows: (1) La Biologie, by Charles Letourneau; 518 pages, 112 cuts. (2) La Linguistique, by Abel Hovelacque; 454 pages. (3) L'Anthropologie, by Paul Topinard; 576 pages, 52 cuts. (4) L'Esthetique, by Eugene Veron; 524 pages. (5) La Philosophie, by Andre Lefevre; 640 pages. (6) La Sociologie, by Charles Letourneau; 624 pages. (7) La Science Economique, by Yves Guyot; 600 pages, 67 figures. (8) Le Préhistorique, by G. de Mortillet; 678 pages, 64 figures. (9) La Botanique, by M. de Lanessan; 570 pages, 132 figures. (10) La Géographie Médicale, by Dr. A. Bordier; 688 pages, with figures. (11) La Politique expérimentale, by Leon Donnat; 504 pages. (12) Les Problèmes de l'Histoire, by Paul Mongeolle; 498 pages. (13) La Pédagogie, by C. Issaurat; 512 pages. (14) L'Agriculture et la Science Agronomique, by Albert Larbaletrier; xxiv, 568 pages. (15) La Phisico-chimie, by Dr. Fauvelle; xxiv, 512 pages.

(4) *Dictionnaire des Sciences Anthropologiques.*

8. As we have frequently said in these summaries and elsewhere, the anthropologist must collect things, all possible knowledge about things, and he must also imitate the architect, mechanical engineer, and patent attorney in collecting working drawings. Now this last he has neglected until quite recently. The portfolios of Prince Roland Bonaparte, of Hayden, of de Mortillet and others, are well known, but now we may have such modified by the cheap processes of photographic printing. The anthropological gallery should also include pictures of men and things in action, their physiology, and their anatomy. But this part of our subject lies still chiefly in the future.

9. The segregation of anthropological material from natural history collections in all the great centers of intelligence has been almost immediately followed by the formation of anthropological libraries. The societies also have their centers where books are gathered. For instance, in Cambridge, Massachusetts, the Peabody Museum has its own library, and in addition to that the keeper of the great library in Harvard University sends to the Peabody Museum a duplicate of every card in the Harvard Catalogue which bears an anthropological title. This is an excellent system. At Washington, while material is more abundant, the facility of finding a book is not so good. The material is housed in the Capitol library, the Surgeon-General's library, the

Smithsonian or National Museum library, and the Bureau of Ethnology at the Geological Survey. In Paris, the Société d'Anthropologie, the École, and the Laboratoire all have their books together in one room, but these are far from exhausting the resources of that great city. In Copenhagen the royal librarian turns over the books on various specialities to the department most interested. The precise method followed in Berlin, London, Dresden, Leipsic, and other great centers is not known, but the Peabody plan is far the best, of exchanging cards, when the books are not in duplicate.

In addition to the lists here given the student should carefully study the appendices to the American Anthropologist, Archiv für Anthropologie, Mittheilungen der Anthropologischen Gesellschaft in Wien; and for biological topics, the Index Medicus and Surgeon-General's Catalogue. All the publications of the United States Government are given in Hickox's Guide.

10. Among the books of instruction to collectors of anthropological information no one has had greater popularity or done more good than the little guide published by the British Association for the Advancement of Science. After several years of well earned praise it appears in a new dress, with such corrections as time and experience have suggested. The general tone of all such manuals is toward more rigid and multiplied observations. Professor Goode's epigram, that "a good museum specimen is an exhaustive and truthful label illustrated by an object" is appreciated in all lists of questions. The material history of man must be studied by natural history methods, and as these methods improve, the science will need to re-write its question books.

II.—BIOLOGICAL ANTHROPOLOGY.

Biological anthropology in a restricted and scientific sense is what is learned about man by the biologist in his laboratory and in the use of his instrumentalities of research. The psychologist, the linguist, the ethnologist, the ethnographer, the sociologist, have all need of this man's aid, but it would be entirely contrary to the use of words to declare that the first named investigators were biologists only.

No better way can be devised of showing how the body in health and disease has been invaded by this most zealous class of workers than a list of the principal publications for a year. And this is here appended as a study in the scope of biological anthropology:

Bodily efficiency, Galton: Body proportion in Bavaria, Ranke, Reisch: Brachydactylie, Dérode: Brains, Dercum: Caudal appendage in man, Rabaud: Cephalic index in Provence, Fallot: Cephalometry of negroes, Virchow: Chest measure, Maschkovski: Color, in France, Topinard: Color of the eyes and hair, Topinard: Consanguinity and idiocy, etc., Bourneville and Courbarien: Consumption among the Sioux, Treon: Contortionists, anatomy of, Dwight: Crania of Canstadt, Neanderthal, and Olmö, D'Acy: Crania from East Africa, Virchow:

Craniometry and cephalometry, Benedikt: Craniometric apparatus, Koeler: Craniometry, Virchow: Cretinism, Arnozan: Darwinism, Wallace: Deaf mutes, Riccardi: Deformation of children, Porter: Degeneration by marriage of kin, Coleman: Dental irregularities of Indians, Townsend: Descent and disease, Eccles: The ear in anthropology, Gradenigo Tulia: Evolution, Cope; Dewar; Girard: Evolution and the structure of the human body, Heger: Fœtal measurements and sex, Davis: The human foot, Ellis: Goitre, its etiology and distribution, Capus: The hand in the animal series, Topinard; Virchow: Head growth in Cambridge students, Galton: Hereditary transmissions, Goodall; Hoke: Heredity, Galton; Hohngren; Warfield; Weismann: Heredity and alcoholism, Legrain: Heredity and atavism, Nicolucci: Heredity and disease, Lithgow: Heredity, physiological and psychological, Dolan: Hermaphrodites, Barnes; Deniker: Human degeneracy, Sergi: Human variety, Galton: Hyoid bone, anthropological value, Wortmann: Inca bone, Matthews: Inferiority of the left side of the body, Duchenne: Inheritance, Galton: Inheritance of injuries, Weismann: Irregularities of teeth in normal and abnormal persons, Talbot: Left-leggedness, Owen: Macrobians in Greece, Ornstein; Marriage and descent, Tylor: Marriage and heredity, Nisbet: Measurements of soldiers, Baulin: Microcephaly, Anton: Mongolian eye, Drews: Mortality of soldiers in French colonies, Lagneau: Natality in France, Saporta: Orientation of crania, Benedikt: Osteology of the Veddahs, Thomson: Parturition, normal posture in, King: Pelvis, Russian female, Runge: Periodicity in weight—growth in children, Zacharias: Physical development, Hambleton: Physical development of children, Gratzianoff: Polydactyly in horses, Von Mojsisovics: Prolongation of human life, Hammond: Proportions of the human body, Bertillon: Proportions of the body in Europeans, Topinard: Skin of Europeans and Malays, Glogner: Spinal curvature in Australians, Cunningham: Stature, Fröhlich: Steatopygia, Gillet de Grandmont; Topinard: Supernumerary auricles, Morgan: Supernumerary mammæ, Sutton: Surd-mutism, Riccardi: Syndactyly, Baum; Robin: Transformism, Virchow: Use and modifications of organisms, Ryder.

III.—PSYCHOLOGY.

The best work done in the field of physiological psychology finds an efficient reporter in our own language through the pages of the American Journal of Psychology. The removal of Prof. Stanley Hall from Baltimore to Worcester meant only a more vigorous prosecution of the laboratory work.

Astronomers will be glad to follow up the researches of Dr. Stanford respecting what they call personal equation. In four papers filling nearly two hundred pages, Dr. W. H. Burnham narrates first the history of theories concerning memory. He says, "The continued Platonic and Aristotelian influences may still be noticed in these modern theories, the former appearing in the transcendental conception of memory, which has been taught by the German idealists, and appeared in modified form in the Scottish school, and later its ablest champion in Lotze; the latter appearing more or less in the empirical conceptions of the Associationists, Herbartian, as well as English, and in modern physical theories. Finally it must be plain that whatever be the relative merits of the idealistic and the physiological theories of memory, the facts of introspection have been pretty thoroughly worked over in the continued discussions of memory from the days of Plato and Aristotle down to the last German student who has contributed a theses Zur Theorie der Reproduction. After our historical orientation, the quarter of the horizon that looks most promising is in the direction of empirical study." The progress of empirical research is then comprehensively sketched by Dr. Burnham.

The value of language study in mental discipline has been questioned in our day as compared with the pursuit of the natural sciences. From a purely physiological side and in a most ingenious manner Dr. M. Putnam Jacobi examines the subject. *A priori*, the study of language must be an extension, more or less complex, of the process of acquiring language;—the highest physiological acquisition that distinguishes the human race from the lower animals. After noting the portions of the brain whose activities are involved in so simple a process as the utterance of a word so commonplace as "bread," Dr. Jacobi says: "It is plain, therefore, that to learn the name of a thing and to learn how to use this name, much more mental action is required than simply to acquire sense perceptions about it. The acquisition of foreign languages in addition to the native tongue multiplies the number of verbal signs which the mind habitually couples with visual impressions. In registering and using these multiple signs, the mind is compelled to more complex operations than when only one sign is used. When in different languages different primary words or roots are used to represent the same object; then the mind using them all becomes acquainted with the several aspects of that object which have impressed the minds of those among whom these different names have sprung up. Thus a larger impression of the object is formed, and the mind of the speaker, which is rendered more flexible and active by engaging in more complex internal processes, is also enlarged by a richer store of external impressions. The immense mental discipline to be derived from the study of the European languages is likened to the delicate manipulation of the fingers as compared with the gross movements of arms and legs. The acquisition of language develops the mental sphere in which ideal conceptions arise, combine with one another, and generate endless successions of new ideas. The process of acquiring foreign languages, in addition to the mother tongue, modifies the original process, by extending, refining, and complicating it. Impressions are immensely multiplied and the mind becomes accustomed to take cognizance of such subtle differentiations that its delicacy of perception is indefinitely increased. The capacity to appreciate subtle distinctions, more subtle than those existing in nature outside the mind, is essential to scientific work." The whole essay of Dr. Jacobi should be studied by those who all along have felt the superiority of language study, but who could not translate their convictions into modern psycho-physical phraseology.

The accumulation of psychological literature has been chiefly in the fields of abnormal psychology, such as hallucinations, alexia, amnesia, aphasia, apraxia, illusions, idiocy, alcoholism, melancholia, and insanity; animal psychology, child-mind, dreams, experimental psychology, hypnotism, memory, nerve action in health and disease, physiological psychology, psycho-physics and reaction time; the senses and the disorders of sleep. There is no doubt that, whatever may be one's ultimate theory of mind, psychologists, like astronomers and biologists, have often to wait for the instrument maker. Indeed, there seems for that reason to be no further advance possible in speculative psychology until the instrumental side of the study is improved.

Centers of ideation in the brain, Hollander. Das Morel'sche Ohr; a physic study, Binder. Hereditary degeneracy, psychic state in, Magnan. Instinct, Fauvelle. Intellectual fatigue, Topinard. Memory, Burnham. Memory in surd mutes, Riccardi. Mental faculties of anthropopithecus, Romanes. Mental fatigue, Galton. Merycismus, ruminatio humana, Sievers. Notion of space, de la Rive. Observations of rude phenomena, Langley. Opening address Clark University, Hall. Origin of human faculty, Romanes. Personal equation, Sanford. Physiology of aversion, Mantegazza. Psychic time measure, Fricke. Psychology of spiritualism. Jastrow. Sense, problematic organs of, Lubbock. Thought, experimental science of, Ardigo. Notion of space, Axenfeld.

IV.—ETHNOLOGY.

The classification of mankind by physical characters has been resumed with vigor by J. Deniker (Bul. Soc. d'Anthrop. de Paris, ser. 3, vol. XII, 320–336). From Linnæus to our day, only four or five races or species have been recognized on physical marks. Bory de Saint Vincent, Desmoulins, d'Omalius d'Halloy, and Fr. Müller admit fifteen or sixteen, or perhaps a greater number of "races" or "species," but they differentiate them by linguistic or sociologic criteria. Only the classifications of I. Geoffroy Saint Hilaire (1858), and of Topinard (1879), reckon more numerous divisions (eleven and sixteen races) based upon physical characters. In 1885, Topinard, then in possession of more minute details, increases the number of races to 19.

The tables here given were presented to the Anthropological Society of Paris at its session of June 6 (1889). M. Deniker takes the ground that in the divers peoples, nations, hordes, tribes, etc., which we now see scattered over the earth, we have not a clear group of species as in zoology. These are mixtures of elements the most heterogeneous; there are no pure races on the earth. In classifying races therefore we must seek further than among the ethnic groups for the fundamental units. Save two or three peoples, there is not an ethnos on earth that is not the product of mixing two or three or several races, and it is our task to discover the elemental races. The first step in this analysis is to distinguish the *types*, by which is meant ensembles of salient characters associated and incarnated in a number of individuals. Now these *types* may appear in more than one area, pure or altered, belonging to peoples which at first glance seem to have had no connection. Such is the Negrito type, seen in its purity among the Aetas, the Mincopies, the Sakai, etc., and turns up here and there among the Melanesians, the Australians, the Malays, the Nagas, the Dravidians, etc.

That which varies is the proportion in which a type enters into an ethnic group. When it is altogether preponderating, the people are nearly a pure race, as the Bushmen, Aetas, Mincopies, or Ainos. The *types* are the units of classification and the numerical preponderance of individuals therein gives importance to a type as a constituent element in an ethnic group. Just so far as we are able to go back from type to race we can tell the races which go to make up a people. M. Deniker makes out thirteen of these races, a small number of which are pure, the others are represented by varietes, two for the Ethiopian, Xanthochroi, Indonesians, and three or more for others. Now these thirteen races are evidently variations of a smaller number (perhaps only one) of species of the genus Homo.

The first table differentiates the thirteen races into the thirty types fixed by M. Deniker. The first group corresponds with the Oulotriches of Bory Saint Vincent (Ulotriches of Haeckel), and comprehends Negroes, Melanesians, and Bushmen. The second group comprehends blacks with curly hair, but not woolly, Negritos, Australians, Ethiopians, and corresponds to the Australoids of Huxley. In the third group, wavy haired Caucassians, is subdivided according to Huxley into the brown, Melanchroi, and the blonde, Xanthochroi. The fourth group comprehends races with white, yellow, or yellowish skins and slightly wavy hair, Uralo-Altaic, Ainos, Indonesians. The fifth group, finally, includes Mongoloids and the Americans.

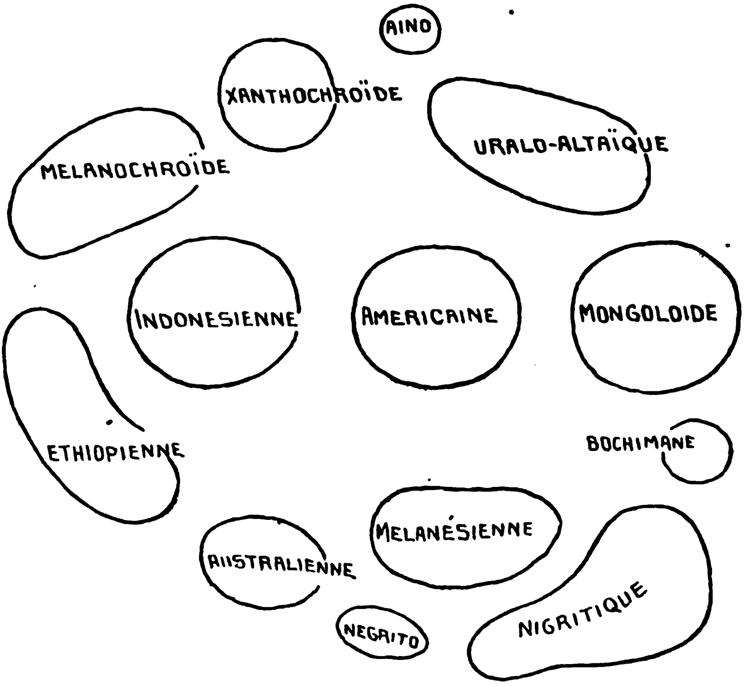
TABLE I.—DENIKER'S CLASSIFICATION OF HUMAN RACES.

			RACES.			TYPES.	
Hair crisp, woolly.		Yellow skin; short; steatopygeous	I. ***Bushman*** (Koi-koi partly).			*Bushman*.	1
	Skin black.	Nose wide, straight, flat; lips salient; forehead protruding.	II. *Nigritic*	Dolichocephalic	Nose flat, medium stature, prognathic	*Negro* (of Sudan).	2
					Nose prominent, tall, little prognathic	*Bantu* (Zulu).	3
				Brachycephalic, short stature		*Akka*.	4
		Nose recourbé, large at the tip, supraciliary ridges prominent.	III. *Melanesian*			*Melanesian* (Papuan).	5
Hair frizzled.	Skin black.	Short; not very hairy	IV. *Negrito*	Brachycephalic, nose wide, depressed		*Negrito*.	6
		Short or medium; hairy	V. *Australian*	Dolichocephalic, nose wide, depressed		*Australian*.	7
		Skin brown; nose salient	VI. *Ethiopian* (Kushite, Hamitic partly).	Tall; nose straight or aquiline		*Bédja* (Galla, Peul, or Foulba, Nubian).	8
				Short or medium, nose often retroussé		*Dravidian*.	9
Hair wavy and fine; nose salient and narrow.		Skin sunburnt; hair black, eyes dark	VII. *Melanochroic*	Mesocephalic	Straight nose, stature medium	*Indo-Atlantic*, or *Aryan* (Indo-European, Mediter.).	10
				Dolichocephalic	Nose recurved, pointed, aquiline, occiput prominent.	*Arab* (Aramaean).	11
					Nose straight, narrow toward the tip	*Berber* (Kabyle, Fellah).	12
				Brachycephalic	Nose hooked, narrow, hair frizzled, body hairy	*Assyrioid* (Semito-Iranian).	13
					Nose straight. fine, often retroussé, short of stature.	*Rhetian*, or *Celto-Ligurian* (Mediter.).	14
		Skin white, rosy; hair blonde, eyes light	VIII. *Xanthochroic*	Dolichocephalic, tall, hairy		*Nordic*, or *Kymrian* (Scandinavian).	15
				Brachy cephalic, medium height, pilous system little developed		*Karélian*.	16

Hair fine, straight, and distinctly wavy.	Skin white, lips thin.	Nose narrow; not hairy; cranial form diverse.	IX. *Uralo-Altaic* (Turco-Finnish).	Nose retroussé, hair blonde			*Suomi* (Western Finns).	17
				Brachycephalic, hair brown			*Lapp.*	18
				Nose straight and coarse.	Mesocephalic, or dolichocephalic		*Ugrian* (Ostiak, Samoyede, Eastern Finns, Tubal).	19
					Brachycephalic		*Turkish* (Turco - Tartar, Turanian).	20
		Nose wide; hairy	X. *Aino*	Dolicho cephalic			*Aino.*	21
	Skin yellowish; lips fleshy		XI. *Indonesian* (Malayo-Polynesian)	Hair wavy, skin olive			*Polynesian.*	22
				Hair nearly straight, skin yellow brown			*Malayo - Indonesian* (Moï, Thaï, Naga, Dyak, Miaotsé).	23
Hair coarse, straight, smooth.	Skin yellow or whitish yellow; eyes "mongoloid;" nose depressed, small; forehead protruding.		XII. *Mongoloid*	Brachycephalic, medium stature.	Nose narrow, fine; face rounded		*Mongol.*	24
					Nose gross; face elongated		*Tunguz.*	25
				Dolichocephalic	Face round; short of stature		*Eskimo.*	26
	Skin reddish, yellow or olive; eyes straight, wide open; nose salient and large; forehead prominent.		XIII. *American*	Brachycephalic	Nose aquiline; stature tall or medium		*Red-skin.*	27
					Nose straight or a little curved	Short	*Southern Indian.*	28
						Tall	*Patagonian.*	29
				Dolichocephalic	Nose straight, often retroussé; short		*Palaeo American* (Fuegian-Botocudo).	30

In the second table M. Deniker ingeniously shows by the size and location of spaces the relative importance and relationship of races.

TABLE II.—RELATION OF HUMAN RACES.



The subjoined list refers to titles in the bibliography and will guide to the principal publications of 1889.

During the winter of 1889–'90 Dr. Daniel G. Brinton published two papers in the proceedings of the American Philosophical Society maintaining that the ethnic affinities of the ancient Etruscans lay in the direction of the Libyans on the North African coast. His arguments were that the Etruscans were tall, blonde, and dolicho cephalic, which was also the type of the Libyans; that their social life and form of government was much closer to African than to Asiatic models; that their own traditions unanimously stated their advent on Italian soil to have been by sea from the south; and finally, that their language, what little we know of it, seems to have roots and forms, explainable by the Libyan dialects. This last argument Dr. Brinton expanded at some length in his second paper entitled "A Comparison of Etruscan and Libyan Names," in which he attempts to analyze a number of Etruscan personal mythological and geographic names by the various modern and ancient Libyan dialects.

General Works.—Classification of races, Deniker. Classification of races, Lombard. Comparative ethnography, Forrer. Crossing of races, Bonnafont. Degeneration of races, Kaarsberg. Egyptian classification of races, Poole. Ethnographic parallels, Andree. Ethnologic studies, Ollivier-Beauregard. Ethnology and anthropology, Lissauer and Kollmann. Origin of the races, Lamotte. Posture, influence on race, etc., Thompson. Pygmy races, Flower. Race and evolution, Fauvelle. Race and language, Holmes. Racial portraits, earliest, Petrie. Race in history, LeBon. Three human subspecies, Lombard.

North America.—American Indians, Henshaw; McLean. Arizonians, Inca-bone among the ancient, Matthews. Aztecs, Biart; Sotomayer. British Columbia, Boas; Br. Association, Hale. Indian and Israelite, Mallery. Indians of the District of Columbia, Mooney. Indians of Siletz Reservation, Dorsey. Eskimo, East Greenland expedition, Holm. Eskimo of Hudson Strait, Payne. Eskimo race and language, Chamberlain. Lucayan Indians, Brooks. Melungeons, Burnett. Mexico, Hale. Snanaimuq (Nanaimo) Indians, Boas. Onondaga customs, Beauchamp. Yaquis of Mexico, McKenzie.

South America.—Botocudos, Branner. Brazil, races of, Lomonaco. Paraguay, inhabitants of, Stewart. Venezuela, ethnology of, Ernst Marcano.

Europe.—Anglo-Saxons at Rome, Tesoroni. Basques, Charency. Bosnia, Asboth. Etruscans, ethnic affinities, Brinton. Gypsies in western Europe, Bataillard. Gypsies, bibliography of, Crofton. Hungarian, Filtoch. Lapps, Fuebert Dumonteil. Norway and its people, Bjorson. Rumanian ethnology, Pulszky. Russia, Ikow; Leroy Beaulieu. Slavic ethnology, Krauss. Sweden in heathen times, Montelius. Types of population in Vienna, Peis. Zigeuner, Weisbach.

Asia.—Ainos, hair and eyes of, Lefevre; Török. Annam and Tonkin, Planchert. Aryans, D'Acy; Hale; Lapouge; Rendall; Sayce. Babylonian empire, races of, Bertier. Caucasus, anthropometry in, Von Erckert. Hittites, Dickerman. Hyksos, Tomkins. India, aboriginal tribes of, Driver; Quarterly Rev. CLIX. Israelite and Indian, Mallery. Japan, Dickson. Jews, comparative anthropometry of, Jacobs. Ostiaks, Samoyedes, and Ziriens, Rabot. Turkestan, Russian, Capus. Samoyedes, Rabot. Western Asia, early races of.

Africa.—Algeria and Tunis, Ober. Angolese, Darnes. Berbers in Marokko, Quedenfeldt; Rinn. Black races of Africa, Verrier. Bushmen, Cuvier; Topinard. Egypt, ethnographic types from, Thomkins. Hottentots, Deniker. Masai-land, Thomson. Morocco, Constant; Harris; Martiniere; Soller; Thompson. Negroes of subequatorial Africa, Hovelacque. Yoruba country, Batty.

Oceanica.—Australia, Lumholtz. Borneo; Daly; Posewitz. Caroline Archipelago, Kubary. Fiji Islanders, Trotter. Gilbert Islanders, Parkinson. Baduwis in Java, von Ende. Hawaiian Islands, Coan. Malays, Bassler. Malay Archipelago, Laugen. New Caledonian crania, Manouvrier. New Guinea, report. Report of special commission for 1887 on the British New Guinea, Archæol. Rev., Lond., 111, 276, New Hebrides, Martine. Maoris, Tregear. Maoris, White. Moa, hunters of New Zealand, Mc'Donnell. Nicobar Islanders, Man. Papuans, Hamy. Papuans, Schellong. Crania, Philippine Islanders, Struve. The Island of Reunion, Blondel. Solomon Islands, Woodford. Tasmanians, Ling Roth.

By looking over the bibliographic list accompanying this summary it will be readily seen that there are many works of inestimable value to the ethnologist which would be indexed under other catch-words. Mr. Galton's paper on gold working among the Peruvians, and Mr. Kinahan's on Irish proverbs, were written—the former from a technical point of view, the latter for a folk-lore journal, but the student of the Peruvians or the Irish could omit neither their arts nor their lore. The best the summarist can do is to base his index on the motive of the writer with such cross reference as he can make.

V.—GLOSSOLOGY.

The eighth international congress of Orientalists was held in Stockholm and Christiania from the 2d to the 13th of September. The meeting was under the immediate patronage and presidency of His Majesty King Oscar. Never in the history of science have more elaborate preparations been made for the entertainment of such an assembly.

The congress was composed of five sections, as follows: (1) Semitic and Islam. (*a*) Languages and literatures of Islam. (*b*) Semitic languages, other than Arabic; cuneiform texts and inscriptions. (2) Aryan. (3) African, including Egyptology. (4) Central Asia and the far East. (5) Malay and Polynesia.

At the Paris Exposition the student of living languages did not lack for opportunities to investigate the natives of every part of the globe. In the separate exhibits, colonial headquarters, cafés, bazaars, and, above all, on the Esplanade des Invalides, the highest and the lowest could be investigated.

An amusing example of the French desire to exhaust the possibilities of a subject was furnished by the placard posted in every available space by the proprietors of the Decauville Railroad. This was a narrow gauge tramway running to all parts of the Exposition, and the passenger was warned in more than twenty tongues not to expose his head or his arms or his legs to the trees along the route.

The volume of Paul Regnaud, on the origin and philosophy of language, treats the subject purely from a natural history point of view. The study is divided into three parts: (1) Exposition of theories already proposed. (2) Attempt at a new theory. (3) Future of languages and linguistic studies. Under the first head are reviewed, language as a revelation, as an instinct, as an invention, as having a spontaneous origin. In the second part the evolution of language is considered in in the two lines of form and sense. Upon the subject of the future of language, M. Regnaud, from purely natural reasons, does not hold to the possibility of a universal language.

To follow minutely the literature of comparative language, no better guide can be found than Techmer's Internationale Zeitschrift fur Allgemeine Sprachwissenschaft, founded in Leipzig in 1884, and now published in Heilbronn. The editor commences in the first number of vol. IV, issued in 1888, and completes in the second number, issued in 1889, a bibliography of language publications in 1886. The first ten pages are devoted to journals and periodicals, with their contents. A liberal definition is allowed to the word language, so as to include papers on general ethnology. Then follow 162 pages of titles with elaborate digest in fine print, one of the most exhaustive helps to the general student. To leave no stone unturned for the reader's convenience, Dr. Techmer furnishes at the close a syllabus of the entire catalogue, and with each catch-word the name of the authors who have written on that subject and the page where his title and review may be found. Would space permit, the whole syllabus should be here re-produced, but the classification without the names here printed will show the magnitude of the scheme:

Science of language in general. History of the science of language.

I. Natural history and language: Relation of this study to anthropology. (1) Acoustic methods of expression, including phonetics, anatomy, storing of language, rhythm, metric; (2) optical and other modes of expression; (3) present condition of acoustic and optical expression, phonetic writing, sound writing, shorthand, orthoepy and orthography, principals of transcription, universal speech, deaf-mute language.

II. Psychological side of language study: (1) Relations to psychics; (2) roots; (3) suffixes, affixes; (4) words; (5) semiology and change of meaning; (6) analogy; (7) etymology; (8) psychological subject and predicate.

III. Historical side of language study: (1) Phylogenetic development of speech; (2) origin and prehistoric development; (3) relations to mythology; (4) relation to the science of religion; (5) relation to ethnography; (6) relation to aesthetics; (7) historic development; (8) science of language and philology; (9) paleography; (10) conflict of languages; (11) grammar in relation to logic and psychology.

IV. Glossography, or the study of special languages: (*a*) languages not Semitic or Indo-germanic in Africa, America, Asia, Europe; (*b*) Semitic languages; (*c*) Indo-germanic languages: Indic, Iranic, Greek, Latin, and its derivatives, Keltic, Slavic, Lithuanic, Germanic, Scandinavic, English, Hollandic. Ontologic development of a language.

In 1888 R. de la Grasserie, of Rennes, published a work on the divisions of linguistic study (Divisions de la Linguistique, Maissonneuve et Ch. Leclerc (1888) Paris), in which he considers the subject under three heads: (1) The study of each language separately; (2) the comparison of related languages; (3) comparison of non-related languages. The author remarks that in botany or zoology, classification must be founded upon the knowledge of the essential characteristics of the beings under investigation, their true resemblances and their differences, and the degrees of importance among these last. The origin and relationship of each group must then be noticed. In this way a true synthesis of nature is the result. The following syllabus is followed by the author in a series of papers published in Techmer's Zeitschrift:

I. Classification of allied languages.

Title 1. Partial and subjective classification (in abstract): (A) Allied languages; ascending, fixed, descending; (B) related languages; related, allied, isolated; (C) allied languages, classed as if not related.

March of morphological evolution. (1) Transition between the three systems of expressions, more or less linguistic, of thought, (*a*) syntactic or the order of words, (*b*) employment of relational words, (*c*) of phonic modification. (2) Transition in each of the three systems between the concrete and the abstract. (3) Transition in each of three systems between the subjective and the objective. (4) Transition in each of the three systems between the non-formal and the formal.

Title 2: Classifications, objective and genealogic, of allied languages; natural families: (1) Indo-germanic; (2) Semitic; (3) Uralic; (4) Bantu; (5) Dravidian; (6) Malay-polynesian; (7) Turkic; (8) Algonkin; (9) Mandé; (10) Maya; (11) Hamitic.

II. Classification of languages not allied.

Title 1: Partial and subjective classification of unallied languages. Chapter 1. Purely phonetic classification. (A) From the point of view of the isolated word; (B) from the point of view of words united; (C) From the point of view of accent. Chapter 2. Classification purely psychological. Chapter 3. Classification morphological. Section 1. Languages with imperfect expression, psychological languages. 1. Concrete psychological languages. (A) Non-formulated. This characteristic exists in the relations, the determinations of ideas in the same proposition. (B) Formulated.

(*a*) Subjective. The concretness is thus graduated. (1) From the stand-point of necessity; (2) from the stand-point of comprehension; (3) from the stand-point of energy; (4) from the stand-point purely material, or purely intellectual, or both combined. (*b*) Objective: The concreteness is thus graduated. (1) From the stand-point of necessity; (2) from the stand-point of comprehension; (3) from the stand-point of energy; (4) from the stand-point of material or intellectual character, or both combined; (5) from the stand-point of application which is made of this concreteness in the principal ideas, or to those of determination or relation. 2. Abstract psychological languages; (A) analytical non-formulated languages; (B) analytical formulated languages. Section 2. Languages with sufficient expression, either morphological languages, or with unmeaning (vides) words. The languages with non-significant words are: (1) Formulated or non-formulated; (2) subjective or objective; (3) abstract or concrete; (4) invariable or with phonetic variations. Section 3. Languages with perfect expression, or languages with purely phonetic expressions. 1. Proceeding from the modification of a phonetic (phoneme) radical of the full word and its principal application to lexicology. Class 1. Languages with subjective phonetic mutation. Class 2. Languages with objective phonetic mutation. Genus 1: System of the Indo-Germanic languages: (*a*) Umlaut, (*b*) Ablaut, (*c*) phonic reduplication. Genus 2: Systems of Hamitic, Nubian, Celtic languages. Genus 3: System of Semitic languages. (A) Semitic system. (*a*) Use of the system for determination. (*b*) Use for relation. (*c*) Use for lexicology. (B) System of diverse languages. 2. Proceeding from the accord of a phoneme placed upon another radical. Class 1. Languages with subjective, phonetic accord. Group 1. Languages of the Bantu family. Group 2. Languages of the north Caucasus. Group 3. Indo-Germanic languages. Class 2. Languages with objective, phonetic accord. (A) Re-production upon the dominated word of the dominant word. (*a*) At the end of the dominated word; (*b*) at the beginning of the dominated word. (B) Re-production upon the dominated word of the end of the dominant word.

Title 2: Total classification, natural and objective, of non-allied languages.

VI.—TECHNOLOGY.

In Berlin, close to the Ethnological Museum, is the Kunstgewerbe Museum. It is difficult to say which of these is the more interesting. In the Ethnographic Museum the ruling concept is chorographic, but also ethnic. Each of the vast rooms is designed to cover a portion of the earth's surface which shuts in a recognized body of humanity and of human arts.

The Kunstgewerbe Museum contains much that is like the ethnographic collection, but the reigning concept is technographic. A trade, craft, art, profession is worked out ethnically, nationally, historically. That is, you are called upon to study the natural history of inventions.

In Oxford, at South Kensington, in Cluny, in Amsterdam, indeed in many European cities, the most interesting collections are thus arranged.

In the literature of anthropology, a great number of books, papers read before societies, and articles in periodicals, are devoted to the tracing out of separate industries. It is thus that in the National Museum Mr. Watkins traces the first wheel up to the latest paper car-wheel, or Captain Collins discloses the relation between the bull-boat of the Tigris or the Missouri and our last pontoon.

An extremely interesting example of technology coming to the aid of archæology is Mr. Edward B. Tylor's explanation of the mythical figure holding before the tree of life a cone-shaped object in Assyrian sculptures. This object resembling a fir-cone, the professor thinks, is the inflorescence of the male date palm as it appears when freed from its sheath ready to have its pollen dusted over the female flowers. (*Academy*, June 8.)

Indeed, there is no end to the arts that are being traced to their simplest forms by the technographic method. The following list of references to the bibliography of this paper will indicate the variety of such studies:

Anglo-Saxon industries, de Baye. Antler hatchets, Forrer. Architecture, Christian, Holtzinger. Art, Anthropometry and, Duhousset. Artists, Roman, of the middle ages, Frothingham. Art, The deformed and diseased in, Chavot. Boomerang, The, Eggers. Bow, Composite, Balfour. Bracelets, Serpent-head, from Persia, Polak. Bronze, Origin of, Buthelot. Caricature, Beauregard. Carrying industry, Beginnings of, Mason. Cattle known to ancient Polynesians, Tregear. Coinage in India, Smith. Copper mines of Mould-builders, Lewis. Cradles of the Aborigines, Mason. Crosses, Ancient, in France, Jadart. Debasement of Pueblo art, Holmes. Distribution of Monuments, Peet. Dog, The, Mortillet. Electrotechny, Brackett. Flint-working, Messikommer. Food of Nevada Indians, Witherspoon. Food of the Japanese, Kellner. Gold Breastplate, Peruvian, Galton. Gold, Gaulish, Cartailhac. Gold work in Peru, Galton. Hair-dressing, Feminine, archæologic, LeBlant. Habitation, History of, Garnier. Habitation, History of, Lavenue. Habitations of mankind, de Varigny. Houses of the Kwakiutl, Boas. Industries of Ireland, McCarthy. Keramic in Bohemia, Hoernes. Land measures, Round. Magic lantern (Schattenspiel), Turkish, Von Luschan. Manufacture of stone implements, Fowke. Manufacture of stone implements, Moorhead. Metal, Early age of, Spain, Siret. Metallurgy of copper. Mine, Ancient, in Arkansas, Chapman. Mining in America, Ancient, Appy. Mining in North America, Ancient, Newberry. Primitive money, Stearns. Money, Elk-teeth for, Balfour. Mound-building, Cherokee, Mooney. Naval archæology, Henrique. Nephrite and Jadeite, Bahnson. Nephrite and Jadeite, Clarke and Merrill. Pottery, Lustred, Mexican, Addis. Poisoned arrows in Melanesia. Precious stones of North America, Kuntz. Sculpture, Origin of Greek, Farnell. Shoemaker, Navajo, Stephen. South American culture objects in the Leipzig Museum, Reiss, etc. Specialized forms of stone implements, Brown. Tattooing, Queen Charlotte Islands, Boas. Tattooing, Variot. Textiles, prehistoric, Buschan. Terror in primitive art, Ferrie. Throwing sticks, Bahnson. Throwing knife of the Negroes, Schurtz. Time-indicators, Indian, Thompson. Tobacco, etymology of the word, Ernst. Transportation by human beings, Mason. Weights and Measures, Babylonian, Lehmann. Weights, Babylonian, Long. Weights, The oldest, Brugsch. Woman's share in culture, Mason.

VII.—ARCHÆOLOGY.

The pre-historic station of Lengyel, on the Danube, within the estate of Count Alexander Apponyi, in the comitat of Tolna, Hungary, is doubtless the most remarkable discovery of its kind in Europe during the year 1889. The station is a fortified enciente, within which have been found several groups of habitations and two cemeteries. Among the habitations, to the number of about two hundred, some were in form of a bee-hive dug in the loess of the Danube about three or four meters deep and two to three meters in diameter, and entered from the top. Alongside of these dwellings were other smaller souterrains, whose walls were formed of reeds and small branches interwoven and covered with a thick layer of clay, apparently hardened by fire. In these souterrains were many large jars similar to those found by Schliemann in Troy, and filled with different kinds of grain slightly parched. At Lengyel some of the habitations were above the soil. Their foundations are not more than a meter deep, and their walls were formed of wattling. But of the superstructure little can be said. One of the cemeteries belonged to the Neolithic period; the objects recovered in the other bring it into relationship with the palafittes and terramares, or the finds of the Villanova or first Hallstadian period. In the habitations and cemeteries over twelve thousand objects were found. Marquis de Nadaillac concludes that Lengyel belongs to the ancient Græco-Asiatic civilization, and that here we see traces of one of the immigrations which have exercised such a grand influence on the primitive populations of Europe.

The most original investigations into the Stone age made in the United States in 1889, were those of Mr. William H. Holmes, of the Bureau of Ethnology of the Smithsonian Institution, at Piney Branch, 1 mile north of Washington City, and those of Prof. F. W. Putnam in the Little Miami Valley, Ohio. The work of Mrs. Holmes is described in the January number of the *American Anthropologist* for 1890. The following résumé will convey some idea of the digging:

A hill slopes by a steep decline towards a running brook. Upon its sides is a dense growth of hard-wood timber and over its surface have been found for many years the rudely chipped stones called "turtlebacks."

In the autumn of 1889 Mr. Holmes carried a trench up the sides of the hill, going down to bed-rock all the way. At first his trench was only a few inches deep, but the depth increased to 9 feet about 50 feet up the hill-side. At every depth however the same rude examples were found as occurred on the surface, until suddenly the explorer came to a steep escarpment of bowlders in hard clay. The mystery was solved. Mr. Holmes had unearthed an aboriginal bowlder quarry, and the thousands of stones were the remnants of its occupation. Two questions are propounded by this discovery: the first is with reference to the antiquity of the workshop, the other to the function of the stone relics. Mr. Holmes inclines to the view that they are all refuse, the remains of unsuccessful attempts to make implements, and that the place is not very ancient. In this view he is not followed by all his colleagues.

In the latter part of September, 1889, Mr. Charles Francis Adams, president of the Union Pacific Railroad, announced the finding of a clay image during the boring of an artesian well at Nampa, Idaho, a station on the Oregon Short Line Railroad, 20 miles from Boisé City, about half way between Boisé City and Smoke River, being 7 miles from the former and 12 miles from the latter. This region is covered with deposits of lava rock belonging to late Tertiary or Quaternary times.

Beneath these lava deposits in California occur much gold-bearing gravel, and it was therein that Professor Whitney found the Calaveras skull.

The finding is thus described by Professor Wright: In boring the well, the surface soil was penetrated 60 feet to the lava rock, which was found to be 20 feet thick. Below this for 200 feet were alternate beds of quicksand and clay; then coarse sand was struck from which the image came up. Below this was vegetable soil and then sand rock. The image therefore lay buried to the depth of about 300 feet beneath deposits which had accumulated in a lake formed by some ancient obstruction of the Snake River Valley, and over this accumulation there had been an outflow of lava sufficient to cover the whole and seal it up. The image is carved out of soft pumice-stone and has a coating of red oxide of iron.

The subjoined list comprises the principal publications of the year in this department:

Ancient stone implements, India, Ball. Ancient village sites in the District of Columbia, Proudfit. Antiquities of Chili, Reed. Antiquities of man in America, Abbott. Archæology, Powell. Archæological glossary of the Middle age and of the Renaissance, Gay. Archæology in Europe, Cotteau. Archæology of Alabama, Holmes. Archæology of Canada, Boyle. Archæology of Finisterre, Du Chatelier. Archæology, Mexico, Seler. Archæology, Nicaragua, Bovallius. Archæology of North America, Haynes. Archæology of Ohio, Read. Archæology of France, Mas d'Azil, Dresch. Archæology of Servia, Kanitz. Archæology of Venezuela, Ernst. Bronzes discovered in Crete, Frothingham. Burial mounds, Thomas. Byzantine archæology, Diehl. Caches of flint implement, Smith. Chronology of the human period, Davis. Cup-stones in Perthshire, Gow. Egyptian archæology, Maspero. Egypt in time of Pharaohs, Loret. Emblematic mounds, Peet. Fort Ancient, Ohio, Moorehead. Geologic antecedents of man in Potomac Valley, McGee. Greek archæology, Smith. Guatemala sculptures, Bastian. Hallstatt in Austria, its civilization, Hoernes. Human remains in England.

Beddoe.. Human remains from Gourdan, France, Hamy. Ice age in North America, Wright. Celtic and Gaulish archæology, Bertrand. Lacustrian and palustrian villages, Castelfranco. Megalithic monuments, Gaillard. Mound explorations in Iowa, Harrison. Mound explorations, Iowa, Starr. Mounds in Missouri, Blankinship. Neolithic period in Charente, Chauvet. Nicaragua foot-prints, Peet. Oriental archæology, Clermont-Ganneau. Palæolithic man in America, McGee. Palæolithic period in the District of Columbia, Wilson. Pile structures in Venezuela, Forrer. Pleistocene Obsidian implement from the, McGee. Pottery of the Potomac tide-water, Holmes. Prehistoric archæology in Europe, Cotteau. Prehistoric France, Carthailac. Prehistoric man in America, Powell. Prehistoric Scandinavia, Undset. Prehistoric Sicily, Stillmann. Prehistoric station in Cochin, China, Holbe. The race of Lagoa Santa, Brazil, Hansen. Reindeer period in Vezere, etc., Girod. Relics from central New York, Beauchamp. Roman remains in Carniola, Haverfield. Rome, in the light of modern discoveries, Lanciani. Rude stone monuments east of Jordan, Conder. Rude stone monuments of Ireland, Bradley. Ruins in Cambodia, Fournireau. Russian archæological congress, Stieda. Shell mounds of the Potomac, Reynolds. Stone age in Italy, Castelfranco. Stone age in Sweden, Lanabee. Stonehenge, Evans. Stone monuments in Dakota, Lewis. Tertiary man, Arcelin. Viking age, Du Chaillu.

VIII.—SOCIOLOGY.

The firm hold which the methods of natural history have taken upon sociology is exhibited in the review of a threadbare subject with a reversal of public judgment. The efforts of Lord Kingsborough, Adair, and others to prove that the North American Indians were the lost tribes of Israel brought discredit upon their statements about the Indians. Colonel Mallery, as vice-president of the American Association in Toronto, reviewed the subject, re-affirmed the statements about both Indians and Israelites, and then proceeded to show that the similarities between the two peoples could be accounted for by a well-known principle in ethnology without assuming either consanguinity or contact.

The British Association for the Advancement of Science did an excellent thing in appointing a commission to study the Indians of Canada. Dr. Boas has reported extensively upon the social life of the coast Indians of British Columbia, a branch of ethnologic science for which he had specially qualified himself.

Mr. Stuart Culin, of Philadelphia, has utilized the presence of a large number of Chinese there to acquaint himself with some of their social customs. His studies in their apparatus of gambling have been prosecuted with extreme care, and the result is a series of monographs of great value.

The one absorbing topic among sociologists at present is the cause and prevention of crime. A congress of criminology was held in Paris during the exposition, at which gathered the most eminent students of the subject. The questions proposed to the congress were divided into three sections:

Section 1, *Criminal Biology.*—The latest discoveries in criminal anthropology, Cesar Lombroso and L. Tenchini. Anatomical characteristics of criminals, Dr. Manouvrier. Rules for anthropometric and psychological researches in prisons and insane asylums, Prof. Sciamanna and Virgil Rossi. Conditions determining crime and their relative value, E. Ferri. Infancy of criminals in relation to natural pre-disposition to crime, Romeo Taverni. Organs and functions of sense among criminals, Dr. L. Frigerio and Dr. Ottolenghi.

Section 2, *Criminal Sociology.*—Determination of the class of delinquents to which a criminal belongs, R. Girofalo. Conditional liberation, Dr. Semal. Criminality in relation to the ethnography, Dr. Alvarez Taladriz. Ancient and modern foundations of moral responsibility, M. Tarde. Criminal process from a sociologic point of view, G. A. Pugliese. Anthropology from the stand-point of its judical application to legislation and to questions of civil right, M. Fioretti. The cellular system from the stand-point of biology and criminal sociology, von Hammels.

Section 3, *Questions on which no reports or expositions were made.*—Atavism among criminals, Dr. Bordier. The place of this study in anthropology, Dr. Manouvrier. Instruction in medico-legal studies in the faculties of law, Prof. Lacassagne. Anthropometry and description of criminals from fifteen to twenty years of age, Alphonso Bertellon. How to make the instructions of criminal anthropology serviceable to the police, MM. Anfosso and Rometi. Correctional education, Dr. Motet. Moral and ruling perversions of children, Dr. Magnan. Mental degeneracy and simulation of idiocy, Dr. Paul Garnier. Influence of the professions on criminality, Henri Contagne. Degenerate and biological anomalies in women and girls, Drs. Belmondo and A. Marro. Vegetative functions in criminals and defective persons, Drs. Ottolenghi and Rivono. Causes and remedies of murder, MM. Barzilai and V. Rossi. Political applications of criminal sociology, Pierre Sarrante. Criminal anthropology in ancient Egyptian society, Ollivier Beauregard. Criminal anthropology in relation to sociology, A. de Bella. Moral and criminal responsibility of surd-mutes, M. Giampietro. Relation of criminal anthropology with legal medicine, Dr. Succarelli. Penal law, its effects and methods from the point of view of anthropology, Vittorio Ollivieri. Criminal sociology, Dr. Calajani. Contagion of murder, Dr. Aubry. Political assassins in history and in the present, Dr. Regis. The rôle of woman in the etiology of crime, Guiseppe d'Aguanno. Medico-psychological observations on Russian criminals, J. Orchanski.

Dr. A. B. Meyer, of Dresden, has rendered a generous service to the history of ceremony in his sumptuous quarto, number VII, of the publication of Konigliches ethnographisches Museum zu Dresden, upon the masks of New Guinea and the Bismarck Archipelago. The author refers to Andree's work (Arch. f. Anthrop. XVI, 1886, 477, and Ethnographische paralleln, N. F., 1889, 106); to Dall's paper (3d. Rep., Bur. Ethnol., 1884, 67), and to the Berlin Museum publication, Amerika's Nordwestkuste (1883, 1884). Fifteen plates accompany the text, done in heliotype process, the most excellent way of saving the peculiar grain of the material.

The leading publications of the year relating to sociology are as follows:

A history of the ancient working people, Ward. Accouchement among Clallam Indians, Bissell. Anthropophagy, Zabarowski. Bilqula, marriage, Boas. Brain of a matricide, Hotzen. Brain of an Amuck runner, Zuckerkandl. Brains of criminals, Fallot. Camping circles, Siouan order in, Dorsey. Castle-life in Middle Ages, Blashfield. Children's games, Dorselshue, Udal. Chinese chess, Volpicelli. Chinese games with dice, Culin. Chinese marriage customs, Fielde. Class system of Australians, Howitt. Consanguineous marriage, Oakley. Crime, Morris. Crime and accident in Edward First's time, Rye. Crime, its physiology and pathogeneses, Morris. Criminal anthropology, Belmondo. Criminal anthropology, Lombroso. Criminal characteristics, Hansen. Criminal characteristics, v. Holder. Criminal ethnography, crime in Creole countries, Corre. Criminal sociology, Colajanni. Criminality, Morrison. Criminality and occupation, Contagne. Criminals, Knecht. Criminals, classifications of, de Bella. Criminals, taste, hoariness, baldness, wrinkles of, compared with the normal, Ottolenghi. Deformation of the skull in Malecollo, Flower. Degeneracy and criminality, Fere. Degeneration in criminals, Kim. Delinquent classes, Ferri. Distribution of American totems, Wake. Domesday land measures, Pell. The ear of criminals, Gradenigo. Egyptian cosmetics, Virchow. Evolution of property, Letourneau. Gentes in camps, place of, Dorsey. Glossary of criminal anthropology, Rossi. Families, number of children, Chewin. Forms of crime, Field. Heirship of youngest, Kaffir, Nicholson. Holidays, Gale. Humanitarianism, Salt. Immigration and crime, Round. Israelite and Indian, Mallery. Jewish mortuary inscriptions, Block. Kinship in Polynesia, Starcke. Labor and life of the people, Booth. Marriage, Mnichovski. Marriage customs, New Britain group, Danks. Message-sticks in Australia, Howitt. Municipal government in Germany, Baxter. Mutilation, Ollivier-Beauregard, Naming children, Seely. Omaha mortuary customs, La Flesche. Population of Europe, primitive, Nadaillac. Parsee burial, Buckland. Partition of Africa, Debize. Pathogeny of vice, Lydston. Peasant life in Roumania, Sylva. Pedagogics, Bell. Penance, survival of, Howarth. Peons of Mexico, Croffut. Personal identification, Galton. Physical education in Russia, Pokrowski. Place of Gentes in Siouan camps, Dorsey. Playing cards, Chelon. Political power, its origin, a study of Aryan, Janvier. Polygamy in Turkistan, Capus. Precocious marriages, Rouvier. Prehistoric trepanation, Hansen. Primitive family, Starcke. Prisons, art in, Laurent. Protection and free-trade, Ward. Punishment, ethics of, Lilly. Running a muck, Malay, Hagen. Russian social life, Vogue. Salutation, Ling Roth. Slave, The history of a, Johnston. Slavers, Arab, American Exchange. Sociability and transformism, de Broglie. Social regulations in Melanesia, Codrington. Socialism, Rae. Suffrage and its mechanism, Blodgett. Tattooing, etc., Joest. Tenement-house life, Riis. Thief-talk, Wilde. American totems, Wake. Totem clans in the Old-Testament, Matthews. Totemism in Britain, Gomme. Town-life as a cause of degeneracy, Barron. Tribal boundary marks, Stephen. Village communities, Gomme. Widowhood in manorial law, Gomme. Woman among the south slaves, Schulenburg. Woman's place in nature, Allen. Woman's position among the early Christians, Donaldson. Women, types of American, Boyesen.

IX.—RELIGION AND FOLK-LORE.

The folk-lorists are just encountering a difficulty which has confronted the archæologists and technologists for a number of years. It is this: How are we to account for tales and myths and lore found in lands distant by thousands of miles and centuries of time, and yet so similar in dramatis personæ and incidents. Leaving out of view the nature theories of Müller, Cox, and de Gubernatis as at present unpopular, we have two extremely active candidates for our acceptance in the opinions of Lang and his colleagues on one side and Benfey on the other side. The views of Andrew Lang and of Mr. Tylor are that similar stimuli acting upon similar stages of culture and similar conditions produce similar results. The idea of Benfey is that many resemblances are too close to be accidental, and can be accounted for only by what Major Powell calls acculturation. The conflict is therefore fairly on, with the ablest of opponents on either side.

The first annual meeting of the American Folk-Lore Society was held in Philadelphia, November 28 and 29, in the halls of the University of Pennsylvania. Dr. Daniel G. Brinton presided and Mr. Horace Furness pronounced the address of welcome. A resolution was passed recommending a more extensive publication than the Journal of American Folk-Lore. The council was also instructed to provide a questionnaire or guide to the collection of Folk-Lore, to be circulated in pamphlet form. The meeting was made a very happy one by the courtesies of the authorities of the University and of the people of Philadelphia. The following papers were read:

Additional collection a pre-requisite to correct theory in Folk-Lore and Mythology, W. W. Newell. Chinese secret societies in the United States, Stewart Culin. Superstitions connected with human saliva, G. L. Kittridge. Some saliva charms, Mrs. Fanny D. Beyen. Primitive man in modern belief, Henry Phillips. Voodooism in Missouri, Miss Mary A. Owen. The Kootenay Indians, Rev. E. F. Wilson. Cherokee theory and practice of medicine, James Mooney. Folk-Lore of the bones, D. G. Brinton. Survivals of astrology, Munroe B. Snyder. Teutonic folk-names in America, Albert H. Smyth. Derivations of folk-tales, etc., in the United States, W. H. Babcock. Louisiana Folk-Lore stories, Alcee Fortier.

The bibliographic notices and references to sources of information in the Journal of American Folk-Lore, place the student immediately in relation with home and foreign literature upon this most popular branch of anthropology.

The first congress of folk-lorists was opened in Paris on the 29th July, at the Trocadero. The occasion of the exposition brought together French, Spaniards, Italians, Russians, Poles, Finns, Swedes, English, American, and Chinese. The officers of the congress were: President, Charles Ploix; vice-presidents, Bruyere, de Rialle, Leland, Dagomanor, Nutt, Prato, Nyerop, Tchengkitong; secretary, Sebillot. The subsequent meetings were at the Mairie of the sixth arrondissement, near St. Sulpice. The question of classification, tabulation, and analysis were referred to a committee.

The next congress will be held in London, 1891.

The Folk-Lore Society of London, the most active of all the organizations devoted to this branch of anthropology, held its annual meeting on Tuesday, November 26. The policy of the society has been carried out in two directions, (1) the systematic collection of the remnants of British Folk-Lore, and (2) the classification of general folk-lore in such a shape that the scientific value of each item may be tested and examined.

As the Folk-Lore Journal in its present shape did not sufficiently represent the scientific aims of the society, it was decided to issue the journal under a new title, Folk-lore. The Archæological Review will be fused into the new publication.

The prospectus gives a good analysis of Folk-lore as it is regarded by the English Society, and is here appended: (1) Original articles, whether collections of facts or expositions of theory. (2) Reprints of English material, not easily accessible, and translations of little read languages. (3) A record of the progress of study in folk-lore and in allied branches of science. This record will comprise: (*a*) A bibliography of English and non-English books relating to folk-lore, mythology, archaic and savage institutions, mediæval romantic literature, archaic history, etc. (*b*) Summaries of contents of folk-lore periodicals and citation of articles of interest to the folk-lorist in general periodicals. (*c*) Reports on well-defined sections of folk-lore, to be issued at stated times, briefly summing up the progress and results of study within each section during the interval from one report to another, each section to be intrusted to a member of the society, who will make himself responsible for the production of the report.

The following sections are planned:

Comparative mythology. Celtic and Teutonic myth and saga. Institutions; (*a*) archaic, (*b*) savage. Folk-lore in its more restricted use: (*a*) folk-tales and cognate subjects, (*b*) ballads and games, (*c*) folk-usages. Pre-historic anthropology and archaic history. Oriental and mediæval romantic literature. (4) Tabulation of folk-tales and analysis of customs and superstitions.

The impracticability of separating the study of comparative religion from folk-lore at present is seen in the titles given below, while in the common affairs of life no less than in the conduct of the gods the savage and the untutored mind live much in presence of a spirit world.

The most important of these publications are the following:

Amulets against evil eye, Tylor. Arab amulets, Pallary. Arab legend, Bolton. Aryan sun-myths, Morris. Ballads of London, Babcock. Bavarian folk-moot in sickness, Hofler (three papers). Blackfoot sun-dance, McLean. Bread-lore, Gregor. Budha's alms dish and the Holy Grail, Nutt. Celtic axes as amulets, Corot. Celtic myth, Nutt. Cherokee legends, Ten Kate. Cherokee plant-lore, Mooney. Comparative mythology, White. Cosmogony of the Mojave Indians, Bourke. Counting out rhymes, Indian, Matthews. Cross, svastika, etc., in America, Brinton. Death's messengers, Morris. Devil and witch stories, Gregor. Egyptian "Ka" (spiritual body), Edwards. English folk-tales in America, weather-lore, and current superstitions, Bergen and Newell. Fairy stories, Colardeau. Folk-lore, African (the story of creation), Clodd. Folk-lore of Bahama negroes, Edwards. Folk-lore, Burmese, St. John. Folk-lore, Cairene, Sayce. Folk-lore of Corea, Allen. Folk-lore, European, in the United States, Curtin. Folk-lore, German, White and Allen. Folk-lore, Huron, Hale. Folk-lore, Irish, White and Allen. Folk-lore, Magyar, Katona. Folk-lore, Mexican, Janvier. Folk-lore, New England, Currier. Folk-lore, New Hebrides, Codrington. Folk-lore, Ojibwa, Hoffman. Folk-lore, Omaha, Dorsey. Folk-lore, Omaha, Fletcher. Folk-lore, Oriental, White and Allen. Folk-lore, Pennsylvania Germans, Hoffman. Folk-lore, Scottish, White and Allen. Folk-lore, Scottish, Gregor. Folk-tales, Slavonic, Wratislau. Folk-lore, Teton, Dorsey. Folk-lore, Wexford, A. S. G. Folk-lore legends, White and Allen. Folk-lore, sub voce. Folk-medicine of Pennsylvania Germans, Hoffman. Gambling songs, Navajo, Matthews. Gezidees or devil worshippers, Brouski. Harvest customs, Frazer. House that Jack built, Brewster. Human sacrifices in Babylonia, Ward. Ireland, holiday customs in, Mooney. Irish proverbs, Kinnahan. Kelpie stories, Gregor. Lama pantheon, Pander. Legends of Annam and Tonkin. The lizard in the ethnology of Oceanica, Giglioli. Louisiana nursery tales, Fortier. Mohawk legend, Chamberlain. Masks, New Guinea, Meyer. Myths and effigy mounds, Peet. Myth of the robin red-breast, Fletcher. New fire among the Iroquois, Hewitt. Prehistory and Christian belief, Nadaillac. Priestly function among the lower races, Bastian. Plume sticks, Indian, Matthews. Popular superstitions, Berenger Feraud. Questions on customs, Frazer. Raven myth, Deans. Realism and naturalism in poetry and art, Lenoir. Religion of the Semites, Smith. Rhymes from old powder horns, Beauchamp. Sacred fire drill of Japan, Hough. Satyrs and giants, Petersen. Serpent ring in classical antiquity, Hoernes. Slavic moon myths, Krauss. Star names, Chinese, Edkins. Superstitions of Scottish fishermen, Gutherie. Swiss legends, Murray Annesley. Teutonic mythology, Rydberg. Thunder bird, Eells. Tonga superstitions, Roberts. Traditions of Winnebagoes, Martin. Viking age, du Chaillu. Voodoo-worship in Hayti, Newell. Winnebagoes, Traditions of, Martin.

Upon the endowment of the late Lord Gifford for a chair in each of the Scottish Universities for teaching natural theology, defined to be "the knowledge of God, the Infinite, the All, the first and only cause, the one and Sole Substance, the Sole Being, the Sole Reality, and the Sole Existence, the Knowledge of His Nature and Attributes, the Knowledge of the Relations which men and the whole universe bear to Him, the Knowledge of the Nature and Foundation of Ethics and Morals, and of all Obligations and Duties hence arising," Max Muller was elected to fill the chair in Glasgow for the first time. His lectures on Natural Religion upon this foundation are now published and form one of the important contributions of the year.

X.—MAN AND NATURE.

The study of the earth in its relation to man continues in two directions, the investigation of man's relation to geology and the accumulation of knowledge concerning climatology and the earthly forces effectual in human fecundity, longevity, vigor, health, etc. The most puzzling enigma of the year has been previously mentioned, the finding at Nampa, Idaho, of an image over 300 feet beneath the surface. There is just enough of uncertainty about this discovery to keep the matter forever in dispute. A much more solid foundation for argument is laid in the diggings of Mr. Holmes on Piney Branch, in the District of Columbia, where the ordinates of correct deduction were furnished by following the original horizontal stratum and by the perpendicular face of the bowlder bed.

A few titles are herewith appended to show the drift of investigation:

Acclimation at Panama, Vernial. Climate of tropical Africa, Virchow. Glacial period, Falsan. Man and nature, theories transformistes, de Quatrefages. The world's supply of fuel, McGee.

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THE LAST STEPS IN THE GENEALOGY OF MAN.*

By Dr. Paul Topinard.

Translated by Walter Hough.

Our science does not yet know the precise ways, direct or indirect, by which the present orders and families have advanced. The polyphyletic table of the genealogy of mammals that seems best to represent the present state of the inquiry, is far from having the ideal simplicity of the monophyletic tree of Hæckel. The genealogy of the celebrated professor of Jena is an admirable work, which has been the starting point for numerous studies that have rendered immense service. But he will himself acknowledge it to be a preliminary attempt, which he will certainly re-consider some day.

There are certain truths worthy to be remembered. The first is that our existing mammalian orders, families, and genera, are the product of a long evolution of successive transformations, and were not in existence before the eocene and miocene periods. At that time also according to the present teaching of paleontology, the first placental mammals began to be developed from the marsupials by means of differentiation and multiplication of types which have led to our present forms.

The second truth is that the progressive passage from the marsupial fauna of that time to the existing fauna did not take place by a single series of species for each order, family, or genus, but in all cases, where science has sufficient evidence, by multiple series anastomosing, intercrossing, and forming sometimes a perfectly inextricable network.

Here and there however, science seems already to have advanced;—for instance, in the case of the ungulata, whose genealogical table has been tolerably made out; the carnivora, whose numerous origins have been shown; the cheiroptera, and the pinnipeds or aquatic carnivora. Other orders resemble a veritable cross-roads, as the insectivora and rodentia.

For some orders we have recorded only the probabilities or provisional suppositions in regard to their derivation and development.

One important branch leading to man, in the doctrine of Hæckel, is that of the lemurs which follow the marsupials, the eighteenth stage from the moners in the genealogy of Hæckel.

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I.—LEMURS.

The lemurs have been classed among the quadrumana by Geoffroy Saint Hilaire, Cuvier, de Blainville, Duvernoy, and Milne Edwards, and among the primates by Linnæus, Lesson, Huxley, and Broca; that is to say, separated from man in the first case and re-united to him in the second. Vogt and Hæckel give them the name of pro-simians. The Germans call them half-apes (Halbaffen); the French sometimes the false-apes. The main question is, to what extent are they apes? Do they merit the name of pro-simians, and should they figure among the primates?

What do we understand by the primates? The best definition seems to be the following: The primates are non-aquatic, placental mammals (which excludes the cetacea, sirenia, and pinnipeds); they have no hoofs (which excludes the ungulata and proboscidea); they have three kinds of teeth (which sets aside the rodentia and edentata), and their molars are not in sharp and cutting ridges, or with sharp and conical points (which excludes the carnivora and insectivora).

But have they not certain characters in common? Not absolutely. Naturalists omit in their scheme of characters the type of cerebral convolutions. The primates have a discoidal placenta, a uterus with a cavity not two-horned, and the penis pendant.

Passing over the first two characters, the third is observed likewise in the cheiroptera or bats. The primates have two pectoral mammæ, but so have the cheiroptera and sirenia.

The teeth that everywhere furnish characters of the first order, vary as to number, form, and degree of continuity: all we are able to say here is that they are much more specialized, much closer together, and are above all, much more fixed in their general formula, as the families rise toward man.

Under the last head there are four stages: the lemurs, the monkeys of the old world, the monkeys of the new world, and man.

The nails, which among the primates take the place of claws, are one of their most important characteristics. So long as the claws are horny productions, compressed transversely, more or less long, recurved and sharp pointed, they serve as organs of attack and defense; and in the hoof the horny growth curves in on every side and envelops the digital extremity to hinder direct contact with the ground, and adapt it exclusively to walking. The nails are horny growths flattened above and below, growing straight and serving to facilitate prehension and touch. Their adaptation to that use is more or less perfect and applies more or less to the fingers of the primates; this allows us again to divide them into perfect primates, such as man and the monkeys (minus a certain group), and the imperfect primates.

The well developed thumb, separated from the other fingers and opposable, is a character of adaptation, the corollary of the nails. More completely, it is besides an organ for clamping, for seizing, and for touching; by this the primates may be sub-divided also, but into three groups, viz: Man, in which the thumb is opposable only in the upper extremities; the monkeys, in which it is opposable in all of the extremities, and the imperfect primates in which the adaptation may be either less apparent, or more marked, in the lower extremities than in the upper. Other characters could be pointed out, for the most part showing grades in the ascending series of the primates; but the above are sufficient for our purpose.

To so consider the primates is perhaps to prejudice in a measure the result sought. As soon as one introduces into the series a progressive development of characters, and divides the primates into superior, medium, and inferior, one is held to be indulgent toward the characters which appear to be obscure or lacking among the lowest. When we admit that the lower primates are but the commencement of the series, the passage from the other orders to that of the primates is but a step. Now the lemurs will supply us with the greater part of the imperfect primates to which we have alluded.

The lemurs embrace, or should embrace, three groups of animals: the galeopitheci, the cheiromys, and the lemurs properly so called.

The galeopitheci, or flying cats (from γαλὴ cat and πιθηκυς, monkey) inhabit the Sunda, Molucca, and Philippine islands. They exemplify the difficulty of fixing in our classification certain groups characterized as paradoxical, and for the reason that they are groups of transition, having the right really to be found in many groups. By Oken they have been classed with the rodents, by Étienne Geoffroy Saint Hilaire with the carnivora, by Cuvier with the bats, by Linnæus, Broca, Brehm, Huxley (in 1862), and Vogt, with the lemurs, and by Huxley (in 1872), with the insectivora.

That which permits of their being called lemurs is their general appearance and their arboreal and nocturnal habits. Most of their characters however oppose it. They have claws on all the fingers, and the thumb is not opposable, hence they are not primates, not even incipient. They possess that which Mr. Huxley calls a *patagium*, that is, a fold of skin on the sides of the body extending along the outside of the lower limbs and along the outside of the upper limbs, encircling the tail and prolonged between the fingers of normal length. It is the organ exhibited among the flying marsupials called petaurites, and which modified recalls the jurassic pterosaurians on the one hand, the cheiroptera and particularly the pteropus on the other, without agreeing among the last, however, with the wing of a bird.

This *patagium* has caused the galeopitheci to be classed with the cheiroptera. That which causes them to be placed among the insectivora by Professor Huxley is their dentition, the conformation of their skull, and their brain. In short, we discard them from the lemurs and consequently from the primates.

The cheiromys embraces but one genus, the aye-aye of Madagascar, It resembles a squirrel, but it also approaches the monkeys and maki. It has claws only on the upper limbs; the thumb is freely developed and is not opposable. In the lower limbs four of the fingers have claws; the well-developed thumb has a flat nail and is opposable. Its dentition is curious, making it a rodent as an adult, and an insectivore or lemur when an infant at its first dentition. Owen, de Blainville, Huxley, Brehm, and Vogt all place them among the lemurs. It is evidently a primate at its inception; but a species as if hesitating whether it should remain in the primates or in the rodents. The exposition of M. Vogt, on pages 13 and 77 of his "Mammalia," implies that its origin was in the insectivora.

The lemurs proper are divided into fossil and recent. The former appeared in the Eocene, and at that time existed parallel with the marsupials, which were then in the course of extinction, and the first placental mammals, which were the carnivores, the rodents, the ungulates and the insectivores. Europe has furnished five genera, America more, the most important being the anaptomorphus, from which Professor Cope derives man. The present species may be divided into three geographical groups. The first and the most numerous embraces the island of Madagascar, the second that island and Africa south of the Sahara. The third the island of Ceylon, the peninsula of Malacca, the Moluccas, and the Philippines. These regions constitute in the theory of Haeckel the remains of a vast austral continent, which he has called Lemuria. Among the genera belonging to the group of Madagascar I cite the makï, the indris, and the tarsius; in the second group, the galago, of which a species is found only however in Madagascar; and in the third or oriental group the loris.

The lemurs are arboreal and nocturnal animals, as previously said. Oken calls them the nocturnal monkeys of the Old World. They have four opposable thumbs with a single exception, the tarsier, which does not have the upper thumbs opposable, but only the lower ones. All their fingers, as a general rule, have nails, save the posterior index, which is armed with a claw, or the anterior little finger of the loris. However, the nails are sometimes rudimentary and as though developing from the claw. Relative to the teeth it is impossible to establish a general formula. The number varies from thirty to thirty-six. For example, the formula of thirty-two has been given to man and the catarrhine apes; the indri has thirty, because it lacks an upper premolar; the tarsier thirty-four, because it has a lower incisor less and for each jaw a premolar more; the maki thirty-six, because it has a lower incisor and an upper premolar extra; the Loris also thirty-six, because it has an incisor and a lower premolar more. All these considerations tend to establish that the lemurs have not a fixed and homogeneous type, but that they constitute a transitional group from animals with claws to animals with nails, and should consequently be regarded as the first, if not the second, step (considering cheiromys as the first) in the line of the better-characterized monkeys. However, serious objections are raised against that way of looking at it. The first is that of Broca. In 1870, when his celebrated monograph on the order of primates appeared, my lamented master (Broca) maintained in nearly the same terms as Huxley that the lemurs are the fifth family of the order of primates, but more separated from the other families than any one of those is from one another.

In 1877, after a communication before the Société d'Anthropologie, he changed his opinion, and for the following reason: Haeckel based his bifurcate division of placental mammals on the existence of extended or limited placenta and the absence or presence of a deciduous membrane which detaches with the débris of the egg at the time of birth. Among the mammals with diffuse and deciduous placenta are classed the ungulata and cetacea. The others are sub-divided in their turn into four branches, in which the circumscribed placenta presents itself under different aspects reducible to two, one an annular or zone-like insertion, the other a disk or discoidal insertion. The carnivora and proboscidea are examples of the first mode of insertion. Man, the anthropoids, the ordinary monkeys, and the lemurs,—that is to say, all the primates are in the second class. Now Broca had shown to the society the placenta of a lemur, the *propithecus diadema*, in which the placenta was neither discoidal nor annular, but is diffuse, and which had no decidua. The lemurs hence are separated violently from the other primates by a character of the first order.

Vogt answered that we have as yet examined but four specimens of lemurian placentas, and that this organ among them is neither diffuse nor zonary, nor discoidal, but bell-shaped, a transition from the zonary to the discoidal. Afterwards, without denying the importance of the placenta as a basis of classification for the mammals, he showed that its importance had been exaggerated, that all the intermediate ones fall in between the different forms, and that very different shapes may frequently be observed in the same order. Vogt accepts however the opinion of Broca, but it was on account of other considerations. According to him the lemurs are to be separated from the monkeys, and consequently are not their ancestors. He remarked that the opposable thumb has nothing absolute about it since it has been already observed among certain marsupials, and likewise the nails, since the lemurs have claws on more or fewer fingers. That is true, but Vogt retains the galeopitheci among the lemurs, and they are the most important feature in his argument. As to the contradictory physical characters invoked by Vogt they are numerous and weighty. To enumerate: —the lemurs have the two parts of the jaw independent, but they are always joined among the primates; their low and slim jaw contrasts with the high and heavy jaw of the monkeys. The intermaxillary bone persists throughout life among the lemurs but it is co ossified early among the recognized primates. The orbits are opened behind or have but a slender ring, whilst they are always closed among the primates. The lachrymal bone is largely exterior or facial, whilst among the primates it is intra-orbital. Their dental types are various, whilst it is fixed among the monkeys. The cerebellum is uncovered among the lemurs, and covered over among the primates. The uterus is bifid, contrary to the assertion of Hæckel. Beside the pectoral mammæ, they have often inguinal mammæ. They have never been observed to have breech callosities or cheek pouches as among the monkeys of the Old World. The pelvis and the ear are entirely different.

Vogt concludes in these words: "In summing up, it follows from all these facts that absolutely no relation exists between the prosimians and the monkeys, and from the same, none with man. With the exception of the opposable thumb, which is found among the marsupials, the lowest and most ancient of the mammals, the prosimians have not a single anatomical character in common with the monkeys. It is derogatory to all principles of positive science to class the prosimians among the probable ancestors of the human species."

Are these objections really so weighty? From a morphological point of view, they are certainly important; but they do not oblige us to throw out the lemurs from the order of the primates. None of these divergent characters are in contradiction to the idea that they are but the rough draft of a beginning of the primates.

The characters drawn from the nails and the opposable thumbs outranked the others at the time in determining the general idea involved in the choice of the word primate. But man has the orbit open, or closed, the angles of the uterus are prolonged more or less, the intermaxillary and symphisial sutures may or may not be united, he is not the less man. The same is true for the monkeys. The adaptation of the extremities,two, or four to the function of prehension, is the characteristic trait of the primates. But is the inconvenience of admitting the lemurs into the order of primates of moment when it is made in the terms of Huxley? The lemurs are the last family of the order of primates and are more remote from the other families than they are from each other. The distance from the anthropoids to man is also very great, as shown in the volume of the brain and the cranial characters flowing from it, and nevertheless I range man among the primates. Strictly, they can separate the lemurs and make a special order, so that the genealogical attachment to the monkeys will not be so prejudicial, but that will compel us to do the same with man. Vogt is inconsistent; he retains the word pro-simians as synonymous with lemurs.

Having finished with the links which do or do not attach the lemurs to the primates, it remains to speak of their relations with the other neighboring groups. I have sufficiently insisted on the relationship with the marsupials and more particularly with the phalangers. The insectivores are next to be considered.

All authors from Cuvier to M. Vogt have noted the resemblance of the teeth of the lemurs to those of the insectivores. "Their teeth," writes Cuvier, "begin to show us (from higher to lower) the sharp tubercles interlocking one into the other as in the insectivora." "The galegos," one finds a little further on, "have the teeth and the insectivorous diet of the other lemurs." "The dentition of the tarsians is that of an insectivore," says Mr. Vogt. "The lobes of the molars are usually well forward as among the insectivora," says M. Huxley. We have already pointed out the insectivorous first dentition of the cheiromys. Gratiolet, going farther, classed the lemurs in the insectivora. The origin of the insectivora besides, is by no means irreconcilable with that of the marsupials. The primitive type of these was the insectivore of the trassic and jurassic periods. The phalangers are an existing species. We must seek in the fossil species the true origin of the lemurs, since these appeared in the eocene or beyond. The last relation to point out is that with the ungulates, according to the eminent professor of the Museum, M. Albert Gaudry, whose work on the placoid and ganoid fishes and the amphibious labyrinthodonts deserves attention. "I have asked myself," said he, in his *Tertiary Fossils*, "if the lemurs had not a common origin with many of the extinct pachyderms." The resemblances between the present lemurs and the ungulates, proven by Alphonse Milne Edwards and Grandidier in their great work on Madagascar, leads to that belief.

Two genera bear out this idea. The first is the genus adapis, of which a Parisian species, coming from the gypsum beds of the upper eocene of Montmartre, has been classed by Cuvier among the pachyderms; but it is found, judging from the teeth, the skull, and some parts of the limbs to be but a lemur. The second is the aphelotherium, classed by Gervais, likewise with the pachyderms and at present recognized as a lemur. The resemblance holds good with the eocene species of the stock of the present perissodactyls, such as the hyracotherium, the lophiotherium and the pachynolopus.

In the United States, Professor Cope has discovered many species of adapis and confirmed these resemblances. It is always well to remark that the genealogy leading up to man is outside of the question. Mr. Cope divides the fossil lemurs of America into three families; the anaptomorphus, which leads up by two branches, one to the monkeys and the other to man, the mixodectins, the limits of which I am not able to state, and the adapides, which lead to the ungulates. The branch of adapis is therefore, according to Cope, foreign to the branch leading to man.

II.—MONKEYS.

The more I study this question, the more I am convinced that the anthropoids should be re-united to the accepted monkeys, of which they are only a higher family; I am more persuaded that they are more separated from man, as I do not yield to the belief of a certain school in taking a purely morphologic point of view. As to the physiological, or rather the intellectual point of view, it is not to be discussed for a moment.

The principal classifications of the primates are as follows: Cuvier: Two groups—man and the monkeys—the latter, under the name of quadrumana, divided into monkeys, makis, and oustitis, the first group embracing those which are called the great monkeys or anthropoids.*

Broca's latest way, which is but a variation of Linnæus's two groups: Man and the united anthropoids; the monkeys, those of the Old World, or pitheci, and those of the New World, or cebians.

Huxley's last way: Three groups, man, the monkeys, and the lemurs. The monkeys are divided into catarrhine, platyrrhine, and arctopithecine. The catarrhines are subdivided into anthropomorphs and cynomorphs.

Vogt, in his work entitled "Mammals:" First group, man, which we place here for sake of completeness, but who is not treated of; second group, the monkeys of the Old World, divided into the anthropomorphs without tails and monkeys with tails; third group, the monkeys of the New World, divided into platyrrhines and arctopitheci; fourth group, the lemurs or pro-simians.

It follows therefore (with but the exception of Broca) that all agree in uniting the great monkeys or anthropoids to the common monkeys under the term monkeys or catarrhine monkeys, or monkeys of the Old World, and that Huxley and Vogt (whom no one would suspect of revolutionary theories, I was on the point of saying,) think as Cuvier. Is Broca as isolated as I have affirmed? I mention here that Broca never formulated his division as have the foregoing, but that it is the incontestable result of his teaching here, and especially of that of his last years. This fact seemed so apparent that I was compelled to express it in a table in my *Elements of General Anthropology*, appearing in 1885, to make evident the resemblance of his classification to that of Linnæus. Hervé and Hovelacque, who were in possession of notes taken at the course of Broca, so understood it and have re-produced it with some additions to complete it in their "Summary of Anthropology" (*Précis d'Anthropologie*), appearing in 1887. Would Broca have put it into a table rashly, as Hervé and Hovelacque and I have done, specifying that he treated only of physical man? I can not say. One phrase of his memoir of 1870, on the order of primates (page 83), where he qualifies the uniting of man and the anthropoids in the same group as extravagant, bears out this idea. I imagine he would have said, "Certainly this table is correct, but it is only one aspect of the question."

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* Cuvier divided the quadrumana into three groups: The monkeys or quadrumana which have four straight incisors in each jaw and flat nails (nails properly so called) on all the fingers; the makis or quadrumana, which have in either jaw incisors in number other than four or of other shape and the nails flat on all the fingers except the little finger, armed with a pointed and turned-up nail (a claw), and the ouistitis or doubtful quadrumana, though he ranges them in the first group. The makis are our lemurs.

However that may be, the classification that I attribute, right or wrong, to Broca is held to be his by many people, and against it I would protest.

From my special studies and my knowledge of the differences (great and little) from monkeys presented by man, drawn from the volume of the brain, the cranial characters that are the consequence of it, the facial characters that accompany them, and the characters of the skeleton that are developed parallel with them,—that is to say, of all the characters that I have specially studied, I am compelled to abandon the classification of Linnæus, and to adopt the abused one of Cuvier, in which, besides, critics never have seriously reproached anything but the employment of the word quadrumana and the exact definition of the hand on which he based it. Cuvier may not have been very much of a philosopher, but he was the first of observers.

Let us consider for a moment the word quadrumana. When Broca opposed the term quadrumana as applied to the monkeys to distinguish them from two handed man he set forth the fact that the presence or absence of the thumb was not enough to authorize the name hand or foot, but in man the upper limbs accorded with the function of prehension, to which the extremity of the limb is the immediate organ, but the lower limbs are likewise constituted in view of the function of locomotion and support, which its extremity seems intended to supply. In a word, there is harmony between every part everywhere, of which the different details constitute the characteristics of the function, hand and foot. This is extremely true, but with man only, who occupies the summit of the evolutionary series. It is far from the same when we descend the course of the series.

Among the monkeys, the anterior limbs are still adapted to the function of prehension, but they are at the same time organs of locomotion; the posterior limbs are still adapted for walking, but they are at the same time organs of prehension. Among the lemurs, are still the same general types of all the members for prehension and progression, but in fact the anterior extremity is more a paw and the posterior more a hand by comparison; as for example in the cheiromys. The monkeys are both quadrupeds and quadrumana. Notice the three chief segments of each limb: forward it is an arm, but backward it is a true leg; however, look only at the last segment both before and behind; it is a hand by the principal characters of the free and opposable thumb and the nails.

In man the harmony is perfect because the functions are specialized and because the organs are all adapted in the same way, those forward for prehension and those rearward for walking.

Beyond our branch of primates then, where its origin is seen, the fore limbs appear with the same types but less definite, less precise, all four for prehension, the forward ones more; all four for locomotion, the hinder ones more. Following the marsupials, evolution commences, specializations take place in different directions.

Among the galeopitheci and the cheiroptera, the particular adaptation works in the way of flight, one part or the whole limb is not only transformed, but bends itself to the needs and obeys the calls upon it.

Among the ungulates, the adaptation works in the way of locomotion by the four limbs, exclusively; gradually these mold themselves on the same type, the useless bones disappear, or are fused together, certain superfluous movements cease in the ratio that others increase, including the necessary corresponding anatomical arrangements. Here Broca ought to have taken his model type of the locomotive limb, as among man he possessed the model type of the prehensile limb.

Among the carnivora, that have to bound over the earth to catch their prey while at the same time they must be able to seize, hold, and rend it, the fore paws have remained perfect locomotive organs, but at the same time, organs of attack by their claws, and organs of prehension to a certain extent,—particularly in the anterior extremities.

Among the monkeys an adaptation of another kind has taken place. Those from whom they descend lived in the trees, ran on the branches; they had need of increasing their power of prehension; they had to clasp the rounded trunks of trees and catch the branches in passing from one to the other. The adaptation appears to show in the posterior members first; later in the anterior ones. The make-up of the limb has not had to lose its own type on that account; it is enough that the extremities are adapted in a certain way. The nails, the free opposable thumb, the very movable fingers, are enough; nature is contented with that without mounting to the higher segment.

One fine day a revolution takes place. In the same way as an adaptation to an arboreal life has taken place at the expense of other prior species, so an adaptation to terrestrial life occurred with an upright attitude, favorable to a more extended vision, a diminution of the olfactory sense and the facial prominence over which it presides, a perfecting of touch, and above all intelligence. From that time all the living forces of adaptation have tended towards the same end, the lower thumb has ceased to be opposable, the other toes have decreased in length, what the foot loses the hand gains; man was created exclusively two handed above, exclusively two footed below, all the accessory parts in the segments of the limbs agreeing with the types, which had existed since the marsupials but less accentuated until then.

The little character of the opposable thumb brought out by Cuvier, marks then perfectly that which is common and special among the monkeys, the ability of clasping limbs of trees by the four extremities. Without doubt he expressed but one of the particulars of that make-up so perfect in man, who has given birth to the words hand and *foot*, but it is an essential one. One does not know enough to deny however that the second character necessary to the function of prehension, that is the great mobility in every way of the segments of the limb may not be well developed among the monkeys in the lower limbs. Cuvier then had a perfect right to call all monkeys quadrumana, although they were at the same time quadrupeds, and to oppose them to man.

I unite then the anthropoids and the ordinary monkeys under the name of monkeys, and I should not recoil before the synonym of quadrumana, if that of monkeys does not satisfy me.

The monkeys are divided into two groups, those of the Old World called catarrhines, because they have the nasal septum narrow and the nostrils opening below the nose (from κατα, below and ριν, nose), and those of the New World called also platyrrhines, because they have the septum of the nose wide, and the nostrils opening on the side (from πλατυς, flat). We will commence with the latter.

The monkeys of the New World are entirely arboreal; they are divided into two families; the monkeys properly so called, and the arctopitheci.

The first are divided into diurnal monkeys, embracing the howlers, the ateles, the sajous, etc., and the nocturnal monkeys, embracing the sagouins, the sakis, the nyctipitheci, and the saimiris.

The second family requires particular notice. The arctopitheci or hapalians are a separate group among the monkeys of which I have spoken, from two considerations. They embrace the ouistiti (a charming little monkey made illustrious by one of our novelists), and the tamarin. They are arboreal as the preceding ones, and nocturnal like the latter.

The arctopitheci are an example of the imperfection of our means of classification. They are monkeys and are like the monkeys of America in most of their affinities, but they lack the single character which distinguishes all monkeys including the lemurs, and they have neither the dentition of the monkeys of America nor of the Old World. We cut out the galeopitheci from the lemurs by the absence of the first character; is it necessary to treat the arctopitheci the same way with regard to the monkeys?

Here are their characters. When one seizes the skull in a way to hide the lower part of the face, it is entirely an American monkey. Like the monkeys of America it has a round head, a flat face, lateral nostrils, and no rump callosities or cheek pouches. But it does not have opposable thumbs on any of the limbs, which leaves out the only character common to all the monkeys and false monkeys. Furthermore, they have claws on all the fingers except the hinder thumb (the hallux) which has a nail. The teeth number thirty-two, that is to say, the count of the monkeys of the Old World and man, but with a different formula; a small molar more and a large molar less. Furthermore, their teeth have certain insectivorous characters; the lower canine is small; their molars interlock a little as those of the insectivora, and the front ones have sharp, conical points. The lower incisors of certain species are pointed.

Cuvier hesitated to make them quadrumana. For our part we should readily see here an introduction to the Primates, a kind of American lemur, a transition from the insectivora to the monkeys of the New World.

Fossil monkeys have been found in America. A most remarkable thing is that all have thirty-six teeth, and agree with the types of that continent, as if the platyrrhine monkeys had always lived there. The highest among them is the laopithecus, which one should compare with the anthropoids of our continent.

In short, one is led in America to a special series so constituted by its origin and its termination, viz: many insectivora, arctopitheci, nocturnal monkeys beginning with the saimiris, diurnal monkeys, and the laopitheca. Vogt, Schmidt, and Cope, have agreed on this insectivorous descent.

The monkeys of the Old World are less arboreal than those of the New World, and are entirely diurnal. Most of them have rump callosities and cheek pouches. Their teeth are in general less omnivorous than those of man and tend by the canines to the carnivorous type; they are also farther apart. They are divided into the great monkeys, monkeys without tails, or the anthropoids, and monkeys with tails, which are divided into semnopitheci, cercopitheci, and cynocephali. The semnopitheci (from σεμνος, venerable) embrace the entelle, which has received that name because it is sacred in India, and plays a part in Aryan legends. It inhabits India, Indio-China, Borneo, and Java. The colobe of Abyssinia and Guinea, completes the list. The cercopitheci include the guenon, which is found only in Africa, the magot, which inhabits Africa and appears even on the Rock of Gibraltar, and the macaque, which has been observed at two points in Asia,—India, and Japan. As for the cynocephali, they are the large dog-muzzled monkeys of numerous species which inhabit almost all of Africa.

The monkeys of the Old World are related on the one hand to the lemurs, and on the other to the ungulates.

The first relationship is openly maintained by Hæckel, and by Cope. Hæckel rests entirely on the shape of their placenta, not a very convincing proof. Mr. Cope depends chiefly on the conformation of the teeth, which is a more solid argument. Huxley does not say that the monkeys descended from the lemurs, but his description leads us in that direction. Vogt rejects that genealogy, as we have seen; Schmidt does the same.

The second relationship (that with the ungulates) is entertained by Gaudry, and is the consequence of the one which he has established between the lemurs and the ungulates. There we had two genera, the adapis and the aphelotherium, that establish the communication, the point of junction being at the eocene origin of the perissodactyl branch of the ungulates. Here we have as yet but one known genus, the oreopithecus of Gervais, which by its dentition resembles the choeropotamus, belonging to the artiodactyl branch of the ungulates. In review we have genera of ungulates, belonging to the same stock as the suides or very close to it, which have marked resemblances to the monkeys; they are the cebochoerus (or pig monkey) of Gervais, the acotherulum and the hyracotherium of Owen. It is worthy of remark here that the ungulates, going on the one hand to the lemurs and on the other to the monkeys, are all eocene, whilst the only real monkey leading to the ungulates is miocene. It is also worthy of remark that in his general proof of the relation of the preceding species with the ungulates, Gaudry did not separate the lemurs from the monkeys, as if from a paleontological stand-point; in the ancient species the two were tangled together.

Assuredly this is a slender basis upon which to establish the derivation of monkeys and ulteriorly of man from the ungulates. For all that, the hypothesis has made some stir. Vogt seems disposed to accept it, and Schmidt concludes that chapter in his book with these words: "The monkeys have distinctly a double origin; the American branch has had ancestors in the form of insectivores, the Euro-Asiatic branch, including the anthropomorphs, ancestors in the form of pachyderms. We are thus brought very close to the question of the pachydermal origin of our primitive ancestors."

Observe that the catarrhine monkeys are dispossessed of their affiliation with the lemurs. I declare that I can not bring myself to accept the idea. The lemurs are, according to my belief, the lowest of the primates, of the quadrumana, and as such, those which bear every probability of having produced the others.

I will indulge in a single reflection. I am an anatomist, a craniologist, and it is far from me to throw any doubt on the great value of the smallest morphologic character; but I ask myself if really, underneath the particulars which may show the conformation of the teeth, the fingers, and the toes of the tarsus and carpus, back of the characters that reflect the precise kinds of alimentation and the precise way of locomotion, there is not something more general answering to the special habits, to the course of life or habitat more or less terrestrial, aquatic, diurnal or nocturnal, that imprints on the make-up of the organism that general appearance of relationship that the naturalist perceives over and above all those special modes of adaptation that he studies with so great care to find a testimony, an expression, a formula for the support of his thought,—of his vision, if I may so express myself. Clearly a particular trait, a progressive variation of form, reflects the higher kind of influence to which I allude. The teeth, the condyle of the jaw and its articular cavity, the temporal fossæ give very exactly the diet of the animal and consequently certain of its habits. The *patagium* of which we have seen the first traces among the marsupial *petaurites* allows us to establish a series leading to the bats by way of the galeopitheci. I have shown you that the genealogy of the perissodactyls, one of the most satisfactory that science has yet established, rests essentially on a single character, the number and degree of atropy of the fingers or toes.

Is the form of this chosen characteristic, all? Has not nature different ways of attaining the same end, and cannot she divide her influence over the make-up of the organism without making any characters particularly distinctive, and even at the same time leaving present characters in appearance contradictory? Mice are known entirely by their way of progression, head, and general form; nevertheless, they are found under different names among the aplacentals and the placentals, among the rodents and among the insectivores, terrestrial, semi-aquatic, semi-flying, or flying altogether. It is the same with the genus squirrel; they are scattered in many orders under names simply modified in certain particulars. There is among the marsupials a type of remarkable leaping animals, which, while entirely preserving that type, are dispersed in different placentary orders, because they have acquired new characters.

I ask then, if the peculiar ways of the monkeys, if their habitat, which is exclusively arboreal among their better defined representatives, and which impresses a stamp on their entire individuality, the proportions of the body, the extent and situation of the articulating surfaces, the freeness of movement by means of segments one over the other, is not a sufficient incitement to establish their relationship to the lemurs and not at all with the ungulates. In the same way as the lemurs, which live a similar life, lead to certain marsupials, so these also dwell constantly in the trees. Between the ungulates and the monkeys I see nothing in common. I can not understand an animal with hoofs walking on the end of the digital extremity alone, having the metatarsals co-ossified, drawn out and raised, the fore limbs drawn close to the body and moving almost in the same parallel plane; that is to say, adapted to a measured and rythmic terrestrial locomotion, giving birth to a plantigrade animal with nails, with movable fingers made so by being molded upon the trees in grasping the branches, with limbs endowed with the most dissimilar movements of abduction and adduction; whereas it does not require any effort of imagination to conceive an adaptation already commenced in that way among the lemurs and having but to be continued and more specialized among the monkeys.

Before starting on the relationship of the monkeys of the Old World with man we must look into another question. We have verified an intrinsic ascending series; do we find a similar one among the monkeys of the New World?

Two stages of evolution appear at the start, one that relates to the tailed or ordinary monkeys, and the other which takes in the four catarrhine monkeys without tails or anthropoids. The latter show two degrees, the one for the gorilla, the chimpanzee, and the orang; the other for the gibbon, which is the transition shown between them and the tailless monkeys, more particularly the semnopitheci. With the four it is necessary to class two fossil anthropoids, the *Pliopithecus antiquus*, noted in 1837, by E. Lartet in the miocene of Sansan (Gers), an animal probably near to the gibbon, and the *Dryopithecus fontani*, found by Fontan in the miocene of St. Gandens (Haute Garonne), which is incontestably an anthropoid, but different from the present anthropoids. I have not included the laopithecus, an American monkey, which would be the third fossil anthropoid known. One can also give as a proof of evolution in the monkey group the *Mesopithecus pentilici* of which Gaudry has unearthed the fragments of twenty-five individuals in the miocene of Pikermi, Greece. It does not belong in any of the present genera, but approaches in its skull the semnopitheci, and the macaque in its limbs. One can believe then that it is an ancestor of both by a kind of doubling of type like that which was produced in a large number of marsupial types.

Vogt, in spite of himself, gives an argument in favor of this internal evolution. In the arboreal life of monkeys there is gradation; the monkeys of America and the semnopitheci never leave the trees; the magots often set foot on earth and would be semi-arboreal; the macaques and cynocephali are terrestrial. Now is it not permissible to believe, seeing their perfect adaptation to life in the trees that the magots and with much stronger reason—the macaques and cynocephali correspond with an original effort in a new line, a way which continued, we can conceive would cause them to grow straight or to have an intermittently oblique attitude, and thus be helped to new adaptations.

Finally, Gratiolet, at a period when he could scarcely have thought of the doctrine of evolution which was about to spread over the world, and which at all events would have been repugnant to his religious sentiments, put forth the idea of parallel series among the monkeys of our continent; for example, the semnopitheci, proper to southern Asia and the neighboring islands, leading to the gibbon and orang in the same region, particularly in the southeast; of the macaque and magot leading to the chimpanzee; and above all of the cynocephalus leading to the gorilla. Unconsciously Gratiolet prepared the doctrine of the derivation of man from the monkey, siding with the polygenistic ideas then in favor in the school of anti-orthodoxy.

This now leads us to our last genealogical stage, to the passage from the monkey to man.

III.—MAN.

I will set forth on this point the principal opinions that are current, or which can be maintained.

The first is that of the learned professor of Jena, Hæckel. He is monogenistic as to man, as he is monophyletic concerning each of the branches and branchlets of his genealogical tree. The tailless monkeys of the Old World constitute his nineteenth stage above the monera. He divides them into four branches. The fourth is the anthropoids, divided into two branches, an African and an Asiatic; the latter he divides into three. The third division gives us the pithecanthropus or man-monkey, which already holds itself upright, but which lacks speech; this is the twenty-first stage, the anthropopithecus of M. de Mortillet, out of which present man is derived by two branches, the twenty-second and last step of Hæckel, one the negroes with wooly hair, and the other the races with straight hair, of which the Australian would be the prototype. On the chart of the world that Hæckel gives, the place where man would have taken rise by the acquisition of articulate language is put at the the southwest of India, where the center of the continent of Lemuria of which we have spoken would have been. The place is marked *Paradise*; it is the starting point from which man should have spread in all directions, some to the west towards Africa, others to the east towards Australasia and Melanesia, and others to the north towards Europe, Asia, and by Bering's Strait into America.

Huxley does not express his opinion on the immediate descent of man in any of his writings that I have read; he lets the reader draw the conclusions from the developments into which he enters, and these lead to an origin from the anthropoids.

Our eminent palæontologist of the Museum, Professor Gaudry, is also very reserved; nevertheless he will allow us to surmise his opinion where he has not plainly formulated it. On our authority, the following series expresses his entire thought concerning the mammals: Marsupials, ungulates, lemurs, and catarrhines forming a single group, anthropoids, and man. The anthropoid that he points out is the *dryopithecus*. Here is what he says: "The dryopithecus was a monkey of a high order; it resembled man in many particulars; its height must have been nearly the same; in its dentition it recalls the characters of the teeth of the Australian." (*Fossil Primates*, page 236.) Further on he adds: "If then it comes to be proven that the chalk flints of Beauce, discovered at Thenay by the Abbe Bourgeois, have been dressed, the most natural idea that presents itself to my mind would be that they have been worked by the dryopithecus (page 241). Unfortunately we possess but a lower jaw and a humerus of this animal."

Another palæontologist, the American Professor, Cope, has an opinion of his own. Man did not descend from monkeys, anthropoids, or the rest; he descended directly from the lemurs. We have already said that the condylarthri, the original stock of almost all the orders of mammals, gave birth notably to a branch that was divided into three; one was principally represented by the genus anaptomorphus, and was divided in its turn into two twigs, one of which produced the monkeys and anthropoids, and the other which lead directly to man. Here are his principal reasons. They show us on what slender basis our genealogies sometimes rest.

Man has, as a general rule, four tubercles or cusps on the upper molars. The monkeys and the anthropoids have in general five tubercles. The present lemurs, the fossil necrolemur, and the anaphtomorphus have in general three tubercles. But in man three tubercles have been noticed; Cope has published a long list of their degrees of frequency among the races. It is a reversion towards the lemurs, and not towards the monkeys and anthropoids.

The present opinion of Vogt is radically different; but as the learned professor of Geneva has held at different times opinions almost diametrically opposed and has played an important part in the question, we will stop longer with him. His first way of looking at it was formulated in his course of 1862–'64, before Darwin had formally applied to man his doctrine of the derivation of species one from the other by the mechanism of selection, and before Hæckel had completed his course of 1867–'68, in which he showed for the first time his complete genealogical tree. His second opinion is known to me by his magnificent book on the Mammals, appearing in 1883 in France.

First opinion: "Shall we admit scientifically the origin of the type of man from that of the monkey?" says Vogt on page 617 of his "Lectures on Man." "I have put before your eyes all the material known up to the present able to contribute to the knowledge of the bridge which shall span the abyss separating man from the monkeys." (I will give the substance of his remarks): I have shown to you the three great anthropomorphic monkeys on the one hand and the lower human races on the other forming uninterrupted series; the most ancient cranial forms approach to the simian type; furthermore the brain of a microcephal re-produces, as if for our instruction, that which should be the primitive brain, intermediate between that of man and that of the monkeys - - - the descent of man from the monkeys by derivation. But it does not follow that the descent operated in a single way. It has secondary types among the human races as it has them among the monkeys; but prolong the parallel series of Gratiolet and we have the multiple stocks of man.

Here is Vogt's textual conclusion: "The summary of these facts far from indicating a common stock, a unique intermediate form between monkey and man, shows us on the contrary numerous parallel series which must have developed (more or less circumscribed) from as many parallel series of monkeys" (page 626).

Second opinion: Less clear to my mind than the first. On the one hand Vogt maintains his former ideas of the polygenistic simian descent, on the other hand he reverses them by formally denying that man descended from the monkey. The following will better show the inciting causes which preceded his conclusion.

The monkeys to-day as in the Miocene and Pliocene epochs have always been settled in tropical climates, and are essentially arboreal; they leap from branch to branch and do not go far afield,—even those that are terricoles and clamber over the rocks. Between the monkeys of the Old and New Worlds the separation has been complete through all time; the two hemispheres have not been united since the Miocene at least, perhaps since the Eocene; the monkeys which cannot live in cold countries would be very wary of approaching Bering's Straits. The monkeys are little modified then throughout the Old World where they are more arboreal. Since the Miocene, one recognizes among them types high as the laopithecus of America and the dryopithecus of Europe; they have not evolved since. The example of the mesopithecus of Gaudry is the only one which we can cite in favor of any evolution whatever.

Nevertheless Vogt speaks here of a tendency toward a superior organization like that of man, of a similarity that is produced in different ways; the gorilla resembles man more in its limbs, the orang in its brain and the chimpanzee in its skull and teeth. "No fact," says he, "will permit us to admit of an unique line of evolution toward the human organization." Unique, perhaps! but what if multiple? For it would always be a descent from monkeys.

Passing then to the fossil species more particularly, Vogt insists on his proposition that there has not operated "any evolution of the simian type through the geologic periods;" that we can not "signalize any progress of that type since the time of the Upper Miocene." With the exception of one argument of his which I reserve for another place, that is all.

Very well, I must say that I see nothing to lead me to that conclusion. As I have shown just now that there is as much probability of an evolution among monkeys as in any other zoological group. No series of species leads, it is true, positively from any kind of monkey to any kind of man. But in paleontology what they show as a series of species, is usually but a series of characters. Now comparative anthropology shows us a multitude of characters forming series, going from the monkeys to man, by the way of or not of the anthropoids.

Vogt finishes with an argument which has a good deal of weight. "The infant monkey resembles man more than does the adult monkey, age alone emphasizes their characteristic differences by the evolution of the jaws, the cranial ridges etc." And he thus concludes: "From all these facts follows the conclusion that man can not be put into direct generic relation either with the existing monkeys or with any known fossil monkeys, but that both (man and monkey) have risen from a common stock of which the characters show themselves in youth more related to the stock than in the adult being."

A priori, the latter argument of Vogt is very correct. Every-one has remarked the contrast between the cranium of the young orang and the adult orang, of the young gorilla and the adult gorilla. Its value rests on the known principle of the parallelism of ontogeny and phylogeny which may be expressed thus: The forms of the young subject re-produce the forms that have existed among its ancestors and thus indicate their relationship. In other words, the character in progress, or new,—that which should relate a species to a following species, exists in the adult at his highest degree, whilst the character which belongs to the ancestors descends to the infant, though it disappears in the adult; for example, the exclusively pulmonary respiration in the adult salamander and the branchial respiration in the young salamander.

But one should separate that which is produced after birth and which is a matter of growth of the body, or of physiological development by the course of age in the individual life, from that which is an ancestral resemblance depending on embrylogy and intra-uterine ontogeny. In the young man as in the young monkey the skull is rounded in every sense, and smooth, being almost without asperities. The temporal ridges and sagittal ridge (which latter is but the result of elevation and pressure up against—causing ossification on the median line of the former), are ridges developed with age, especially in the male sex, and are proportionate with the strength of the muscles which are inserted on them. They reach considerable development among the monkeys in the species which have powerful masticatory apparatus and enormous temporal fossæ.

The supercilliary arches bulge out in man with age as in the monkeys, not taking so remarkable an aspect among the latter, because they have a more ample frontal sinus; a secondary character. The projecting jaw in both only becomes marked with age. The human child has a small orthognathous face, hidden under the skull, forming an enormous bowl as in the orang; the face grows, elongates and becomes more prognathous partly by simple increase of volume, whilst the skull diminishes relatively, partly because the molars of the second dentition have need of room and push forward. Among the monkeys this feature is very marked, but it has some distinctive characters in man.

Later, I will sum up to show how the agreements between the base of the skull and the base of the face follow the naso-basilar plane, changing proportionately in the adult compared with the child, the angles that craniomentry brings out in that part. The facial angle cited, since it enjoys a certain popularity, is greater in the young monkey as in the young of man. The infantile forms of the young monkey of which Vogt speaks, recur in part in the adult woman. They characterize the same way the male sex of certain races which writers have classed for that reason as infantile, such as the Andamanese.

There is a character implied in the argument of Vogt that seems to come very much to the support of his theory. It is that the young monkey, the orang, or the chimpanzee, for example, is more intelligent than the adult. Then ought not one to say that it has descended from an ancestor more intelligent than the present monkeys? But, greater intelligence is a rule among all young animals, as well as in man, if circumstances are taken into account. At that time the brain is relatively much larger than the body, it is virgin and every way more impressionable, it increases excessively and only asks that it absorb, that it work up, that it put to profit the blood it receives. What is more marvellous than the way our children learn to speak, write and read? Are we adults capable of the burden of quick memory required for the mass of words and ideas that they pick up at that time? Young Australians are equal to Europeans in the schools, they acquire language with an extraordinary facility; but the period comes, their savage nature returns, they drop their clothes, rejoin their kind and manifest no more intelligence than if they had never been among the whites. If at our age we appear so capacious, intellectually speaking, it is that we have accumulated for numerous years, that we reason from habit, in great measure automatically; we are constantly excited by the struggle for existence, by the society of our equals, by the use of language which the monkeys do not possess.

The last argument of Vogt, that the young monkey is more human than the adult, does not therefore convince me.

I have indicated the different current opinions, positive and negative, on the derivation of man. Are there not others, possible?

Although I have addressed many objections to Vogt, the very remarkable uncertainty on the part of a man who does not fear habitually to deliver himself, makes me reflect. I ask first what should be that common stock of the monkeys and man of which he speaks, and which is not the lemurs (Cope's theory)? Although Vogt leaves his reader in suspense, it is easy to discover his tendency. That stock started from some point in the ungulates. But if it is legitimate when one considers the present species the evolved extremities of the branch, it is less when one ascends towards the trunk before the specialization of the ungulates, particularly in that which concerns the four limbs, pushed to the extreme in two different ways, among the equidæ and among the ruminants. After that it must be said that nothing is impossible in nature, but the less probable things, when one sees their work, are attained by the most unforeseen processes and the veriest by-ways. That which selection by the hand of man gave to pigeons, a question so well studied by Darwin, is done again in nature by the hand of chance, the laws and mechanism of which escape us, and which we call by that name for just that reason.

There is an objection to the descent of man from the monkey that I have made, and which goes to the support of Vogt's thesis. As I have said previously, the primordial type of mammifers—(which it is needless here to separate into placental and aplacental,—all the placentals have certainly been aplacentals at their origin and the transition was produced insensibly without geology being able to establish at what time this form is aplacental and that analogous one placental)—the primitive type, I say, is with four limbs having already much that one can recognize, their destination already written, the four set apart for locomotion, but the anterior ones so as to serve moreover as organs of prehension and the posterior ones so as to serve essentially as organs of support and locomotion. This double specialization goes back to the reptiles, not to speak of the dinosaurs, among which it is so marked. Some amphibians show traces of it. Among the most ancient mammals known in all their parts, as the *Phenacodus primævus* of the Lower Eocene of the Tertiary of Wyoming Territory, in the United States, the fore limb is well marked as an organ of prehension and the hind limb as an organ of travel. In the first the humerus articulates within a narrow glenoid fossa at the upper external angle of the scapula in such a manner as to permit the most extensive movements in divers ways, the radius is movable over the ulna, around which it accomplishes the turning movement made necessary by the function of the hand; the five fingers are free, the thumb is more turned on its axis to admit of opposition; the hand is continued on a straight line with the fore-arm. In the leg the femur, as with us, is united to a massive pelvis; the articular surfaces of the knee, the knee-cap, the two immovable bones of the leg are entirely such as arise from the function of locomotion exclusively; the foot is plantigrade, with salient heel, with digits close together, and it is articulated perpendicularly by its arch to the leg, as in man. In another contemporaneous animal and of the same deposit, the coryphodon, (of which I can only judge by the foot and hand figured, but entire,) these two organs show more resemblance, the foot looks a little like a hand, but there is nevertheless a differentiation. But in man that specialization or differentiation has attained its maximum; no other animal is found in the same rank. Among the birds the upper limb has become a wing, a function of locomotion. In man alone the upper limb is exclusively a hand, the lower limb combines in itself all the locomotive function that it divided formerly within certain limits with the anterior, but which nevertheless always retained its essential attribute. It seems then that man should be the direct continuation of the first Eocene mammals, if not of the marsupials which preceded them, the completion of a type begun, and it seems scarcely logical that his transformation should be accomplished at the expense of a branch that seems collateral. The monkeys are produced by the fact of the adaptation of the lower limb to an arboreal life; the upper limb remained as it was; it is a deviation of the axis of evolution, in some way, a deviation from the primitive type. On the one hand the ungulates are detached from the primitive type by a metamorphosis of the anterior limb designed for prehension, into a limb designed for running, and by a harmonious perfection of the four limbs in the same way; on the other the carnivores, whose four extremities, also the teeth, the jaw, and the entire skull put themselves into harmony with the needs to which they are subject and the mode of life and diet adopted; also the monkeys, who avoid the earth usurped partly by the swift herbivores, partly by the sanguinary carnivores, are refugees in the trees, where nevertheless they have prospered; they are supported there and consequently they have appropriated their extremities to that special life. Man being born from the monkeys by the disappearance of the accidental adaptation of the hinder limb to the function normally belonging to the fore limb, that is to say, returning to their primitive archi-ancestral type, such a thing would appear strange! Assuredly such a thing may be; for nature, as I have said, does not take the shortest road. From the carnivora, which are terrestrial animals, have descended in remote times a multitude of animals with fins called the pinnipeds. By a retrogression the latter have seen their limbs atrophy, come close to the body in the form of a paddle, and play the part of fins. But the most probable is generally the simplest. This bend in the road that would have determined the evolution of man, or rather of one of his precursors, is useless. It seems more rational to conceive of the perfect biped and biman type descending from a type that we have seen already sketched in the Eocene times and constituting the fundamental original type of the mammals. It would have been necessary then to consider the branch of the monkeys as a collateral branch in which evolution would not have surpassed that which the present and fossil anthropoids show us.

This hypothesis would resolve certain difficulties which seem unsurmountable in anthropology. The most inferior human races known to us are so near to the superior races in contrast to the distance which separates them from the monkeys, that we can consider the different men as forming an entirely relatively homogeneous, uniform species as M. de Quatrefages maintains. The most ancient human race, that of Neanderthal, is in the same position, whatever they say of it. His cranial capacity, that is to say, that feature which really characterizes man, is indeed considerable and higher than the most inferior of the present human races, such as the Australians. Between the lowest mean of the capacity of the skull of the human races, which I fix at 1100 cubic centimeters in round numbers, and the mean of the highest anthropoid species, which I put at 530 cubic centimeters,* the distance is prodigious when one compares it to the trifling mean differences, one notices between the species, genera, families, and orders of animals coming after, and also when one takes into account the comparison of this volume of the brain with that of the body. In order that this cerebral transformation should be accomplished it has required an unheard of time, defying all our conjectures.

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* From all the absolute *weights* known of the brain, and from all the cranial *capacities* utilizable in the series of the vertebrates, in dwelling on the two limits of the series, I have made out for the latter two schematic tables showing the differences that are presented; first, the general means of man and the anthropoids (Gibbons left out); second, their particular means, the lowest in the human races, the highest among the anthropoids; third, the extreme individual cases, the weakest normal in man, the strongest in the anthropoids. Combining these two tables, that is to say, associating the products furnished by the weight with that furnished by the capacity. I then drew up a third schematic table which gives me an intermediate value, that I have designated under the name of *cerebral volume*.

Here are the results:

(1) The distance between the general mean of humanity and the general mean of the anthropoids (Gibbon always excepted) is 70 to 100 of the first of these means; or the mean normal brain of man is two and one-half times larger than the mean brain of the anthropoids.

(2) The distance from man to the anthropoid in the general means being taken as 100, the distance between the particular means, the lowest observed in the human races and highest found in the three genera of anthropoids is 48, and the distance between the extreme individual cases the closest on the one side and the other is 26.

It is evident that gradually as new material is gathered these figures may vary and that being an intermediate value between two different data, the one expressed in grammes and the other in cubic centimeters hence they have not an absolute value. But such as they are they permit us to associate the data which, separate are frequently insufficient, and throw clearly into relief that gulf that at the present time separates man and the anthropoids (Orang, Chimpanzee, and Gorrilla).

Pliocene man has probably been found in America. Miocene man is incontestable, though we have not been able to prove it clearly. But in the miocene the monkeys are seen for the first time with their present characters. Man would then have established himself since their appearance. Would evolution have chosen an animal whose posterior member was organized for an arboreal life, was at the same time foot and hand, when by the side of and existing prior to it were animals whose organization presented part of the wished-for characters? It is scarcely probable, and considering (I repeat), the number of intermediate species which have been necessary before arriving to the present constitution of our brain, it seems probable that the introduction of man had taken place sooner in the eocene epoch by means of one of the condylarthres having already the principal morphlogical characters of man, those relating to the brain excepted, that Cope shows us served as intermediary to the marsupials and the most of the present mammals. There the differentiations were made according with the different modes of life, which have given on one side, the branch of the ungulates, the branch of the carnivores and many others that have disappeared without founding a stock, on the other side, the branch of the quadrumana, and the human branch.

The human type, that is the cerebral type before culminating in the astonishing development which we perceive and beside which all the rest is but accessory had then a proper stock, a stock that had been the most central continuation of the general primitive trunk of the mammals! In the present state of science they usually compare the make-up of the mammals to a tree ramified into numerous main branches, each of these terminating in efflorescences higher in growth. These are our better specialized groups, viz: the equidæ and the ruminantia among the ungulates, the lion or the dog among the carnivores, etc. In the new system, the comparison with a growing tree of which the central axis sends out the lateral branches would be more correct, the central stock continues to elevate itself as the Lombardy poplar, and bears at its summit, man.

IV.—CONCLUSION.

We have examined the genealogy proposed by Hæckel, and the systems proposed to replace it. Whether the vertebrates have had for their starting point a worm with a soft body destitute of a skeleton, or on the contrary, a crustacean possessing an entirely exterior skeleton, we have previously concluded that our genealogy has passed by the ganoid fishes to join with those called by palæontologists labyrinthodonts, which I have often designated by the name of middle vertebrates. There the current has carried us along not in the direction of the mammals, which however already appeared in the triassic epoch, but plainly to the kingdom of the reptiles where we have to deal with the dinosaurian origin of the monotremes or of some analogous group. We have found the placental marsupials (designated by us under the name of confirmed proto-mammals), and we have shown whence with certain proximate reservations (the whales for instance), all the present placental mammals have issued, and consequently our race. But here the problem is complicated. Until that point, our origin appeared clear—save at the very origin of mammals. The lemurs are already a cause of embarrassment. On the immediate descent of man, the uncertainties increase. Many opinions each expressed by illustrious authorities are before us; sometimes making objection, sometimes making confirmatory arguments.

There are only two doctrines to consider; one that makes man come from the primary trunk of the mammals in a direct line and without intermediate orders, not from a mathematical point, but from that confused mass succeeding the marsupials in which the differentiations are undecided and tend toward the ungulates, or toward man; the other which accepts the branch or order of the primates with all its consequences, the lemurs or pro-simians at the base, the monkeys or simians following, and man all alone at the summit.

After a careful balancing of the two, I confess that I incline toward the latter solution, and conclude for our descent from the monkey. In my mind one consideration out-ranks all others. The type of cerebral convolutions among all primates where it is well characterized in its ascending evolution, is that of man; it varies from the cebian, to the pithecus, from the latter to the anthropoid, and from it to man, only in degree.[*] The extreme development of the simian type of convolutions and the abrupt increase of the volume of the brain in going from the anthropoid to man on which I have laid so much stress are the two fundamental anatomical characters of man, histological examination being left out of consideration.[†]

If on the one hand we find as details that the foot of monkeys has a thumb more or less opposable; that the latter should be more or less adapted to their arboreal life; that it might seem strange to us that the human line after having seen its foot partly transformed should take again the original foot of its remoter ancestors; on the other hand, we have the details of the cranial and facial characters that result in man from the great volume of his brain, the atrophy of the nasal fossæ and of their numerous posterior cavities (posterior nares) which has led to the disappearance of the muzzle, the perfection (in compensation) of touch and sight, which with the modifications that necessitated the equilibrium of the skull, have raised him up to the bipedal attitude, and have thus evolved an entirely new series of differential characters. That which rules all is the already human cerebral type, but in a rudimentary state among the monkeys, as it is the same type, amplified and perfected in man.

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[*] See P. Broca, "Anatomie comparée des circonvolutions cérébrales." *Revue D'Anthropologie,* 1878, page 385.

[†] According to M. Chudzinski, so competent on this subject, not only the type of the convolutions but to an equal degree the muscular and visceral anomalies showing themselves in man plead in favor of simian descent. Certain muscular anomalies give likewise a reversion towards the state of climbers or tree dwellers (see Chudzinski's memoir in the *Revue d'Anthropologie,* "On the muscular and visceral variations among the races" and in the bulletins of the Societe' d'Anthropologie, "An anomaly observed in the Orang)." See also his great work crowned by the Institute, "On the comparative anatomy of the convolutions," that appeared in 1878 and of which the *Revue d'Anthropologie* has given a review in the volume of 1879, page 707.

All the organs, foot, hand, teeth, thorax, pelvis, and digestive tract, evolved among the mammals, have been transformed capriciously, have taken different ways, and are specialized in different senses, sometimes in the same line. One only remained stationary, or little varied; that is the brain, except in man. In him or one of his ancestors among the primates it has taken its rise, it has grown and developed, bending everything to its needs, subordinating everything to its own life, the skull, the face, the whole body, putting on everthing its imprint. The fish swims, the ruminant browses, the carnivore seeks his prey, the monkey is arboreal, man thinks. Everything in him gravitates around that characteristic. The philosopher has said truly: "Man is an intelligence served by organs."

We have descended then from the monkeys, or at least everything appears *as if* we had descended from them. From what monkey known or unknown? I do not know; no one of the present Anthropoids has assuredly been our ancestor. From several monkeys or a single one? I do not know; and also do not know yet if I am monogenistic or polygenistic. In the study of the human races I see arguments for and against both systems. Until further knowledge is arrived at, we must reserve our opinion.

We must see what arguments comparative craniology will bring in favor of or against this or that genealogy. At that time alone will we be permitted to determine on the place that anthropology gives to man in nature. Whatever may be the result arrived at, that place—believe me—will be as enviable as you could desire. At the origin, towards the beginning of the Miocene perhaps, monkey and man were but one; a division takes place, the fissure has grown, has become a crevasse; later an abyss, with talus more and more scarped, like the cañons of the Colorado;—an abyss which our friend, Abel Hovelacque, does not wish to see, but that Messrs. Vogt and Huxley (little suspected of orthodoxy) admit;—an abyss that widens every day under our eyes, though permitting still the recovery of those lost paths going from one side to the other, (of which Huxley speaks in his preface to his book on "The Place of Man in Nature,") but which sooner or later will become impassable by the disappearance on the one hand of the last of the present anthropoids, and on the other of the lowest human races, and will leave man isolated and majestic, proclaiming himself with pride the king of creation.

Let us not blush then for our ancestors; we have been monkeys, as those formerly have been reptiles, fish, nay worms or crustaceans. But it was a long time ago, and we have grown;—evolution I say has been very prodigal of its favors in the struggle for existence, she has given all the advantages to us. Our rivals of yesterday are at our mercy, we let those perish that displease us, we create new species of which we have need. We reign over the whole planet, fashioning things to our will, piercing the isthmus, exploiting the seas, searching the air, annulling distance, wringing from the earth her secular secrets. Our aspirations, our thoughts, our actions have no bounds. Everything pivots around us. What is there to desire more? That the future will perhaps reveal. Evolution has not said its last word.

THE STATE AND HIGHER EDUCATION.*

By HERBERT B. ADAMS, PH. D.

This is an era of educational endowment upon a generous scale. A recently published report of Col. N. H. R. Dawson, Commissioner of Education, shows that the sum total of noteworthy educational gifts during the year 1886–'87, was nearly $5,000,000. More than two-thirds of the entire amount were distributed among nine institutions, four of them collegiate, one academic, three professional, and one technical. The institution most highly favored was Harvard University, which received from individual sources nearly $1,000,000. From one man came a legacy of $630,000. Haverford College, supported by the Society of Friends, received $700,000 in one bequest. Of the two hundred and nine gifts recorded by the Commissioner of Education, twenty-five represent $50,000 or more; seventy-two were sums between $5,000 and $49,000; and one hundred and twelve were sums less than $5,000. The most striking fact in all this record of philanthropy is that such a large proportion of the entire amount, fully two-thirds, was given to higher education. The year 1888 is richer than 1887 in individual bounty to institutions of learning. Nearly ten millions were given by three persons for the encouragement of manual training, but there are rumors of even larger benefactions for university endowment. The collective returns for 1888 are not yet published, but it is certain that the past year will surpass any hitherto recorded in the annals of American education.

Whatever forms modern philanthropy may take, one thing is certain, universities are not likely to be forgotten. At the founding of the new Catholic University in Washington, Bishop Spalding said that a university "is an institution which, better than anything else, symbolizes the aim and tendencies of modern life." Will not broad-minded people recognize the truth of this statement and strengthen existing foundations? Senator Hoar, at the laying of the corner-stone of the new Clark University, said, "The university is the bright consummate flower of democracy." Will not American patriots cultivate endowments made by the generosity of sons of the people? Are the noble gifts of Johns Hopkins for the advancement of learning, and the relief of suffering, likely to be forgotten by present or future generations? All history testifies to the gradual up-building of universities by individual benefactions. The development of European and American colleges is one long record of private philanthropy. *Private philanthropy will do all it can*, but public interest demands that the State should do its part.

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* An address delivered before the Department of Superintendence of the National Educational Association, in the National Museum, Washington, D. C., March 8, 1889.

The encouragement of higher education by government aid, in one form or another, has been a recognized principle of public policy in every enlightened state, whether ancient or modern. Older than the recognition of popular education as a public duty was the endowment of colleges and universities at public expense for the education of men who were to serve church or state. It is a mistake to think that the foundation of institutions by princes or prelates was a purely private matter. The money or the land always came from the people in one form or another, and the benefit of endowment returned to the people sooner or later. Popular education is the historic outgrowth of the higher education in every civilized country, and those countries which have done most for universities have the best schools for the people. It is an error to suppose that endowment of the higher learning is confined to Roman and German emperors, French and English kings. Crowned and uncrowned republics have pursued the same public policy. Indeed, the liberality of government towards art and science always increases with the progress of liberal ideas, even in monarchical countries like Germany, where, since the introduction of parliamentary government, appropriations for university education have greatly increased. The total cost of maintaining the Prussian universities, as shown by the reports of our Commissioner of Education is about $2,000,000 a year. Only about 9 per cent. of this enormous outlay is met by tuition fees. The state contributes all the rest in endowments and appropriations. Prussia now gives to her universities more than twice as much as she did before the Franco-Prussian war, as shown by the report of our commissioner at the Paris Exposition in 1867. In that year France gave her faculties of higher instruction only $765,764. After the overthrow of the second empire, popular appropriations for higher education greatly increased. The budget for 1888, shows that France now appropriates for college and university faculties $2,330,000 a year, more than three times the amount granted under Louis Napoleon. Despotism is never so favorable to the highest interests of education as is popular government. Louis XIV, and Frederick the Great, according to the authority of Roscher, the political economist, regarded universities, like custom-houses, as sources of revenue, for the maintenance of absolute forms of government. The world is growing weary of royal munificence when exercised at the people's expense, with royal grants based upon popular benevolence and redounding to the glory and profit of the prince rather than to the folk upholding his throne. Since the introduction of constitutional government into European states, representatives of the people are taking the power of educational endowment and subsidy into their own hands, and right royally do they discharge their duty. The little Republic of Switzerland, with a population of only three millions, supports four state universities, having altogether more than three hundred instructors. Its cantons, corresponding upon a small scale to our States, expend over $300,000 a year upon the higher education. The federal government of Switzerland appropriated, in 1887, $115,000 to the polytechnicum and $56,000 in subsidies to cantonal schools, industrial and agricultural; besides bestowing regularly $10,000 a year for the encouragement of Swiss art. The aggregate revenues of the colleges of Oxford, based upon innumerable historic endowments, public and private, now amount to fully $2,000,000 a year. The income of the Cambridge college endowments amounts to quite as much. But all this, it may be said, represents the policy of foreign lands. Let us look at home, and see what is done in our own American commonwealths.

Maryland began her educational history by paying a tobacco tax for the support of William and Mary College, in Virginia. This colonial generosity to another State has an historic parallel in the appropriation of a township of land by Vermont for the encouragement of Dartmouth College in the State of New Hampshire, and in the corn that was sent from New Haven to the support of young Harvard. In colonial days Maryland had her county schools, some of them classical, like King William's School at Annapolis. All were founded by authority of the colonial government and supported by aid from the public treasury. The principle of state aid to higher education runs throughout the entire history of both State and colony.

The development of Maryland colleges began on the Eastern Shore. In the year 1782, representatives of Kent County presented a petition to the legislature, saying that they had a flourishing school at Chestertown, their county seat, and wished to enlarge it into a college. The general assembly not only authorized the establishment of Washington College, which still exists, but in consideration of the fact that large sums of money had been subscribed for the institution by public-spirited citizens of the Eastern Shore, resolved that "such exertions for the public good merited the approbation of the legislature and ought to receive public encouragement and assistance." These are the very words of representatives of Maryland more than a century ago. Their deeds were even better than their words. They voted that £1,250 a year shoud be paid from the public treasury for the support of Washington College. That vote was passed just after the conclusion of a long war with England, when the State and indeed the whole country lay impoverished. Toward raising this government subsidy for higher education, the legislature granted all public receipts from marriage licenses, from liquor licenses, fines for breaking the Sabbath, and all similar fines and licenses that were likely to be constant sources of revenue.

The founding of St. John's College occurred two years later, in 1784. This act by the State of Maryland was also in response to a local demand. It was urged by the citizens of Annapolis that King William's School, although a classical institution, was inadequate to meet the educational demands of the age. It was very properly added that the Western Shore, as well as the Eastern, deserved to have a college; and so St. John's was established as the counterpoise of Washington College. The legislative act is almost identical with that establishing the earlier institution, although the appropriation was larger. The legislature gave St. John's 4 acres of good land for college grounds, and building sites and an annual appropriation of £1,750 current money. This sum, in the words of the original act, was to "be annually and forever hereafter given and granted as a donation by the public to the use of said college on the Western Shore to be applied by the visitors and governors of the said college for the payment of salaries to the principal, professors, and tutors of said college." The establishment was to be absolutely unsectarian. Students of any denomination were to be admitted without religious or civil tests. Not even compulsory attendance upon college prayers was required so modern were the legislative fathers of Maryland.

The next step in the higher educational history of Maryland was the federation of the two colleges into the University of Maryland. The two boards of visitors and two representatives of each faculty constituted the University Convocation, presided over at Annapolis on commencement day by the governor of the State, who was *ex officio* chancellor of the University. One of the college presidents acted as vice-chancellor. Thus more than a century ago Maryland inaugurated a State system of higher education which, if it had been sustained, would have given unity and vigor to her academic life. But unfortunately, in 1794, the legislature yielded to county prejudices and withdrew £500 from the £1,250 annually granted to Washington College and began to establish a fund, the income of which was distributed among various county academies on both shores of the Chesapeake. This was the origin of the subsidies still given in one form or another to secondary institutions in the State of Maryland. In 1805, the remaining appropriation of £750 belonging to Washington College and the entire £1,750 thitherto granted to St. John's College were withheld for the avowed purpose of "disseminating learning in the different counties of the State."

For six years there was a famine in the land as regards the support of higher education. At last in 1811, the legislature resumed appropriations to St. John's College. Realizing that it had misappropriated to local uses subsidies "granted annually forever" to St. John's, the legislature endeavored for many years to compromise by giving a smaller allowance.

The court of appeals ultimately decided in 1859 that such a re-adjustment was a breach of contract, and that the college could collect what was due it from the State. There is perhaps some excuse for the economy of Maryland in its treatment of St. John's College, namely, "hard times." A State that went through the financial crises of 1837 and 1857 without repudiation deserves some historical credit. St. John's College was suspended during the civil war, but appropriations were renewed in 1866, and have been continued, with slight variations, down to the present day. The amount granted in 1888 was $3,000 for the institution itself and $5,200 for boarding twenty-five students, one from each senatorial district.

The first University of Maryland ceased to exist by the act of 1805, which withheld appropriations from St. John's College; but in the year 1812, a new University of Maryland was instituted by authority of the State, in the city of Baltimore. The proceeds of a State lottery were granted to the institution for a library, scientific apparatus, botanical garden, etc. The corporation was to have a full equipment of four faculties, representing the arts, law, medicine, and theology. Two faculties of law and medicine still perpetuate the spirit of the founders of the University of Maryland, and are honorable and distinguished promoters of professional education. It can not be said that they were ever treated with adequate generosity, though they actually received from State lotteries between $30,000 and $40,000, and were never taxed.

The present generation has not been so generous to the cause of higher education as were the fathers of the State, but nevertheless Maryland, in her entire history, has appropriated something over $650,000 for what may be strictly called college education, not counting $60,000 given to the State Agricultural College, nor $40,000 proceeding from State lotteries. While this collective bounty is small, it is money given by voluntary taxation and not taken from institutions of learning. Most of the amount was raised in times when the State was poor or heavily in debt, and when public money came with difficulty. Moreover this financial generosity of Maryland establishes the principle for which we are contending, namely, that this State, like all other enlightened States in the world, has recognized the duty of support to higher and unsectarian institutions of learning. She has at different times appropriated $650,000 to colleges and to the University of Maryland from her public treasury.

Let us now inquire what other States in the American Union have done for higher education, always recognizing of course great inequality in State population and in the taxable basis.

Virginia, whose earliest educational foundations Maryland helped to lay by her tobacco tax, has expended upon colleges and university over $2,000,000, during her history as a State, not counting the colonial bounty to William and Mary. Since the war, Virginia has given her university $40,000 a year. Before the war, she gave $15,000 a year.

The original university-establishment cost the State about $400,000. The people of Virginia are proud of their university, and it would be suicide for any political party to cut off the yearly appropriation from the institution founded by Thomas Jefferson. The State of South Carolina was Jefferson's model for generous appropriations to the cause of sound learning. She has given $2,800,000 to that object. Georgia has given $938,000 for the same purpose. Louisiana has given $794,000 from her State treasury for the higher education in recent years, and according to the testimony of her own authorities, has distributed over $2,000,000 among schools, academies, and colleges. Texas has spent upon college education $382,000, and has given for higher education 2,250,000 acres of land. The educational foundations, both academic and popular, in the Lone Star State, are among the richest in America.

Turning now to the Great West, we find that Michigan has given over $2,000,000 to higher education. She supports a university which is as conspicuous in the Northwest as the University of Virginia is in the South, upon one-twentieth of a mill tax on every dollar of taxable property in the State. That means half a cent on every hundred dollars. This university tax-rate yielded last year $47,272. Wisconsin pays one-eighth of a mill tax for her university, and that yields $74,000 per annum. Wisconsin has given for higher education $1,200,000. Nebraska is even more generous to her State university. She grants three-eighths of a mill tax, yielding about $60,000 a year. The State of California grants one-tenth of a mill tax, which yielded last year over $76,000. Besides this, the University of California has a permanent State endowment of $811,000, yielding an annual income of $52,000, making a total of $128,000 which the State gives annually to its highest institution of learning. Altogether California has expended upon higher education $2,500,000. The State of Kansas, the central empire of the Great West, gives already its rising university at Lawrence $75,000 a year, "levied and collected in the same manner as are other taxes."

It is needless to give further illustrations of State aid to American universities. These statistics have been carefully collected from original documents by historical students, who are making important contributions to American educational history, to be published by the United States Bureau of Education. The principle of State aid to at least one leading institution in each commonwealth is established in every one of the Southern and Western States. In New England Harvard, and Yale, and other foundations of higher learning appear now to flourish upon individual endowments and private philanthropy; but almost every one of these collegiate institutions, at one time or another, has received S'ate aid. Harvard was really a State institution. She inherited only £800 and three hundred and twenty books from John Harvard. She was brought up in the arms of her Massachusetts nurse, with the bottle always in her mouth. The towns were taxed in her interest, and every family paid its peck of corn to make, as it were, hoe-cake for President Dunster and his faculty. Harvard College has had more than $500,000 from the public treasury of Massachusetts. Yale has had about $200,000 from the State of Connecticut. While undoubtedly the most generous gifts have come to New England colleges from private sources, yet every one of them, in time of emergency, has come boldly before representatives of the people and stated the want. They have always obtained State aid when it was needed. Last year the Massachusetts Institute of Technology became somewhat embarrassed financially, and asked the legislature for $100,000. The institution got $200,000, twice what it asked for, upon conditions that were easy to meet.

Turning now from historic examples of State aid to the higher education by individual American commonwealths, let us inquire briefly concerning the attitude of the United States Government towards institutions of science and sound learning.

Washington's grand thought of a National University, based upon individual endowment, may be found in many of his writings, but the clearest and strongest statement occurs in his last will and testament. There he employed the following significant language:

> "It has been my ardent wish to see a plan devised on a liberal scale which would have a tendency to spread systematic ideas through all parts of this rising empire, thereby to do away local attachments and State prejudices, as far as the nature of things would, or indeed ought to, admit from our national councils. Looking anxiously forward to the accomplishment of so desirable an object as this is, in my estimation, my mind has not been able to contemplate any plan more likely to effect the measure than the establishment of a *university* in a central part of the United States, to which the youths of fortune and talents from all parts thereof may be sent for the completion of their education, in all branches of polite literature, in arts and sciences, in acquiring knowledge in the principles of politics and good government, and as a matter of infinite importance in my judgment, by associating with each other, and forming friendships in juvenile years, be enabled to free themselves in a proper degree from those local prejudices and habitual jealousies which have just been mentioned, and which, when carried to excess, are never-failing sources of disquietude to the public mind, and pregnant of mischievous consequences to this country. Under these impressions, so fully dilated, I give and bequeath, in perpetuity, the fifty shares which I hold in the Potomac Company · · · towards the endowment of a university, to be established within the limits of the District of Columbia, under the auspices of the General Government, if that Government should incline to extend a favoring hand towards it."

Here was the individual foundation of a National University. Here was the first suggestion of that noble line of public policy subsequently adopted in 1846, by our General Government in relation to the Smithsonian Institution. The will of James Smithson, of England, made in 1826, was "to found at Washington, under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men." A simpler educational bequest, with such far-reaching results, was never before made. Whether James Smithson was influenced to this foundation by the example of Washington is a curious problem. Smithson's original bequest, amounting to something over $500,000, was accepted by Congress for the purpose designated, and was placed in the Treasury of the United States, where by good administration and small additional legacies (in two cases from other individuals) the sum has increased to over $700,000. Besides this, the Smithsonian Institution now has a library equal in value to the original endowment, and acquired by the simple process of government exchanges; and it owns buildings equal in value to more than half the original endowment. During the past year, as shown by the Secretary's report, the Institution was "charged by Congress with the care and disbursement of sundry appropriations,"* amounting to $220,000. The National Museum is under the direction of the Secretary of the Smithsonian Institution, and the Government appropriations to that museum, since its foundation, aggregate nearly $2,000,000. The existence and ever-increasing prosperity of the Smithsonian Institution are standing proofs that private foundations *may* receive the fostering care of government without injurious results. Independent administration of scientific institutions can co-exist with State aid. It is a remarkable testimony to the wisdom of George Washington's original idea that Andrew D. White, who, when president of Cornell University, happily combined private endowments and Government land grants, lately suggested in *The Forum*† the thought of a national university upon individual foundations. This thought is a century old, but it remains to this day the grandest thought in American educational history.

George Washington, like James Smithson, placed a private bequest, so that the General Government might extend to it "a favoring hand;" but in those early days Congress had no conception of the duties of government towards education and science, although attention was repeatedly called to these subjects by enlightened Executives like Thomas Jefferson, "father of the University of Virginia," James Madison, James Monroe, and John Quincy Adams. It took Congress ten years to establish the Smithsonian Institution after the bequest had been accepted and the money received. Unfortunately, George Washington's Potomac stock never paid but one dividend, and there was no pressure in those days towards educational appropriations from an ever-increasing surplus. The affairs of the Potomac Company were finally merged into the Chesapeake and Ohio Canal, which became a profitable enterprise and endures to this day. What became of George Washington's "consolidated stock" of that period, history does not record. Jared Sparks, Washington's biographer, thought the stock was "held in trust" by the new company for the destined university. There is probably little danger that it will ever be thrown upon the market in a solid block by the Treasury of the United States, to which the stock legally belongs, unless the present surplus should suddenly vanish, and the General Government be forced to realize upon its assets for the expenses of the administration.

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* Report of Samuel P. Langley, Secretary of the Smithsonian Institution, 1887-'88, p. 7.

† *The Forum*, February, 1889.

George Washington's educational schemes were by no means visionary. His stock in the James River Company, which, like the Potomac Company, he had helped to organize, actually became productive and was by him presented to Liberty Hall Academy, now Washington and Lee University, at Lexington, Va., where General Lee died and was buried, having served his native State, as did George Washington, in the capacity of a college president. Washington raised Liberty Hall Academy to what he called "a seminary of learning upon an enlarged plan, but not coming up to the full idea of university." He meant to make it one of the three Virginia supporters of the university at Washington. Liberty Hall, or Washington College, his own William and Mary, and Hampden-Sidney, were all to be State pillars of a national temple of learning.

Washington's dream of a great university, rising grandly upon the Maryland bank of the Potomac, remained a dream for three-quarters of a century. But there is nothing more real or persistent than the dreams of great men, whether statesmen like Baron von Stein, or poets like Dante or Petrarch, or prophets like Savonarola, or thinkers like St. Thomas Aquinas, the fathers of the church and of Greek philosophy. States are overthrown; literatures are lost; temples are destroyed; systems of thought are shattered to pieces like the statues of Pheidias; but somehow truth and beauty, art and architecture, forms of poetry, ideals of liberty and government, of sound learning and of the education of youth—these immortal dreams are revived from age to age and take concrete shape before the very eyes of successive generations.

The idea of university education in the arts and sciences is as old as the schools of Greek philosophy. The idea was perpetuated at Alexandria, Rome, and Athens under the emperors. It endured at Constantinople and Ravenna. It was revived at Bologna, Paris, Prague, Heidelberg, Oxford, and Cambridge under varying auspices, whether of city, church, or state, and was sustained by the munificence of merchants, princes, prelates, kings, and queens. Ideas of higher education were transmitted to a new world by Englishmen who believed in an educated ministry and who would not suffer learning to perish in the wilderness. The collegiate foundations laid by John Harvard in Massachusetts and Commissary Blair in Virginia were the historic models for many similar institutions, north and south. George Washington, the chancellor of William and Mary, when he became President of a Federal republic, caught up, in the capital of a westward-moving empire, the old university idea and gave it national scope. There upon the Potomac he proposed to found a National University, drawing its economic life from the great artery of commerce which connects the Atlantic seaboard and the Great West. As early as 1770 Washington described this Potomac route as "the channel of the extensive and valuable trade of a rising empire."

Was it not in some measure an historic, although an unconscious, fulfillment of that old dream of Washington when, a hundred years later, Johns Hopkins determined to establish upon the Maryland side of the Potomac a university with an economic tributary in the Baltimore and Ohio Railroad, which follows the very windings of that ancient channel of commerce? Forms of endowment may change, but university ideas endure. They are the common historic inheritance of every enlightened age and of every liberal mind; but their large fulfillment requires a breadth of foundation and a range of vision reaching beyond mere locality. Universities that deserve the name have always been something more than local or provincial institutions. Since the days when Roman youth frequented the schools of Grecian philosophy, since the time when ultramontanes and cismontanes congregated at Bologna, since students organized by nations at Paris, Prague, and Heidelberg, since northern Scots fought southern Englishmen at Oxford, university life has been something even more than national. It has been international and cosmopolitan. Though always locally established and locally maintained, universities are beacon lights among the nations, commanding wide horizons of sea and shore, catching all the winds that blow and all the sun that shines, attracting, like the great light-house of Ptolemy Philadelphus on the island of Pharos, sailors from distant lands to Alexandrine havens, or speeding the outward voyager.

The nations of the Old World are proud of their universities and colleges. Three years ago all Germany and the learned institutions of all Europe united in celebrating the five hundredth anniversary of "Alt Heidelberg." Last summer at Bologna, under the auspices of the Italian Government and of the minister of public instruction, the whole civilized world was represented by academic delegates, who had come joyfully together to celebrate the thousandth birthday of "the mother of studies." Every country in Europe takes pride in the history of its universities and of its system of public education. It is time that something should be done for the history of learning in these United States. Dr. G. Stanley Hall, the president of Clark University, in his *Bibliography of Education* (page 41), says: "A history of educational institutions in this country is greatly needed. The field is very rich and almost unknown. No comprehensive history exists."

Before educational specialists can have a History of American Education that is worthy of the name, there must be a vast amount of special investigation. There must be many local and State contributions to the subject before national generalizations of any permanent or practical value can be drawn by educators. One might as well generalize upon the character of the American people without an historical study of immigration and without taking a census of the population, as to write a History of American Education before obtaining the local facts.

A great deal of pioneer work in this direction has been done in Barnard's American Journal of Education; in the Annual Reports and Circulars of Information published by the United States Bureau of Education; in the periodical and educational journals of the country, and in the local histories of particular institutions of learning and of particular systems of schools. A strong and novel impulse in the direction of organized inquiry concerning the history of educational institutions in this country was communicated to the country in the centennial year, 1876, by General John Eaton, then Commissioner of Education. The spirit of local co-operation was enlisted in many of our American colleges, and considerable historical work was then done. Some of it was locally published, but most of it never saw the light. Popular support and Government appropriations were lacking for the adequate prosecution of the work. One magnificent result however of this new spirit of organized inquiry was the great volume on the public libraries of the United States, their history, condition, and management, published in 1876, by the Department of the Interior for the Bureau of Education. A single contribution to the history of education, edited by Dr. Franklin B. Hough, was published by the Bureau in 1883. It was a pamphlet of 72 pages, called Historical Sketches of the Universities and Colleges of the United States, and related to the University of Missouri.

The idea of systematically investigating the history of higher education in this country was revived anew in connection with an inquiry undertaken by the speaker, at the instance of General John Eaton, concerning The Study of History in American Colleges and Universities. Although concerned primarily with the history of a single department of instruction in a few representative colleges, like Harvard, Yale, Columbia, Cornell, the University of Michigan, and the Johns Hopkins University, the writer could not fail to discover something of the general historic interest belonging to the development of certain institutions of learning north, south, and west. To represent a school of history and politics in the South, he had proposed to introduce an account of William and Mary College into the above report, but this institution proved so generally interesting, as representing the history of education in Virginia, that the present Commissioner of Education, Col. N. H. R. Dawson, under whom the report on The Study of History was completed for publication in 1887, encouraged further elaboration of the above account for a special Circular of Information. The monograph on The College of William and Mary; a Contribution to the History of Higher Education, with Suggestions for its National promotion, was issued as Circular No. 1, 1887, and was cordially welcomed by the friends of higher education in all parts of the country,—north, south, east, and west. Aside from the generous public interest kindled in the honorable history and sad misfortunes of this oldest of Southern colleges, next to Harvard the oldest in the country, the publication of this circular accomplished the more practical result of arousing the State of Virginia to a consciousness of its educational inheritance and to an appropriation to restore the old college to a career of active usefulness. If no other end than this had been effected by the above circular, the Bureau of Education would have been justified in entering the field of Southern educational history, where historical and quickening work is most needed. To illustrate the effect upon the educators of the North as well as at the South, it may be added that this monograph furnished materials for a presidential address at a well-known Northern University and for an historical oration at one of the most influential Southern universities.

The immediate success of this pioneer monograph on William and Mary College led the way to the larger thought of treating the history of higher education by States. Accordingly the remaining colleges of Virginia were grouped together by the editor in authorized sketches, supplementary to a study of Thomas Jefferson and the University of Virginia. The idea was favorably received. The need of making generally known throughout the whole country the higher educational systems of individual States and sections is illustrated by the remark of a college trustee in Massachusetts concerning the University of Virginia: "I had not the faintest idea that any such university ever existed or that education ever was on so high a plane in the South." Jefferson's ideas concerning university education, and indeed concerning education in general for this country, were far in advance of his time. Through the instrumentality of professors like George Long, Thomas Hewett Key, Charles Bonnycastle, Dr. Robley Dunglison, and other professors introduced from Europe, Jefferson's plan for an elective system and for real university work in schools of language and science, was practically realized more than fifty years ago. The publication of the facts regarding Jefferson's unique creation, in the vicinity of his own home at Monticello, has proved not only of historical interest but of positive educational value.

The work of organized inquiry into the history of American higher education by States and groups of States was demonstrated by experience to be practicable, and, by general encouragement, to be welcome to all parts of the country. Certain general principles were adopted in the further prosecution of the investigation. Under the direction of an editor, aided by the resources and documentary collections of the Bureau of Education, the preparation of the State monographs was assigned to representative and scholarly men from the State or section of country especially concerned. In all cases the active co-operation and assistance of the various higher institutions of learning in each State were enlisted through a sub-editor. An attempt was made to make the reports at once compact and readable, with a good analysis of contents and a few attractive illustrations of college or university buildings, the plates being by no means confined to the showing of externals, such as dormitories and façades, but picturing also in many cases library and laboratory interiors. In compensation for the lack of absolute historical completeness, full bibliographies of the sources of information were to be appended to each chapter or great subject, so that future students of our educational history might profit by the way-marks left by pioneers.

Although unaided by any special appropriations, and absolutely dependent upon the slender resources of the bureau for the preparation of circulars of information, the work has been extended from Virginia, the oldest of American commonwealths, throughout all the Southern States, where monographs are either completed, or well advanced. The report on North Carolina has lately been published. The returns from South Carolina, Georgia, and Florida are already in the hands of the Government printer. The work has not been restricted to the South. In anticipation of the historical interest connected with the observance of the centenary of the settlement of the old northwest territory organized inquiry was early extended beyond the Ohio River. A monograph upon the History of Higher Education in Wisconsin, prepared by Mr. David Spencer, under the general direction of Prof. William F. Allen, of the University of Wisconsin, has been accepted and sent to the Public Printer. At a meeting of the American Historical Association, held in the National Museum during the Christmas holidays, an introductory paper upon the whole subject of higher education in the Northwest was read by Prof. George W. Knight, of the Ohio State University at Columbus, a graduate of the University of Michigan and author of a scholarly monograph upon Federal Land Grants for Education in the Northwest Territory, published among the papers of the American Historical Association, vol. I. More elaborate monographs, based upon pioneer work in a vast and unknown field, and representing the history of colleges and universities in the States of Ohio, Illinois, and Michigan, will soon be completed.

Here upon this desk lies a manuscript History of Higher Education in the State of Indiana, mother of the President recently inaugurated. This history was prepared by Prof. J. A. Woodburn, of the State University at Bloomington, who has studied for two years in Baltimore. While no brief description can do justice to an exhaustive and laborious work, it may be summarized as representing—

(1) The services of the old Continental Congress and of the Federal Government towards education in the old Northwest Territory.

(2) The early beginnings of higher education in the Territory of Indiana and the rise of State seminaries and academies, with their growth into the State University.

(3) The work of the State normal school and of the various polytechnic and industrial institutions in the State of Indiana.

(4) The organization and early history of the denominational colleges and of other institutions of learning in Indiana.

(5) The development of the school system and the final union or articulation of the same with the colleges and the university.

Work of this kind has been pushed not only throughout the Northwest but through the Southwest. It has been carried beyond the Mississippi River, to the Pacific slope. The leading idea has been to do pioneer work in the West and South, where almost nothing has been hitherto accomplished towards a systematic history of colleges and universities. But the older sections of country have not been left out of consideration. State monographs are in preparation or contemplation in all of the New England States, New York, New Jersey, Pennsylvania, Delaware, Maryland, and the District of Columbia. Everywhere the attempt has been made to secure the co-operation of good men and scholarly investigators, with proper historical training for the work entrusted to their hands, and with a scientific spirit rising above all considerations of sectional, or sectarian, or economic interest. In all cases the work has virtually been a labor of love. The funds available for this wide-reaching and important undertaking have barely sufficed to pay expenses actually incurred, to say nothing of properly compensating local contributors for their time and painstaking research.

An illustration of the practical value to the whole country of investigations like these, lies upon the desk before you. Here is an elaborate monograph, which has occupied many months of patient toil, on the History of Federal and State Aid to Higher Education. The work was done by Dr. F. W. Blackmar, for several years a professor in a California college and now professor of history in the University of Kansas. Some of the State superintendents of education here present have doubtless received numerous inquiries from Dr. Blackmar, who for the past three years has been studying in Baltimore. When this monograph begins to come forth in proof from that tomb of manuscripts, the Government Printing Office, some of you will probably be asked to look over the portions concerning your own State, as Dr. Dickinson has already done for Massachusetts, Mr. Hine for Connecticut, Dr. Murray for New York, etc.

Without anticipating the interesting facts and figures contained in this important monograph, (facts which have been kept back from individual applicants for information until results could be published to the public at large,) it may be said that the work describes:

(1) The attitude of every American colony and State in this Union towards the higher education, considered from an historical point of view.

(2) The chief legislative enactments by colonial courts and State legislatures concerning the establishment and encouragement of higher education.

(3) The history of all financial aid and support given to higher education by every colony and State in this country. The author has found out, from a laborious examination of original statutes, the actual amounts of money appropriated and of lands granted for education by

each of the several States and by the Federal Government, throughout our entire history. There is not a practical educator, college president, or trustee in the land who will not appreciate the importance and utility of such a financial history of higher education in America.

(4) The monograph further shows the progress, development, and present tendencies of higher education in these United States. The history is given of West Point Military Academy, of the Naval Academy at Annapolis, of the Congressional Library, of the Smithsonian Institution, the National Museum, and of the United States Bureau of Education, together with all the financial relations of the General Government towards science and education since the beginning of our life as a nation.

These matters are here communicated to the assembled superintendents of education from all parts of the Union because it is important that you should appreciate their scope and significance, and because you are in a position to strengthen and uphold the highest work of the Bureau of Education. The bureau was originally founded, in the year 1867, "for the purpose of collecting such statistics and facts as shall show the condition and progress of education in the several States and Territories" (Barnard's First annual Report, 1867-'68, p. 63. Garfield's speech). What better method could there possibly be of showing the condition or progress of education in these United States than by an historical review of the origin, growth, development, and present tendencies of American institutions of learning, beginning with the highest, as did our forefathers, with colleges and universities, and gradually enlarging the horizon of inquiry until the whole field of secondary and common school education is embraced in the retrospect? The broadening plains are best seen from the hill-tops. Unless American educators see to it that the higher education is properly recognized in our State and National reports, our whole system of educational inquiry will degenerate into common school statistics and essays on pedagogical methods. The Bureau of Education ought to take a commanding place in the educational work of the country. By the highest kind of original investigations, at home and abroad, it ought to win the respect and confidence of the best men engaged in educational work, whether college presidents or superintendents of schools. Why is it that the interests of labor and agriculture can be raised to the dignity of Departments in the United States Government, with a Secretary of Agriculture holding a Cabinet office, while the educational interests of the Republic are allowed to remain upon a lower level? Simply because the educators of the country are content with that level, because they do not exert one-half the compelling energy of either the farmers or the labor-unions. The Bureau of Education ought to become a ministry of public instruction, with a recognized place in the Cabinet, and with a constantly energizing influence proceeding from the capital of this country throughout the length and breadth of the land, stimulating the colleges and the universities, as well as the school systems of the whole country, by publishing the results of organized inquiry. The present Commissioner of Labor touches the vital interests of American labor and of all American society by his reports on the condition of working classes and on the statistics of divorce. The bureau can attain an honorable and influential position in the educational life of the country only by keeping the vantage-ground already gained, by pursuing higher lines of activity, by pressing boldly forward for larger appropriations and higher objects, and by enlisting the cordial support of the best friends of education throughout all these States. Thus gradually the pressure of public opinion will be brought to bear upon Congressmen, and Congress and the nation will recognize at last that the interests of public education are quite as important to the entire American people as are the interests of any one class, like our American farmers or our American workingmen, however honorable the aims of both classes may be.

Strengthen all existing foundations of the higher education in America, whether in the individual States or at Washington. Bring the representatives of public school systems and of our American colleges and universities into more hearty and efficient alliance. Co-operate with every respectable agency for the higher education of the American people, whether by summer schools, teachers' institutes, the distribution of good literature in popular form, or by the institution of home reading circles and university extension lectures, now so popular in the manufacturing towns and mining districts of England. Break down the antagonism between mental and manual labor. Make industrial and technical education as honorable as classical culture and the learned professions. Teach the science of government and social science, European as well as American history, in the public school. Then shall we all have greater respect and toleration for our fellowmen; then will all begin to appreciate the necessity of supporting all forms of education, even the highest, by the combined efforts of society and the State. A noble popularity must be given to science and art in America. The people of every State should be led to see that the higher learning is not for the benefit of a favored few, but that it is beneficial and accessible to the sons of citizens, of whatever station. In the proper co-ordination of the common school system with the high school and university, the Western States are leading this Republic to a more thoroughly democratic state of society, with fewer artificial distinctions of culture, with more of the spirit of human brotherhood than the world has hitherto seen. The Eastern colleges and universities will continue to train professors and to develop science, but the West and South will apply both men and ideas to democratic uses. The whole country needs this popularization of culture. With universal suffrage and the sovereignity of the people at the basis of our political life, popular intelligence must be cultivated so that it may be both able and willing to hold fast all that is good in human history, not only civil and religious liberty, but all that makes for happiness and righteousness in a great nation.

THE MOLECULAR STRUCTURE OF MATTER.*

By William Anderson.

Five years ago, at Montreal, in his address to the Mathematical Section, Sir William Thomson took for his subject the ultimate constitution of matter, and discussed in a most suggestive manner the very structure of the ultimate atoms or molecules. He passed in review the theories extant on the subject, and pointed out the progress which had been made in recent years by the labors of Clausius, of Clerk Maxwell, of Tait, and others,—among whom his own name (I may add) stands in unrivalled prominence. I will not presume to enter the field of scientific thought and speculation traversed by Sir William Thomson. I propose to draw attention only to some general considerations, and to point out to what extent they practically interest the members of this Section.

In a lecture delivered at the Royal Institution last May, Professor Mendeléeff attempted to show that there existed an analogy between the constitution of the stellar universe and that of matter as we know it on the surface of the earth, and that from the motions of the heavenly bodies down to minutest inter-atomic movements in chemical reactions, the third law of Newton held good, and that the application of that law afforded a means of explaining those chemical substitutions and isomerisms which are so characteristic, especially of organic chemistry. Examined from a sufficient distance, the planetary system would appear as a concrete whole, endowed with invisible internal motions, travelling to a distant goal. Taken in detail, each member of the system may be involved in movements connected with its satellites, and again each planet and satellite is instinct with motions which, there is good reason to believe, extend to the ultimate atoms, and may even exist, as Sir William Thomson has suggested, in the atoms themselves. The total result is complete equilibrium, and, in many cases, a seeming absence of all motion, which is, in reality, the consequence of dynamic equilibrium, and not the repose of immobility or inertness.

The movements of the members of the stellar universe are many of them visible to the naked eye, and their existence needs no demonstration; but the extension of the generalization just mentioned to substances lying (to all appearances) inert on the earth's surface is not so obvious. In the case of gases, indeed, it is almost self-evident that they are composed of particles so minute as to be invisible,—in a condition of great individual freedom. The rapid penetration of odors to great distances, the ready absorption of vapors and other gases, and the phenomena connected with diffusion, compression, and expansion seem to demonstrate this. One gas will rapidly penetrate another and blend evenly with it, even if the specific gravities be very different. The particles of gases are (as compared with their own diameters) separated widely from each other; there is plenty of room for additional particles; hence any gas which would, by virtue of its molecular motion, soon diffuse itself uniformly through a vacuum will also diffuse itself through one or more other gases, and once so diffused, it will never separate again. A notable example of this is the permanence of the constitution of the atmosphere, which is a mere mixture of gases. The oxygen and the nitrogen, as determined by the examination of samples collected all over the world, maintain sensibly the same relative proportions, and even the carbonic acid, though liable to slight local accumulations, preserves, on the whole, a constant ratio, and yet the densities of these gases differ very greatly.

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* Presidential address before the Mechanical Science Section of the British Association A. S., at Newcastle, September, 1889. (*Report of British Association*, vol. LIX, pp. 718–732.)

Liquids (though to a much less degree than gases) are also composed of particles separated to a considerable relative distance from each other, and capable of unlimited motion where no opposing force—such as gravity—interferes; for under such circumstances their energy of motion is not sufficient to overcome the downward attractions of the earth; hence they are constrained to maintain a level surface. The occlusion of gases without sensible comparative increase of volume shows that the component particles are widely separated. Water (for example) at the freezing point occludes above one and three quarter times its own volume of carbonic oxide, and about 480 times its volume of hydrochloric acid, with an increase of volume in the latter case of only one-third. The quantity of gas occluded increases directly as the pressure, which seems to indicate that the particles of the occluded gas are as free in their movements among the particles of the liquid as they would be in an otherwise empty containing vessel. Liquids therefore are porous bodies whose constituent particles have great freedom of motion. It is no wonder consequently that two dissimilar liquids, placed in contact with each other, should interpenetrate one another completely, if time enough be allowed; and this time, as might be expected, is considerably greater than that required for the blending of gases, because of the vastly greater mobility of the particles of the latter. The diffusion of liquids takes place not only when they are in actual contact, but even when they are separated by partitions of a porous nature, such as plaster of Paris, unglazed earthenware, vegetable or animal membranes, and colloidal substances, all of which may be perfectly water-tight, in the ordinary sense of the term, but powerless to prevent the particles of liquids making their way through simultaneously in both directions.

When we come to solid substances the same phenomena appear. The volumes of solids do not differ greatly from the volumes of the liquids from which they are congealed, and the solid volumes are generally greater. The volume of ice (for example) is one tenth greater than the water from which it separates. Solid cast-iron just floats on liquid iron, and most metals behave in the same way; consequently, if the liquids be porous the solids formed from them must be so also; hence, as might be expected, solids also occlude gases in a remarkable manner. Platinum will take up five and a half times its own volume of hydrogen, palladium nearly 700 times; copper, 60 per cent.; gold, 29 per cent.; silver, 21 per cent. of hydrogen, and 75 per cent. of oxygen; iron from 8 to 12½ times its volume of a gaseous mixture chiefly composed of carbonic oxide. Not only are gases occluded, but they are also transpired under favorable conditions of temperature and pressure, and even liquids can make their way through. Red-hot iron tubes will permit the passage of gases through their substance with great readiness. Ordinary coal gas—when under high pressure—is retained with difficulty in steel vessels, and it is well known that mercury will penetrate tin and other metals with great rapidity, completely altering their structure, their properties, and even their chemical compositions.

The evidence of the mobility of the atoms or molecules of solid bodies is overwhelming. Substances when reduced to powder, may even at ordinary temperatures be restored to the homogeneous solid condition by pressure only. Thus Professor W. Spring some ten years ago produced from the powdered nitrates of potassium and sodium—under a pressure of thirteen tons to the square inch—homogeneous transparent masses of slightly greater specific gravity than the original crystals, but not otherwise to be distinguished from them. More than that, from a mixture of copper filings and sulphur, he produced—under a pressure of thirty-four tons per square inch—perfectly homogeneous cuprous sulphide ($Cu_2 S$), the atoms of the two elements having been brought by pressure into so intimate a relation to each other, that they were able to arrange themselves into molecules of definite proportion; and still more remarkable, the carefully dried powders of potash, saltpeter, and acetate of soda, were by pressure caused to exchange their metallic bases, and form nitrate of soda and acetate of potash.

At high temperatures the effects are more easily produced on account of the greater energy of motion of the atoms or molecules. In the process of the manufacture of steel by cementation, or in case-hardening, the mere contact of iron with solid substances rich in carbon is sufficient to permit the latter to work its way into the heart of the former, while in the formation of malleable cast-iron the carbon makes its way out of the castings with equal facility; it is a complete case of diffusion of solid substances through each other, but on account of the inferior and restricted mobility of the particles at ordinary temperatures, a higher degree and longer time are needed than with liquids or gases. Again, when by the agency of heat, molecular motion is raised to a pitch at which incipient fluidity is obtained, the particles of two pieces will unite into a homogeneous whole, and we can thus grasp the full meaning of the operation known as "welding." By the ordinary coarse methods but few substances unite in this way, because the nature of the operation prevents, or at any rate hinders, the actual contact of the two substances; but when molecular motion is excited to the proper degree by a current of electricity, the faces to be joined can be brought into actual contact, the presence of foreign substances can be excluded, and many metals not hitherto considered weldable, such as tool steel, copper, and aluminium, are readily welded.

The movement which we term radiant heat, acting through the instrumentality of the luminiferous æther (which is believed on the strongest grounds to pervade all space and all matter) is competent to augment the quantity of movement in the particles of substances, and generally to cause an enlargement of volume. Again, energy in the form of light operates changes in the surface of bodies, causing colors to fade, and giving to photography the marvelous power which it possesses; decomposing the carbonic acid of the atmosphere in the chlorophyl of green leaves, and determining chemical combinations, such as chlorine with hydrogen to form hydrochloric acid, or carbonic oxide with chlorine to form chloro-carbonic acid. It is inconceivable that these effects could be produced unless the undulations of light were competent to modify the molecular motions already existing in the solid, liquid, and gaseous bodies affected.

Electricity exerts a similar influence. Generated by the molecular movements caused by chemical activity, whether directly, as in the primary battery, or indirectly, as in the dynamo, it is competent to increase the molecular movements in bodies so as to produce the effects of heat directly applied; it is capable of setting up motions of such intensity as to produce chemical changes and decompositions, to say nothing of the whole series of phenomena connected with magnetism, with induction, or the action through space and through non-conducting bodies, which, as in the case of radiant heat and light, seems to imply the existence of an interatomic ether. Conversely, changes of molecular equilibrium, brought about by the action of external forces, produce corresponding changes in electrical currents; witness the effects of heat, for example, on conductivity, and the wondrous revelations of molecular change obtained by the aid of Professor Hughes's induction balance.

The behavior of explosives illustrates also, and in a striking manner, the effects of disturbing molecular equilibrium. An explosive is a substance which contains in itself, in a solid or liquid form, all the elements necessary to produce a chemical change by which it is converted into the gaseous state. The application of heat, of pressure, or of impact, causes chemical union to take place, first at the spot where the equilibrium is disturbed by the application of external force, and afterwards, with great rapidity, through the mass, the disturbance being propagated either by the air surrounding the particles or by the luminiferous æther, with all the rapidity of light; the chemical re-action is accelerated by the pressure which may arise, for example, if the explosive be confined in the chamber of a gun or in the bore-hole of a blast. High explosives (as they are termed) are comparatively inert to ordinary ignition; but when the molecular equilibrium is suddenly disarranged throughout the mass by the detonation of a percussion fuse, combination takes place instantly throughout, and violent explosion follows. In a similar manner some gases, such a acetylene, cyanogen, and others, can be decomposed by detonation and reduced to their solid constituents. Professor Thorpe has devised a very beautiful lecture experiment, in which carbon disulphide is caused to fall asunder into carbon and sulphur by the detonation of fulminate of mercury fired by an electric spark. In these cases a reverse action takes place, but it illustrates equally well the conversion of one form of energy into others, and the consequent disturbance of molecular equilibrium in the substances affected. It seems to me clear therefore the time has come when the conception of dynamic equilibrium in the ultimate particles of matter in all its forms must take the place of the structural system of inert particles.

I cannot conceive how the phenomena which I have enumerated can be explained on the supposition that matter is built up of motionless particles;—how for example a stack of red and yellow bricks could ever change the order of arrangement without being completely pulled asunder and built up again, in which case an intermediate state of chaos would exist: but I can easily comprehend how a dense crowd of people may appear as a compact mass, streaming it may be in a definite direction, and yet how each member of that mass is endowed with limited motion, by virtue of which he may push his way through without disturbing the general appearance; how the junction of two crowds would form one whole, though perchance altered in character; and how even Professor Spring's experiments may be explained by the supposition that bystanders on the edge of a crowd would be forced, by external pressure, to form part of it and partake of its general movements.

When it is conceded that molecular motion pervades matter in all its forms, and that the solid passes (often insensibly) into the fluid, or even direct into the gaseous, it follows, almost of necessity, that there must be a borderland, the limits of which are determined by temperature and pressure, in which substances are constantly changing from one state to another. This is observable in fusion, but to a marked degree in evaporation, where the particles are being incessantly launched into space as gas and return as constantly to the liquid state.

If steel be looked upon as a solution of carbon and iron, then the hardening of steel is explained by the theory that dissociation has taken place at the temperature at which it is suddenly cooled, the sudden cooling fixing the molecular motion at such an amplitude or phase that it gives a characteristic structure, one of the properties of which is extreme hardness. In tempering, the gradual communication of heat causes dissociation again to take place, the molecular equilibrium is modified by the increased energy imparted to the particles, and when suddenly cooled at any point there remains again a distinct substance, composed of iron and carbon, partly in various degrees of solution and partly free, and again possessing special mechanical qualities. . . .

There is one more circumstance connected with my subject to which I must draw your attention, because, though its application to the mechanical properties of substances is very recent, it promises to be of great importance. I allude to the Periodic Law of Dr. Mendeléef According to that law, the elements arranged in order of their atomic weights, exhibit an evident periodicity of properties, and as Professor Carnelley has observed, the properties of the compounds of the elements are a periodic function of the atomic weights of their constituent elements. Acting on these views, Professor Roberts-Austin has recently devoted much time and labor to testing their exactness with reference to the mechanical properties of metals. The investigation is surrounded by extraordinary difficulties, because one of the essential features of the inquiry is that the metals operated on should be absolutely pure. For chemical researches, a few grains of a substance are all that is needed, and the requisite purity can be obtained at a moderate cost of time and labor; but when mechanical properties have to be determined, considerable masses are needed, and the funds necessary for obtaining these are beyond the reach of most private individuals.

In view of the difficulty of obtaining metals of sufficient purity, he selected gold as his base, because that metal can be more readily brought to a state of purity than any other, and is not liable to oxidation. In a communication to the Royal Society made last year, he shows that the metals alloyed with gold which diminish its tenacity and extensibility have high atomic volumes, while those which increase these properties have either the same atomic volumes as gold or have lower ones. The inquiry has only just been commenced, but it appears to me to promise results which, to the engineer, will prove as important and as fruitful of progress as the great generalization of Mendeléef has been to chemists. A law which can not only indicate the existence of unknown elements, but which can also define their properties before they are discovered, if capable of application to metallurgy, must surely yield most valuable results, and will make the compounding of alloys a scientific process instead of the lawless and hap-hazard operation which it is now.

The practical importance of the views I have ennunciated are I think sufficiently obvious. Every one will admit that an external force can not be applied to a system in motion without affecting that motion; consequently matter, in whatever state, can not be touched without changes taking place which will be more or less permanent. The application of heat will cause a change of volume, and at last, a change of condition; the application of external stresses will also produce a change of volume; and it is natural to infer that there must be some relation between the two, and accordingly Professor Carnelley has drawn attention to the fact that the most tenacious metals have high melting-points, though here again there is a great want of exactness, partly on account of the difficulty of measuring high temperatures, and partly by reason of the scarcity of pure materials. Again, long-continued stresses, or stresses frequently applied, may be expected to produce permanent changes of form, and so we arrive at what is termed the fatigue of substances. Stretched beyond their elastic limits, (which limits I do not suppose to exist except when stresses are applied quickly,) substances are permanently deformed, and the same effects follow the long application of heat.

The constant recurrence of stresses, even those within the elastic limit, causes changes in the arrangement of the particles of substances which slowly alter the properties of the latter, and in this way pieces of machinery, which theoretically were abundantly strong for the work they had to perform, have failed after a more or less extended period of use. The effect is intensified if the stresses are applied suddenly, if they reach nearly to the elastic limit, and if they are imposed in two or more directions at once, for then the molecular disturbance becomes very intense, the internal equilibrium is upset, and a tendency to rupture follows. Such cases occur in artillery, in armour-plates, in the parts of machinery subject to impact; and, as might be expected, the destructive effects do not always appear at once, but often after long periods of time. When considerable masses of metal have to be manipulated by forging, or by pressure in a heated condition, the subsequent cooling of the mass imposes restrictions on the free movement of some, if not all of the particles; internal stresses are developed which slowly assert themselves, and often cause unexpected failures. In the manufacture of dies for coining purposes, of chilled rollers, of shot and shell hardened in an unequal manner, spontaneous fractures take place without any apparent cause, and often after long delay, the reason being that the constrained molecular motion of the inner particles gradually extends the motion of the outer ones until a solution of continuity is caused. Similar stresses occur in such masses as crank shafts, screw shafts, gun hoops, etc. - - -

The influence of time on steel seems to be well established; the highest qualities of tool steel are kept in stock for a considerable period;

and it seems certain that bayonets, swords, and guns are liable to changes which may account for some of the unsatisfactory results which have manifested themselves at tests repeated after a considerable interval of time. As all these things have been hardened and tempered, there must necessarily have been considerable constraint put upon the freedom of motion of the particles. This constraint has gradually been overcome, but at the expense of the particular quality of the steel which it was originally intended to secure.

I have now laid before you the views respecting the constitution of matter which I think are gaining ground, which explain many phenomena with which we are familiar, and which will serve as guides in our treatment of metals, and especially of alloys; but I must admit that the subject is still by no means clear, that a great deal more definition is wanted, and that we are still awaiting the advent of the man who shall do for molecular physics what Newton did for astronomy in explaining the structure of the universe.

PETROLEUM.

One of the most remarkable features of the last thirty years is the introduction of petroleum, and the wonderful development to which the trade in it has attained. Under the generic name of petroleum is embraced a variety of combinations of carbon and hydrogen, each of which is distinguished by some special property. At ordinary temperatures and pressures some are gaseous, some are liquid, and some solid, and most are capable of being modified by suitable treatment under various temperatures and pressures. The employment of petroleum in the arts is still extending rapidly. Used originally for illuminating purposes, it is now employed as fuel for heating furnaces and steam-boilers, and as a working agent in heat engines: valuable medicinal properties have been discovered; and as a lubricant it stands unrivalled.

As a working agent in heat engines it is employed in two ways: First, as a vapor generated from the liquid petroleum contained in a boiler, very much in the same way as the vapor of water is used in an engine with surface condenser, the fuel for producing the vapor being also petroleum. Very signal success has been obtained by Mr. Yarrow and others in this mode of using mineral oil, especially for marine purposes and for engines of small power; there seems to be no doubt that by using a highly volatile spirit in the boiler a given amount of fuel will produce double the power obtainable by other means, and at the same time the machinery will be lighter and will occupy less space than if steam were the agent used. The other method is to inject a very fine spray of hot oil associated with the proper quantity of air into the cylinder of an ordinary gas-engine, and ignite it there by means of an electric spark or other suitable means. Attempts to use oil in this way date back many years, but it was not till 1888, that Messrs. Priestman Brothers exhibited at the Nottingham show of the Royal Agricultural Society an engine which worked successfully with oil, the flashing point of which was higher than 75° F., and was therefore within the category of safe oils. The engine exhibited was very like an ordinary Otto gas-engine, and worked in exactly the same cycle. A pump at the side of the engine forced air into a small receiver at a few pounds pressure to the square inch. The compressed air, acting by means of a small injector, carried with it the oil in the form of fine spray, which issued into a jacketed chamber heated by the exhaust, in which the oil was vaporized. The mingled air and oil was thus raised to a temperature of about 300°, and was then drawn, with more air, into the cylinder, where, after being compressed by the return stroke of the piston, it was exploded by an electric spark, and at the end of the cycle the products of combustion were discharged into the air after encircling the spray chamber and parting with most of their heat to the injected oil. The results of careful experiments made by Sir William Thomson and myself on different occasions were, that 1.73 pounds of petroleum were consumed per brake horse-power per hour; but the combustion was by no means perfect, for a sheet of paper held over the exhaust pipe was soon thickly spattered with spots of oil.

The enormous consumption of petroleum and of natural gases frequently raises the question as to the probability of the proximate exhaustion of the supply; and without doubt many fear to adopt the use of oil, from a feeling that if such use once becomes general the demand will exceed the production, the price will rise indefinitely, and old methods of illumination, and old forms of fuel, will have to be reverted to. From this point of view it is most interesting to inquire what are the probabilities of a continuous supply; and such an investigation leads at once to the question, "What is the origin of petroleum?"

In the year 1877, Professor Mendeléef undertook to answer this question; and as his theory appears to be very little known, I trust you will forgive me for laying a matter so interesting before you. Dr. Mendeléef commences his essay by the statement that some persons assume (without any special reason excepting perhaps its chemical composition), that naphtha, like coal, has a vegetable origin. He combats this hypothesis, and points out, in the first place, that naphtha must have been formed in the depths of the earth. It could not have been produced on the surface, because it would have evaporated; nor over a sea bottom, because it would have floated up and been dissipated by the same means. In the next place he shows that naphtha must have been formed beneath the very site on which it is found,—that it can not have come from a distance, like so many other geological deposits, and for the reasons given above, namely, that it could not be water-borne, and could not have flowed along the surface, while in the superficial sands in which it is generally found no one has ever discovered the presence of organized matter in sufficiently large masses to have served as a source for the enormous quantity of oil and gas yielded in some districts; and hence it is most probable that it has risen from much greater depths under the influence of its own gaseous pressure, or floated up upon the surface of water, with which it is so frequently associated.

The process of the formation of petroleum seems to be the following: It is generally admitted that the crust of the earth is very thin in comparison with the diameter of the latter, and that this crust incloses soft or fluid substance, among which the carbides of iron and of other metals find a place. When, in consequence of cooling or some other cause, a fissure takes place through which a mountain range is protruded, the crust of the earth is bent, and at the foot of the hills fissures are formed; or at any rate the continuity of the rocky layers is disturbed, and they are rendered more or less porous, so that surface waters are able to make their way deep into the bowels of the earth, and to reach occasionally the heated deposits of metallic carbides, which may exist either in a separated condition or blended with other matter. Under such circumstances it is easy to see what must take place. Iron, or whatever other metal may be present, forms an oxide with the oxygen of the water; hydrogen is either set free or combined with the carbon which was associated with the metal, and becomes a volatile substance—that is, naphtha. The water which had penetrated down to the incandescent mass was changed into steam, a portion of which found its way through the porous substances with which the fissures were filled, and carried with it the vapors of the newly-formed hydro-carbons, and this mixture of vapors was condensed wholly or in part as soon as it reached the cooler strata. The chemical composition of the hydro-carbons produced will depend upon the conditions of temperature and pressure under which they are formed. It is obvious that these may vary between very wide limits, and hence it is that mineral oils, mineral pitch, ozokerit, and similar products differ so greatly from each other in the relative proportions of hydrogen and carbon. I may mention that artificial petroleum has been frequently prepared by a process analogous to that described above.

It is needless to remark that Dr. Mendeléef's views are not shared by every competent authority; nevertheless, the remarkable permanence of oil-wells, the apparently inexhaustible evolution of hydro-carbon gases in certain regions, almost forces one to believe that the hydro-carbon products must be forming as fast as they are consumed, that there is little danger of the demand ever exceeding the supply, and that there is every prospect of oil being found in almost every portion of the surface of the earth, especially in the vicinity of great geological disturbances. Improved methods of boring wells will enable greater depths to be reached; and it should be remembered that, apart from the cost of sinking a deep well, there is no extra expense in working at great depths, because the oil generally rises to the surface or near it. The extraordinary pressures, amounting to 300 pounds per square inch, which have been measured in some wells, seem to me to yield conclusive evidence of the impermeability of the strata from under which the oil has been forced up, and tend to confirm the view that it must have been formed in regions far below any which could have contained organic remains.

ALUMINUM.*

By H. C. HOVEY.

The formal opening of the great works of the Aluminum Brass and Bronze Company, at Bridgeport, Connecticut, makes it desirable, as a preliminary, that we state a few facts about the unalloyed metal itself. Quite learned men have indulged in wild talk about the metal, which is more widely distributed over the globe than any other, being known to exist in two hundred different minerals, including all granites and common clays.

The problem has been to extract the metal cheaply, and chemists of every land have labored for a solution. Œrsted suggested a process of obtaining aluminum by treating the chloride with an alkali metal. Adopted by Woehler, and modified by Deville, the process was "a reduction of the double chloride of aluminum and sodium by means of metallic sodium in the presence of cryolite." It was thus that Deville was able to show at the Paris Exhibition in 1855, as the greatest of modern chemical wonders, a bar of what he styled "silver-white metal made from clay." He sold aluminum first at $15 an ounce, but in 1857 he reduced the price to $2 an ounce. Improvements cheapened the product still further, so that Colonel Frishmuth, who cast the tip of the Washington Monument in 1884, was able to furnish the metal in bars at $15 a pound. In that year however he made only 1,800 ounces, and the entire import was but 590 pounds.

Prior to 1887, the entire amount manufactured annually was but 10,000 pounds, and it sold that year at $10 a pound. To get even this small amount required the annual manufacture of 100,000 pounds of the double chloride and 40,000 pounds of sodium. To cheapen these two preliminary processes was essential to the cheap production of aluminum.

Hence the importance of the process patented by Mr. Hamilton Y. Castner, June 1, 1886, which was the first patent ever granted for an aluminum process in the United States. Its special feature was a cheap way of getting sodium. He reduced and distilled it in large iron crucibles, raised automatically through apertures in the bottom of the furnace, where they remain until the reduction is completed and the sodium distilled. Through tubes in stationary covers the distilled metal passes to condensers, where it is solidified. When the process is completed, the crucible is lowered and a new one with a fresh charge is substituted and raised into the furnace. The residues are carbonate of soda and metallic iron, both of which can again be utilized. The process is as simple as it is ingenious, and the temperature required is very moderate, the sodium distilling as eazily as zinc. One charge requires about an hour, and a battery of four furnaces can yield a ton of sodium a day. The metal is kept from oxidation by a covering of mineral oil till used.

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* From the *Scientific American*.

The Deville-Castner process takes the double chloride finely divided and mixed with thin slices of sodium, and empties the mixing cylinder on the hearth of a reverberatory furnace, where the mass quickly melts, and a re-action takes place that finally liberates a silvery stream of molten aluminum, that is drawn out from below, while the melted slag runs off from above. The first run is purest and contains about three-fourths of the charge. The remainder is scraped off from the hearth, or found entangled with the slag, from which it has to be separated. The aluminum is finally re-melted in plumbago crucibles, and cast into ingots, bars, or plates.

The *Journal of the Society of Arts*, from whose very extended account the foregoing is abridged, adds that day by day, as the manufacture progresses, improvements are made which either enhance the economy of production or the purity of the product, and speaks in the highest praise of the skill, energy, and perseverance of Mr. Castner and his assistants, by whom, more than any others, aluminum has been brought into the market on commercially practicable terms and in a condition of almost perfect purity.

Graban's process may be briefly described. Powdered cryolite put into a solution of the sulphate of aluminum gives by re-action the fluoride of aluminum, which is then heated till ready to evaporate. The heated fluoride is pulverized and thrown upon melted sodium contained in a vessel lined with cryolite. The heat generated by the violent re-action melts the aluminum as well as the cryolite; and the molten mass being poured out, the pure aluminum settles at the bottom, while the cryolite is at the top. The main advantage of this method over the Castner process is that it goes on at a lower temperature and is extremely simple.

Numerous other processes are described by Richards in his exhaustive work on the subject; *e. g.*, reduction by cyanogen, by hydrogen, by carburetted hydrogen, by carbon and carbon-dioxide, concerning all of which Dr. T. Sterry Hunt remarks that "there has been no pure aluminum made commercially save from the chloride by the use of sodium." Webster is the chief manufacturer in England on his own patents, and large works have been erected in France on Bunsen and Deville's process by electrolysis.

But after all, the only true rival of the Castner-Deville process seems to be the Hall process, on patents of Charles M. Hall, and carried on by the Pittsburgh Reduction Company, who are now selling pure aluminum at a rate cheaper than nickel; and tons of metal are rolled by the Scoville Manufacturing Company, of Waterbury, into sheets, bars, rods, and tubing at a price less than German silver. Briefly, the Hall process is this: A flux being discovered that at a moderate temperature takes the aluminum ore into solution, and that is of lighter specific gravity, and that also is unaffected by the passage of an electric current, he fills a series of carbon-lined steel pots with the flux, which is kept in a melted condition. Carbon electrodes are plunged into these baths, through which passes the electric current, which acts to send the aluminum to the sides and bottom of each pot. The baths are constantly replenished with ore, and the process thus goes on for an indefinite period, night and day, at small cost, and demanding but little attention.

Aluminum, whether pure or in combination, deserves to rank with the noble metals;—although in certain forms it makes the basis of our common clay, every cubic yard of which is said to contain 800 pounds of the metal; in other forms it is massed in mountains; and in others still, it shines among the most precious stones, entering into the composition of the ruby, sapphire, topaz, garnet, lapis-lazuli, and tourmaline.

Cryolite, found in Greenland, and beauxite, first found at Beaux, in France, but since in Austria, Ireland, and elsewhere, are the ores relied on for the manufacture of aluminum. Cryolite is a snow-white mineral, though often tinged red or yellow by impurities. Beauxite is a hard white clay, occurring in beds many feet thick. Corundum, found in Georgia, is the material relied on in America especially for making the alloys. It varies from dull blue to black, and exists in massive form, as well as in crystals. The cost at the factory of these different minerals varies from $60 to $140 a ton.

The properties of aluminum are now generally known. Its color is white delicately tinged with blue, and it resembles silver more than any other metal. It takes a brilliant polish, and may be rolled or forged as easily as gold or silver, and may be beaten into very thin leaves. It can be pressed or stamped into all sorts of shapes, or drawn into very fine wire. Its elasticity and tenacity are about the same as virgin silver, but change greatly under the hammer. It is said to resist the graving-tool till properly varnished, when it may be cut like copper. Its sonorousness is very curious. Cast in bell form its sound is sharp, and not prolonged; but struck as a bar, it is remarkably sweet, pure, and resonant. Its sound is resolved into two tones, related to each other as are D and A. For a musieal instrument, fine effects might be had from a series of chromatic bars.

In estimating the relative cost of aluminum as compared with other metals, we must take its specific gravity into the account. A bar of aluminum weighing 1 pound would be about four times as large as a similar bar of silver, brass, bronze, tin, or iron. Hence, at an equal price, aluminum would be four times as cheap as silver, but as it now costs by weight only one-eighth as much, it must be relatively about thirty-two times as cheap. In other words, the purchaser would find it economical to use aluminum in preference to silver for everything to which it is adapted. As a conductor of electricity it equals silver, and is eight times better than iron, and as a conductor of heat it excels any other metal known. Neither air nor water, hot or cold, affects it, and it resists all acids except hydrochloric. It slowly yields to a mixture of salt and vinegar with a result as harmless as clay itself. It does not seem to be affected by saliva, perspiration, or other animal agents. Hydrogen, nitrogen, sulphur, and carbon do not affect it, but it is rapidly attacked by chlorine, fluorine, iodine, and bromine. From the above observation aluminum does not seem to have an intimate analogy with any other known metal, though Richards and Woehler place it near to silicon and boron in the carbon series.

Aluminum melts slowly at about 700° C. (1292° F.), without a flux, and in an ordinary uncovered earthen crucible lined with carbon. The pieces of divided metal are first dipped in benzine to clean them, and if necessary, are treated with nitric acid and then put in the crucible little by little.

A cinder remains at the bottom of the crucible. The molten metal may be cast either in metallic molds or in very dry porous sand with numerous vents. Deville prefers a plumbago crucible without a lid, and exposes the red-hot metal for a long time to the open air to allow the exhalation of the acid fumes, after which the surface is skimmed without loss of metal. It is then cast into ingots. To get perfectly clean results this process is repeated three or four times. The pure metal thus obtained improves in color with using, while what is less pure tarnishes in time, though perhaps equally brilliant on first casting.

The Aluminum Company, with offices at 115 Cannon street, London, and works at Oldbury, near Birmingham, issued a price-list November 1, 1889, from which we quote aluminum, 99¼ to 99¾ per cent., purity guarantied, 15 shillings per pound; 98 to 99, 15 shillings per pound; 95 to 96, 12 shillings a pound.

The first article manufactured from pure aluminum was a rattle for the young Prince Imperial of France, in 1856, the sonorousness of which was much admired. It was next made into jewelry, medals, and inlaid work. Its extreme lightness led to its being used for sextants, eye-glasses, opera-glasses, and the tubes of telescopes. It has been found useful for the beams of balances, for delicate weights, and in the form of fine wire for embroidery. Culinary articles made from it were to be seen at the London exhibition in 1862, for which it seemed admirably adapted on account of its lightness and immunity from corrosion.

Experiments have been rapidly mutiplied of late, under the encouragement given by reason of the increased cheapness of the metal, and a promising field is surely opening for its employment for many ornamental and useful purposes. The processes of soldering, welding, veneering, gilding, and silvering aluminum are minutely described in Richards's work on the subject.

The aluminum industry is on a firm footing, both in Europe and America. There have sprung up two distinct lines of manufacture; the one a chemical process, and the other strictly metallurgical. The former produces pure aluminum, and continues to be a complicated process demanding skill and patience. The latter produces only the alloys of aluminum, and has been made extremely simple by certain methods not necessary to be here described.

ALLOYS OF ALUMINUM.*

By J. H. Dagger.

Deville's method, modified in detail, is still the chief of the chemical processes for the production of aluminum, and is dependent upon the cost of metallic sodium. The greatest value of aluminum is however in its alloys, and the successful application of the intense heat of the electric arc to their production on a commercial scale marks a departure in electro-metallurgy of which we can not overestimate the importance, rendering it possible to produce rich alloys of this metal at half the cost of any other method, and so widening the field of their application to an extent hitherto unknown.

At the works of the Cowles Company, Lockport, New York, there are in operation fourteen furnaces, the electricity for which is generated by three dynamos, capable of supplying a current of 3,000 to 3,200 ampères, and E. M. F. of 55 to 60 volts. These furnaces can produce 2,500 pounds of aluminum bronze (10 per cent.) and 1,800 pounds of ferro-aluminum (10 per cent.), or a total yield of 430 pounds of contained aluminum per twenty-four hours. The English works of the company at Milton, Staffordshire, contain twelve furnaces with a 500-horse power dynamo, built by Messrs. Crompton, and said to be the largest machine in England and probably in the world; it furnishes a current of 5,000 to 6,000 ampères, with an E. M. F. of 50 to 60 volts. The production of these works is 2,300 pounds aluminum bronze (10 per cent.) and 1,800 pounds ferro-aluminum (10 per cent.) per twenty-four hours, or 410 pounds of contained aluminum.

The furnaces are rectangular in form and are of fire-brick; into each end is built a cast-iron tube, through which the carbon electrodes

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*Abstract of a paper read before the Chemical Section of the British Association, A. S., at Newcastle, September, 1889. (*Report of British Association*, vol. LIX, pp. 538–540.)

enter the furnace; each electrode consists of a bundle of nine carbons, each 2¼ inches diameter, attached to a head of cast-iron for a ferro aluminum furnace and of cast copper for aluminum bronze or alloys containing copper. This head is secured to copper rods, or "leads," which can be readily connected with or disconnected from the flexible cables supplying the current. Each cable is secured to slides travelling on an omnibus bar of copper overhead, and so can be brought into position opposite the furnaces to be used. The electrodes are arranged so that it is possible by means of a handle and screw to advance or withdraw them from each other in the furnace.

The first furnaces were lined with charcoal, but it was found that the intense heat converted it into graphite, which, being a conductor, not only meant loss of power, but the destruction of the furnace walls. This difficulty has been overcome by soaking the charcoal in lime-water and carefully drying before use; each particle of charcoal is thus coated with an insulating shell of lime.

Lining the furnace is the first operation; the bottom of the trough is covered with a layer of prepared charcoal, the electrodes are arranged in the furnace, and a "former," a sheet-iron box without top or bottom, each end being arched to fit over the electrodes, is inserted; charcoal is then rammed into the space between it and the fire-brick walls. This done, the charge of ore, mixed with coarse charcoal and the metal to be alloyed with the aluminum, in form of turnings or granules, is placed inside the iron box, after which this is carefully withdrawn; the space between the electrodes is bridged by some broken pieces of carbon, the charge is covered with coarse charcoal, and the furnace closed by a heavy cast iron cover having a hole in the center for the escape of gases evolved during the reaction; the cover is luted so as to prevent the entrance of air.

The commencing current is about 3,000 ampères, and is gradually increased to 5,000 ampères; a "run" occupies about one and one-half hours. The furnace is allowed to cool; the next, ready charged, is connected with the cables so that the process is a continuous one, the furnaces being successively charged and connected. The crude metal from the furnace is then re-melted in an ordinary reverberating furnace, a sample being taken from each run and assayed for aluminum. The nature of the re-action that takes place in the electric furnace is not very easy to ascertain; the conditions are unlike those of any other process known. The reduction of the aluminum taking place in absence of air and in presence of an enormous excess of carbon, it may be assumed that at the intense heat of the electric arc, the ore melts and gives up its oxygen to the carbon:

$$Al_2O_3+3C=3CO+Al_2.$$

In the absence of copper, the liberated aluminum absorbs carbon and is converted into a carbide of the metal. The escaping gas which burns at the orifice in the cover is almost entirely composed of CO.

The most valuable of the alloys are those with copper. Aluminum bronze has great tensile strength. A bar containing 11 per cent. aluminum made by the electric furnace and tested by the Leeds Forge Company, limited, gave a tensile strain of 57.27 tons, or 128,400 pounds to the square inch. One, containing 7.5 per cent. aluminum, tested by Professor Unwin, broke under 36.78 tons = 89,743 pounds to the square inch. In resistance to compression this alloy equals the best steel; its transverse strength, or rigidity, is about forty times greater than ordinary brass. Its elastic limit is higher than that of mild steel, and it can be worked at a bright-red heat as easily as wrought iron. Its mechanical and physical properties render it useful for every variety of metal work, its high price only having hitherto restricted its use. Its enormous strength and anti-corrodible qualities recommend it as valuable above any other alloy for propeller blades, stern and rudder frames, and for hydraulic and engineering work generally. With above 11 per cent. the alloy becomes brittle; and at 20 per cent. can be powdered readily in a mortar. The addition of small quantities of aluminum lowers the fusing point of iron, and this is utilized in the "Mitis" castings. It insures freedom from blow-holes, increased tensile strength, and high elastic limit. Mr. Keep found that 0.1 per cent. aluminum raised the transverse breaking strength of a one-half inch bar, 12 inches long, from 379 pounds to 545 pounds, or 44 per cent., and the resistance to impact from 239 pounds to 254 pounds, or 6 per cent. The tensile strength of *Mitis* castings may be as high as 27 tons per square inch, with an elongation of 20 per cent. Another alloy made in the electric furnace is silicon bronze, which, owing to its great strength and tenacity, its resistance to corrosion, combined with high electrical conductivity, is perhaps the best metal extant for electric light, telephone, and telegraph wires.

THE EIFFEL TOWER.*

By G. Eiffel.

The notion of a tower 1,000 feet in height is not new. It has haunted the imagination of Englishmen and Americans. As early as 1833, the celebrated English engineer Trevitick proposed to construct a cast-iron tower 1,000 feet high, of which the diameter should be 100 feet at the base and 4 feet at the summit. But his project was never put in execution, and was but imperfectly worked out even on paper.

At the time of the Exhibition in Philadelphia, in 1876, the great American engineers, Messrs. Clarke and Reeves, brought forward a new project. Their tower was to consist of an iron cylinder 9 meters in diameter as a nucleus, and supported by a series of metal buttresses disposed round it and starting from a base with a diameter of 45 meters. This was a distinct improvement on the English project, although it still left room for criticism; and yet the Americans, in spite of their enterprising spirit and the national enthusiasm excited by this conception, shrank from its execution.

In 1881, M. Sébillot proposed to light Paris by an electric lamp placed at a height of 1,000 feet. This idea, which has, in my opinion, no practical value, had no better fate than its predecessors. I need only mention the designs, some in masonry, some in metal work and masonry combined, others, lastly, in wood, like the proposed tower for the Brussels Exhibition, which were produced at the same time as my own. But all these remained in the domain of fancy, proposals easy to frame but hard to execute. I come to the project which has been realized.

In 1885, after the studies which my engineers and I had occasion to make with regard to the lofty metal piers which support railway viaducts like that of Garabit, we were led to believe that it was possible to construct these without any great difficulty of a much greater height than any hitherto made which did not exceed 230 feet. We planned on these lines a great pier for a viaduct which should have a height of 395 feet and a base of 131 feet.

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* From the *New Review*. Copied in the *Eclectic Magazine*, Sept., 1889, Vol. L., pp. 355–359.

The result of these studies led me, with a view to the exhibition of 1889, to propose the erection of the tower, now completed, of which the first plans had been drawn out by two of my chief engineers, Messrs. Nouguier and Kœchlin, and by M. Sauvestre, an architect.

The fundamental idea of these pylons or great archways is based on a method of construction peculiar to me, of which the principal consists in giving to the edges of the pyramid a curve of such a nature that this pyramid shall be capable of resisting the force of the wind, without necessitating the junction of the edges by diagonals, as is usually done.

On this principal the tower was designed in the form of a pyramid, with four curved supports, isolated from each other and joined only by the platforms of the different stories. Higher up only, and where the four supports are sufficiently close to each other, the ordinary diagonals are used.

In June, 1886, a commission nominated by M. Lockroy, then minister of commerce and industry, finally accepted the plans I had submitted to it, and on January 8, 1887, the agreement with the State and the City of Paris was signed, fixing the conditions under which the tower was to be constructed.

It is needless to state that considerable energy and perseverance were required to attain this result, for there was much resistance to overcome, and my project had many opponents.

But I was sustained by the belief that what I proposed would contribute to the honor of our national industry and to the success of the exhibition, and it was not without a legitimate sense of satisfaction that I saw an army of navvies begin, on January 28, 1887, those excavations at the bottom of which were to rest the four feet of the tower which had never been out of my thoughts for the last two years.

I felt moreover—in spite of the violent attacks to which my project had been exposed—that public opinion was on my side, and that a crowd of unknown friends were ready to honor this bold enterprise as soon as it took form. The imagination of men was struck by the colossal dimensions of the edifice, especially in the matter of height.

The towers of Notre Dame de Paris reach a height of 217 feet; the Pantheon 260 feet; the dome of the Invalides, which is the highest monument in Paris, 344 feet; Strasburg Cathedral is 466 feet; the Great Pyramid of Egypt 479 feet; the Cathedral of Rouen rises 492 feet from the ground, and is only surpassed by Cologne Cathedral, which, lately completed, attains to 522 feet; but the Americans again outdid this by erecting at Washington an immense obelisk in masonry which reaches a height of 555 feet, and was constructed with immense difficulty.

Experience has shown however that masonry is not suitable for a construction of the kind. With iron, on the contrary,—of which the properties are so remarkable, since it may be as readily employed in tension as in compression, and can be put together perfectly by rivetting—the execution presented no insurmountable difficulties. Moreover, metal constructions can now be planned with such accuracy as to sanction the boldness which results from full knowledge.

Lastly, without any desire to flatter our national vanity, I may be allowed to say that French industry has held and still holds a high place in Europe in the art of building in iron.

Hence the material of which the tower was to be built was determined not only by the fact that it rendered construction possible, but also because it would supply a brilliant example of a modern industry in which France has been more especially distinguished since its introduction.

The base of the tower consists of four great piers, which bear the names of the four cardinal points. The first matter which offered itself for consideration was the question of the solidity of the foundation of these four piers. A series of borings showed that the subsoil in the Champ de Mars was composed of a deep stratum of clay capable of supporting a weight of between 45 pounds and 55 pounds to the square inch, surmounted by a layer of sand and gravel of varying depth, admirably calculated to receive the foundations. The actual position of the tower was determined by considerations relative to the depth of this stratum, since it was impossible to rest the piers directly on the clay. The foundation of each pier is now separated from the clay by a sufficient thickness of gravel.

Each of the main supports of the tower rests on blocks of masonry, and the masonry rests on beds of concrete which cover an area of 60 square meters. In the center of each pile of stone-work, are two great iron bars 25 feet 6 inches in length and 4 inches in diameter, which, by means of iron cramps, unite almost all parts of the masonry. This anchorage, which is not necessary to the stability of the tower—sufficiently assured by its own weight—gives nevertheless additional security, and has moreover been useful in the construction of the ironwork.

It will be seen from the foregoing description that the foundations are established under conditions of great security, and that in the choice of materials and in the dimensions ample margin has been allowed, so as to leave no room for doubt with regard to their solidity.

Nevertheless, to render perfectly certain that the feet of the tower should remain absolutely level in any event, we have made room, at the angles of the piers where they rest on the masonry, for hydraulic presses of 800 tons. By means of these presses each pier can be displaced and raised as much as is necessary by inserting steel wedges beneath it.

The raising into place of the iron-work which forms the upper part of the tower was accomplished by derricks and windlasses. As soon as the piers reached a height of 100 feet their inclination rendered scaffolding necessary to carry on the construction to a height of 169 feet, at which point are established the horizontal beams uniting the four piers and forming the skeleton of the first story. The solid construction of the first platform was a great step toward the success of the work.

The raising of the pillars between the first and second platforms was rapidly accomplished by the same method as that employed between the ground and the first story, *i. e.*, the pieces of iron were raised by four cranes attached to the beams of the lift placed in each pier.

The work went forward so rapidly that in July, 1888, the four pillars were united by the beams of the second story, at a height of 387 feet, and by the 14th of the month the second platform was fixed, on which fireworks were displayed at the Fête Nationale.

The erection of that part of the tower comprised between the second platform and the summit was carried out by means of the same cranes as had served for the lower part; but these no longer worked on an inclined plane, but were raised along an upright, formed by the central guide of the higher lifts.

The total weight of the ironwork in the tower is rather more than 7,000 tons, without counting that in the caissons, which form a portion of the foundations, or that in the machinery of the lifts.

The different parts of the tower are reached by staircases and lifts There are easy stairs in the east and west piers, which give access to the first story, and it is calculated that by using one for ascent and one for descent they will allow more than two thousand persons to go up and come down in the hour. From the first platform to the second there are four winding staircases, one in each pier, and from the second platform to the summit there is a single winding staircase, which however (unlike the others) is not intended for the use of visitors, but for officials only.

On the first platform is a covered gallery, with arcades, whence visitors can enjoy a view of Paris and its environs, as well as of the Exhibition, with four refreshment rooms in the center,—Anglo-American, Flemish, Russian, and French. On the second story is a second covered gallery; and in the center is the station where passengers change from the lifts which move on an inclined plane of the lower half of the tower, to the vertical lifts of the upper portion.

On the third story is a great saloon more than 50 feet square, shut in by glass on all sides, and whence, sheltered from wind and weather, the spectator can contemplate the magnificent panorama, 45 leagues in extent, which is displayed beneath him. Above this room are laboratories and observatories for scientific purposes, and in the center th[illegible] winding stair leading to the light-house whence the electric light sh over the whole of Paris.

The lifts are on three different systems, and all are provided w breaks, and otherwise insured against the possibility of serious accide

They are all worked by hydraulic power, and together are capable of conveying 2,350 persons in an hour to the first and second stories, and 750 to the summit, the whole ascent being effected in seven minutes. If we include the staircases it will be possible for 5,000 persons to visit the tower in the space of an hour.

The tower is now known to the whole world; it has struck the imagination of every nation, and inspired the most remote with the desire of visiting the Exhibition. The press of all countries confirms this statement, and I have myself received continual proofs of the universal curiosity and interest excited by the monument.

The visitors who go to the top of the tower have beneath their eyes a magnificent panorama. At their feet they see the great city, with its innumerable monuments, its avenues, its towers, and its domes; the Seine, which winds through it like a long ribbon of steel; farther off, the green circle of the hills which surround Paris; and beyond these, again, the wide horizon stretching 112 miles from north to south. At night the spectacle is no less beautiful. Paris with all its lights is like fairy-land, but in this aspect it has hitherto been known only to aeronauts, on whom its beauty has always made a strong impression. The construction of the tower will enable thousands to contemplate a spectacle of new and incomparable loveliness.

Then too for scientific and defensive purposes the gigantic monument will be of great utility. A recent writer, M. Max de Nansouty, says:

"In case of war or seige the movements of the enemy might be observed from the tower within a radius of 50 miles, and that above the heights which encircle Paris, and on which are constructed our new fortifications. Had we possessed the tower at the time of the seige of Paris, in 1870, with the powerful electric lights with which it will be furnished, who knows if the chances of the strife would not have been profoundly modified? The tower would be a means of constant and easy communication between Paris and the provinces by the aid of optical telegraphy, which has in various forms attained such a remarkable degree of perfection."

The tower is itself at such a distance from the fortifications that it is absolutely out of reach of the enemy's battery.

It will be moreover a wonderful meteorological observatory, whence the direction and the force of atmospheric currents can be usefully studied, from the point of view of science and hygiene, as well as the condition and the chemical composition of the atmosphere, the amount of electricity and moisture it contains, the variations of temperature at different heights, atmospherical polarization, etc. It is specially adapted for an astronomical observatory; for the purity of the air at this great height above the low-lying mists, which so often cloud the horizon of Paris, will allow of a number of observations often impossible in our climate.

I will not weary my readers with the enumeration of all the experiments to be made on the tower, of which a programme has been already drawn up by our scientific men, and which include the study of the fall of bodies through the air the resistance of the air to varying velocities, certain laws of elasticity, the study of the compression of gases of vapors under the pressure of an immense manometer of 400 atmospheres, a new realization on a great scale of Foucault's pendulum demonstrating the rotation of the earth, the deviation toward the East of a falling body, etc., etc.; lastly, a series of physiological experiments of the deepest interest.

I may even go so far as to say that there are few scientific men who do not hope at this moment to carry out, by the help of the tower, some experiment connected more especially with their own investigations.

Thus it will be an observatory and laboratory such as was never until now at the disposal of science; and from the first all our scientific men have encouraged me with their warmest sympathy. On my side, and in order to express in a striking manner that the monument which I have raised is dedicated to science, I decided to inscribe in letters of gold on the great frieze of the first platform, and in the place of honor, the names of the greatest men of science who have honored France, from 1789, down to our own day.

Besides all these uses, which I might have explained in greater detail, but which, even in this rapid summary, will serve to show that we have not erected an object of barren wonder, the tower possesses in my eyes a usefulness of a totally different order, which is the true source of the ardor which has inspired me in my work.

The public at large understood this, and it is also the reason of the very general and warm sympathy which has been displayed toward me.

My object was to show to the whole world that France is a great country, and that she is still capable of success where others have failed.

The *Scientific American* said, in 1874, with reference to the tower of Philadelphia, destined to celebrate the centenary of the national independence: "The character of the project is closely connected with the purpose of its erection; the hundredth anniversary of our national existence ought not to be allowed to pass without a permanent memorial, which an exhibition lasting a few months cannot furnish. It is evident that in the space of two years no monument of imposing aspect and original in conception can be constructed with other material than iron; from every point of view we could not choose a more national construction. We will celebrate our centenary by the most colossal iron construction that the world has seen."

Can we not apply to ourselves these words which, remaining a dead letter in America in 1874, have become for us in France a living reality?

May I be allowed to recall here a few words which I pronounced in inaugurating the first stage of the tower, and which sum up my ideas on the subject:

"The beginning was difficult, and criticism as passionate as it was premature was addressed to me. I faced the storm as best I could, thanks to the constant support of M. Lokroy, then Minister of Commerce and Industry, and I strove by the steady progress of the work to conciliate, if not the opinion of artists, at least that of engineers and scientific men. I desired to show, in spite of my personal insignificance, that France continued to hold a foremost place in the art of iron construction, in which from the earliest days her engineers have been more particularly distinguished, and by means of which they have covered Europe with the creations of their talent. Doubtless you are not ignorant that almost all the great engineering works of this nature, in Austria, Russia, Italy, Spain, and Portugal, are due to French engineers, and the traveller discovers with pride, as he passes through foreign countries, the traces of their activity and their science.

"The tower, 1,000 feet high, is before everything a striking manifestation of our national genius in one of its most modern developments; and this is one of the principal reasons for its existence. If I may judge by the interest which it inspires, abroad as well as at home, I have reason to believe that my efforts have not been unavailing, and that we may make known to the world that France continues to lead the world, that she is the first of the nations to realize an enterprise often attempted or dreamed of: for man has always sought to build high towers to manifest his power, but he soon recognized that the laws of gravity hampered him seriously, and that his means were very limited. It is owing to the progress of science, of the engineer's art, and of the iron industry, that we are enabled to surpass in this line the generations which have gone before us by the construction of this tower, which will be one of the characteristic feats of modern industry."

So it is that I have wished to raise to the glory of modern science, and for the more especial honor of French industry, a triumphal arch as striking as those which earlier generations have raised to honor conquerors.

THE EIFFEL TOWER.*

By William A. Eddy.

A tower about 1,000 feet in height was first thought of during the organization of the Centennial Exposition at Philadelphia, in 1876, and its possible construction was discussed in the newspapers at the time. But consultation with engineers and architects probably resulted in the conviction that the scheme was impracticable, and the expense beyond the value of the investment, especially if masonry were used. Aside from the question of outlay, a serious difficulty in the construction of any kind of material to such an altitude, there are questions of pressure and danger that daunt experienced engineers. M. G. Eiffel, constructor of some of the greatest works in France, notably the trestlework viaduct at Garabit, 407 feet high, concluded that the building of such a tower had not been attempted in ancient times, so far as known, because iron construction then lacked the lightness, strength, and adaptability seen in modern work. The enormous weight of masonry in so great a mass would not only imperil, by its tremendous pressure, the courses of stone near the ground, but would cause an irregular settling of the foundations, as in the well-known instance of the Leaning Tower of Pisa. In modern work, a pressure of 66 pounds for each square centimeter† is considered dangerous. It is admitted that 55 pounds in this proportion is too extreme for safety, although, owing to peculiarities of construction, this has been exceeded in some of the following instances cited by M. Navier:

	Pounds.
Pillars of the dome of the Invalides, Paris	32.55
Pillars of St. Peter's, Rome	36.08
Pillars of St. Paul's, London	42.70
Columns of St. Paul-hors-les-Murs, Rome	43.58
Pillars of the tower of St. Merri, Paris	64.85
Pillars of the dome of the Pantheon, Paris	64.94

M. Navier includes an estimate of 99.25 pounds for the church of La Toussant à Angers, which is in ruins, and so not a convincing example. It thus appears that the resistance in some daring structures is from 33 to 44 pounds, and only rises to nearly 65 in two instances. M. Eiffel cites the Washington Monument, which in its simplicity and boldness he considers remarkable. In M. Navier's estimates given for the greatest feats of architectural engineering in the Old World, this

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*From the *Atlantic Monthly*, June, 1889; vol. LXIII, pp. 721–727.

†A square centimeter is about two-fifths of an inch on a side.

huge obelisk stands high on the list of wonderful structures, the pressure at its base amounting to 58.35 pounds in the proportion above given. With the exception of the Eiffel tower, it is easily a bolder undertaking than any other of its kind known in the world, because it stands upon a relatively small base, with no side support, with a weight upon its foundations of 45,000 tons. This immense square shaft, about 55 feet on a side, served as an illustration of the danger in attempting to carry masonry to a greater height than before achieved. Fortunately, the foundation settled evenly, but to prevent probable demolition, part of the base was re-constructed and filled in with concrete. Meantime the structure began to lean to an extent that caused great uneasiness, and finally the suspension of the work. The construction was begun in 1848, and in 1854, when it reached a height of 152 feet, its dangerous condition became somewhat marked. Its original intended altitude of 600 feet was then reduced to 500. In 1880, after great difficulties, the base had been widened and the foundation enlarged and deepened. Work was then recommenced, and the masonry continued upward at the rate of about 100 feet yearly, until the topmost stone was laid December 6, 1884. The inauguration took place February 21, 1885.

An additional source of peril in the use of masonry, not included in the danger of settling, as in the Washington Monument, is the insufficient adherence of modern mortar to great masses of stone, causing serious crumbling, and a reputation for danger much to be dreaded. An attempt to extend stone work to a height of 1,000 feet would cause an expense too great for the end attained, and the danger of fracture would be incessant and unavoidable. It seems that we can excel the ancients very little in the treatment of masonry. There is no easily discovered evidence that they built any such structure higher than the great Pyramid of Cheops, originally 480 feet in height. They had good reasons for this caution. If the foundations are solid, the stone may disintegrate, owing to the unequal distribution of the enormous weight, due to the limited power of the mortar to act as a cushion to equalize the force. The Egyptian and other ancient builders constructed some masonry without mortar by polishing and closely fitting the stone, but it is not probable that they tried to carry such work to a very great height. In some modern buildings it is found that the resistance of very hard stone increases that of the mortar. Stone or brick work might reach a higher point than the Eiffel tower by the invention of cements more efficient than any now known.

In considering the important question of the foundations for this great tower, elaborate borings were made in the Champ-de-Mars at Paris. This is a level field or park, about two thirds of a mile long and half as broad, devoted usually to the drilling of troops and to reviews, upon which the Exposition buildings for 1889, are now approaching completion, in commemoration of the storming of the Bastile one hundred years ago, July 14 and 15, 1789, that memorable event of the French Revolution. It is intended to show the great advances in science, art, and industry, since that crude attempt to establish a republic.

In selecting this location near the river Seine, much thought was given to the question of a foundation, because even a slight giving way would be so magnified in the great height of the structure that the strain sustained by cross-pieces and braces would be far greater than calculated. Fortunately, it was found that the soil consisted of a compact bed of plastic clay, 53 feet in thickness, surmounted by a bank of sand and gravel, and all inclined toward the Seine. This seemed well fitted for the purpose. M. Eiffel was not however entirely satisfied with it. He therefore increased the solidity of the foundations by means of caissons (heavy iron boxes with open bottoms) of compressed air which made their way downward into the soil partly by their own weight and partly by the excavation of the earth beneath them. The air prevented the possible rising of soft clay to smother the workmen. Incandescent electric lamps furnished light beneath the caissons, which were filled with heavy concrete that hardened, making as it were huge bricks of great solidity that sank still deeper. It was owing to this modern device, the compressed air caisson, that a great danger was averted. The remains of unquestionably ancient masonry were found, which might have caused a dangerously uneven settling of the foundation. At each corner of the tower, which is square at the base and about 300 feet on a side, there is a lattice-work pillar that slants inward as it rises upward to a distance of about 600 feet from the ground, from which point the four like pillars continue together to the summit. These corner pillars are each 50 feet square at their bases, and are connected by open curved arches. Any unimportant subsidence of the foundation is provided for by hydraulic presses applied to iron wedges that lift each corner of the entire structure, and so any defect or strain due to contraction or expansion can be regulated. The relative lightness and strength of the material is such that the total weight will not be more for each square centimeter than that of a usual five-story house, certainly not as great as in very high buildings in New York and other large cities. The pressure upon the base of the tower is not more than 9 pounds for each square centimeter, while in the case of the Washington Monument it is, as we have seen, more than 58 pounds in like proportion.

The foundations became practicable, but there was a powerful and irregular force involved in the tremendous side pressure of the wind upon a tower presenting so much vertical surface in spite of its open lattice-work. It is evident that the height of the great Washington Monument has been surpassed only by the use of iron, which has the power to bend and still resist the force of the wind and which is well able to withstand marked contractions and expansions. The horizontal vibration is considerable under a high wind, at such a distance above the earth. The swaying of the long curved uprights will not be felt much by people at the summit. The height of the tower is such that the nature of the motion is gradual and less observable than in light-houses constructed of masonry, in which the elasticity is sometimes remarkable, owing to the quality of the mortar used. It is in recent years only that metallic beams have been made that enable engineers to erect structures to a height of 200 feet. Still further advances in the manufacture of iron make it now easy to attain 250 or even 350 feet. So many unknown quantities require consideration in a tower 1,000 feet high that the problem becomes serious and hard to solve. M. Eiffel points out the significant fact that the obstacles resemble those met with in extending a bridge from 500 feet to twice that distance horizontally, because of the great and accumulating side pressure of the wind exerted upon high vertical structures. It is thus seen that the construction is a greater achievement than would be at first imagined. It was desirable, while estimating the tremendous wind pressure, to avoid the multiplication of upright beams, involving diagonal braces more than 300 feet in length, which would result in an immense ugly iron frame-work resembling an elongated cage, or trestle-work railway bridge set up on end, with a deplorable architectural effect. Clumsy masses of beams and braces were necessarily omitted. The curved lattice-work before mentioned disposed of this question.

The corner pillars narrow from about 50 feet on a side at the base to 16 feet near the summit. They are anchored on solid foundation walls, and above, are bound together by horizontal girders, which serve as supports for several large halls or assembly rooms at different heights. These floors increase the security of the structure. The uncertainty of the wind force and its extent as calculated has led M. Eiffel to be peculiarly prudent in his methods of construction. He assumes for purposes of safety that the force goes on increasing from the base to the summit until the pressure is doubled. In making estimates of resistance the iron lattice-work was considered a solid wall taking the full force of the wind. In the more open parts of the tower the actual surface of the iron was multiplied by four to secure safety from the effects of a severe tempest. The wind in Paris ordinarily exerts a strain of from 13 to 15 pounds for each square meter.* A pressure of 22 pounds is allowed for in Germany, and Austria, in metallic frame works not subjected to the tremors of passing trains. This rule also holds in France. But it becomes necessary to provide for a much severer strain when only one end of the structure is supported, as in the Eiffel tower.

The inclination of the stone-work supporting each corner is at an angle of 50°. In extending upward the slanting ponderous iron-work, it was very difficult to maintain absolute stability, especially before the masses had been made secure by girders at the first gallery. As the work progressed this danger of displacement (requiring the utmost care) was lessened by the decreasing length of the girders that bound the whole together. In high trestle work, the apparently slight metallic bars seem insecure to the casual observer, an effect peculiarly noticeable in the high skeleton iron-work of the Manhattan Elevated Railroad near Eighth avenue and One hundred and tenth street, New York City. The spindling frame work, in this case suggests weakness; but this is an illusion due to an association of strength with the ponderous solidity of masonry or earth-work.

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*39,37 inches on a side.

The tower is spread much at the base to enhance its stability. Perhaps its height is exaggerated by the distant view of buildings in the Exposition grounds. The first gallery, which consists of an immense hall, is to be used as a promenade or for restaurants. It is 230 feet from the ground. Still further up is the second gallery, about 100 feet square and at a height of 377 feet, which exceeds the altitude of the following well-known structures:

	Feet.
The dome of Milan	363
Spire of the Invalides, Paris	342
Spires of St. Patrick's Cathedral, New York	332
Statue of Liberty, New York Harbor (above the water)	328
Brooklyn Bridge towers	279

Continuing up the Eiffel tower until it has narrowed to about 75 feet on a side we come to a point where the four great pillars combine, at about the height of the Washington Monument, the next highest known structure in the world. Only three of the following public edifices, aside from the greatest of the Egyptian pyramids, are more than half as high as the Eiffel tower:—

	Feet.
Washington Monument	555
Cathedral of Cologne	522
Old St. Paul's, London (destroyed by fire)	520
Cathedral of Rouen	492
Pyramid of Cheops	480
Cathedral of Strasbourg	465
Cathedral of Vienna	453
St. Peter's, Rome	432
Present St. Paul's, London	404

After adding 306 feet to the height of the Washington Monument, making 861 feet, the third gallery of the Eiffel tower is reached, where there is a glass-inclosed room 32 feet square, surrounded by a balcony. Surmounting this and 124 feet higher is a small observation room, with two windows on a side, from which can be seen Paris and its environs for a radius of about 75 miles.

The elevators, four in number, are to be worked in pairs, two to be used for visitors ascending and two for those descending, that an incessant stream of people may move in each direction. The ascent is to be made no faster than 20 inches a second, because great speed in stopping and starting would be decidedly alarming and disagreeable.

The escape of lightning is to be provided for by two cast-iron conducting pipes, about 20 inches in diameter, reaching from the summit to the base and thence 60 feet into the ground.

The construction of a tower composed of curves that will best withstand the wind has produced a very graceful architectural outline. The air of trimness in the realization of the design is due to the fact that there has been no waste of material. An upward moving force in taking the direction of least resistance would doubtless assume approximately the form of this structure. Nearly all kinds of growth acquire something like this cone shape while manifesting concentrated motion necessitated by surrounding forces. Many beautiful designs are founded upon the tapering forms of flowers and of leaves, as in the delicate tracery of frost-work. In building to secure safety from the action of the elements, M. Eiffel has perhaps unintentionally followed the methods of nature, and thus the architecural beauty of his work has the best possible confirmation.

The well-worn criticism that this scheme lacks utility is ever present in all daring scientific enterprises. But the value of this tower is admitted by eminent French scientists. It will take the place of the great balloon let up into the air by means of a cable worked by steam, which was so successful during the Exposition of 1878. An ascent can be made without the danger of collapse or gas explosion caused by lightning, often present in a captive balloon. The unexpectedly rapid approach of a local storm might cause loss of life before the winding-in of a balloon could be completed. The view of Paris at night, with its seemingly interminable boulevards brilliantly lighted, is marvellous, and such as aeronauts only have witnessed. The feeling of distance and height will not be lessened by intervening lower slopes as in most mountain views.

It is proposed to put upon the tower a number of electric lamps, powerful enough to light the city. The advantage of such a system had been long thought of, but it was a very difficult project to carry out, owing to the great intensity necessary. It has been decided however that the Exposition buildings and grounds are to be lighted in a manner never before equalled. In 1881, M. Sebillot proposed to place electric lights at an elevation of 1,000 feet, but the idea involved difficulties of construction and a waste of illumination that made it impracticable. It has been found that to make printed matter sufficiently legible in the park and gardens of the Exposition, not less than three concentric zones, numbering forty-eight lamps, would be required at so great a height. With special reflecting mirrors concentrating the light within prescribed limits, it is believed that the effect would be better than anything before accomplished, so far as known.

Many eminent men promptly admit the value of the tower for scientific purposes. M. Hervé-Mangon, of the Meteorological Society of France, points out the importance of observations made at different distances from the earth's surface under these conditions, and that experiments of the greatest interest are possible. The law of the decrease of the temperature with the height would be demonstrated better than from high points of land or from vast structures of masonry, which retain much heat, causing currents of air that interfere with observations or make them inexact. The variability of rain-fall could be well observed, also the average height to which fogs reach above the earth's surface near Paris. A relatively complete knowledge might be gained of the volume of water held in different air strata. This would make clear the reason why clouds light in volume sometimes precipitate so much water. As the condition of the air varies with the height, the advantage of having instruments far enough apart, one above the other, is obvious. On calm days, the general direction of the wind would be free from the effect of local heat accumulation due to the influence of neighboring buildings. All these phenomena could be carefully observed at a height to which only balloons ascend for an appreciable length of time. At this distance from the ground, the atmospheric conditions, freed from the surroundings of a mountainous or hilly region, are not precisely known.

A position above the fogs that very often obscure the horizon of Paris will facilitate astronomical observations impossible in ordinary weather. The vibration of the tower will doubtless exclude it from use in obtaining the precise positions of the stars, as pointed out by some astronomers, but it will leave the field free to researches regarding the chemical constituents of the stellar universe. Observations intended to establish the proper motions of stars by the displacement of lines in the spectrum would be more exact at a height of 1,000 feet than at that of the observatories. Photographic apparatus at the summit of the tower would be more efficient in case of an eclipse near the horizon, but work upon stars or nebulæ, requiring steadiness of position, ought to be reserved for calm nights. In every case the moon and the planets could be studied and drawn under more favorable conditions. The known temperature of the air at different heights is also of great importance in astronomical observations, because the resulting variation in refraction is so often a matter of conjecture.

In addition to the above experiments in meteorology, electrical science, and astronomy, there remain to be considered further questions of vegetable chemistry, peculiarities of growth under various conditions, and more exact data respecting the material constituents floating in the air. Further and finer investigations can be made, showing with additional interest the value of Foucault's well-known pendulum experiment demonstrating the rotation of the earth. The possible relation between magnetism and gravitation, which Faraday investigated with a falling body, might be carried further with advantage.

The instantaneous transmission of time signals for the benefit of all Paris, the more exact measurement of the velocity of sound under various atmospheric conditions, the estimated resistance of the air as a body falls at given rates of speed, the law of metallic elasticity in the contraction and expansion of the iron-work of the structure, the study of compressed gases and vapors with such extensive vertical possibilities,—these are some of the objects to be attained by this tower, destined to be one of the landmarks of scientific advancement. It may be of use as an army signal station in case of war as a position from which to observe the movement of an enemy. At a time of siege or of interruption to telegraphic communication the tower could be used as a center for optical military signaling for long distances, such as the 70 miles from Paris to Rouen. In such instances an answering signal might be sent from a high hill near at hand.

The immense outlay of work in this great structure cost only 6,500,000 francs, $1,300,000. There are twenty-seven iron panels, each of which required a separate diagram, that in turn formed the basis of a series of geometrical designs calculated by means of tables of logarithms. The metallic pieces number about twelve thousand, and the position of each, and the places for its rivets, had to be decided without error. In the iron plates were drilled 7,000,000 holes, which if placed end to end would form a tube 43 miles long. There were five hundred engineers' designs, and twenty-five hundred leaves of working drawings. It was necessary to employ forty designers and calculators, for a period of about two years. It is thus seen that the iron forms a vast complicated net-work not easily realized when contemplating the gracefulness of the completed tower. The large halls at Levallois-Perret had almost the appearance of a government administration.

M. Eiffel did not employ workmen of special skill, accustomed to very high scaffolding. It was feared that few could be found not subject to vertigo. But in the tower they did not work high in the air, with an open and dangerous footing. They were on platforms 41 feet wide, and as calm as on the ground.

It is proper that two great republics should, regardless of nationality, recognize the constructive genius of M. Eiffel, as they have already done in the instance of M. Bartholdi, designer and constructor of the wonderful statue of Liberty enlightening the World. Mr. Roebling's great work, the Brooklyn Bridge, thus seems extended into new conditions. The idea of a tower 1,000 feet high first assumed definite form, it will be remembered, in the United States, and it remained for a man of constructive genius in another and newer republic to crystallize it into an accomplished fact*. The power of thought over the refractory materials of the earth, as shown by the ingenuity of Thomas A. Edison, a power which Emerson illustrated in various ways, is thus emphasized anew. The limits of scientific achievement slowly recede.

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* The tower is designed to be 300 meters (984 feet) high. A slight addition, making it 1,000 feet, could be easily made.

THE TERRESTRIAL GLOBE AT THE PARIS EXHIBITION.*

Some time before the opening of the Paris Exhibition it was announced that one of the attractions of the show would be a great terrestrial globe, one millionth of the actual size of the earth. The globe is now exhibited in a building specially erected, near the Eiffel Tower, for the purpose, and it excites the warmest interest among all visitors who have devoted the slightest attention to geographical science. It was designed by MM. Villard and Cotard, and these gentlemen, who have received many congratulations on their success, have lately issued an account of the manner in which their project has been realized.

Maps on a plane surface give, of course, a very inadequate impression of the real appearance of our planet; and ordinary globes are too small to indicate, even vaguely, the extent of the spaces represented on them. The idea of making a globe one millionth the size of the earth deserves, therefore, to be described as a "happy thought," for although the meaning of a million may not be fully appreciated, it is not absolutely inaccessible to the human mind. When we see a place or a district marked on a globe, and learn that the reality is a million times larger, the proportions are impressively suggested, with at least some approach to accuracy.

The diameter of the globe constructed by MM. Villard and Cotard is 12.73 meters, (42 feet). It has a circumference of 40 meters (131 feet), and a millimeter of its surface represents a kilometer (a little more than 15¾ miles to the inch). The globe consists of an iron frame-work made chiefly of meridians united to a central core. This structure is carried by a pivot resting on an iron support. To the meridians pieces of wood are attached, and on these are fixed the panels composing the surface of the globe. These panels are made of sheets of cardboard bent by hand to the required spherical shape, and covered with plaster specially hardened. Fig. 1 shows how they are applied to the underlying structure. The total surface is divided into forty spindle-shaped spaces, the breadth of which at the equator is exactly one meter. Each "spindle" or gore is itself sub-divided, so that there are 600 panels of various dimensions. The designs are painted on the panels before they are put in their place, in order that the globe may ultimately be easily dismantled and removed.

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* From *Nature*, July 18, 1889: vol. XL, pp. 278–280.

The edifice in which the globe is shown has a metallic frame-work forming a cupola. It is lighted from above, and by the great glass frames of the sides. From a terrace or a narrow foot-bridge at the upper part the visitor can see the polar and temperate regions of the northern hemisphere. As he descends, he is able to see in succession all the regions of the globe to the south pole. At the bottom he comes to the support of the globe with the apparatus for putting it in motion (Fig. 2).

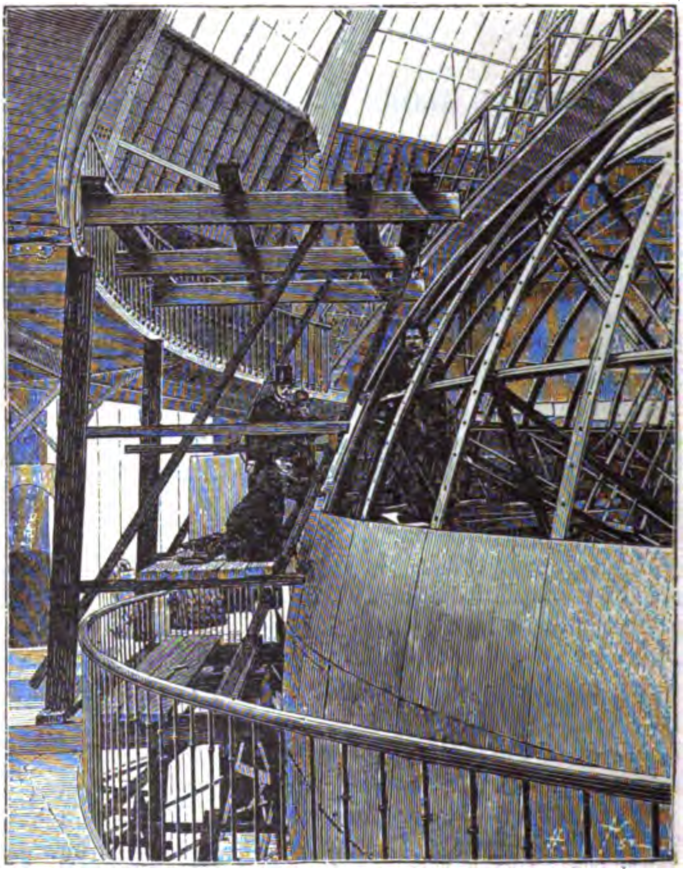

FIG. 1.

Even the loftiest mountains, if shown in relief, could only have been represented by elevations a few millimeters in height. Consequently the various mountain ranges have been painted on the surface. The various depths of the ocean are indicated in a similar manner.

To facilitate the study of the globe, it has been mounted with its axis vertical, and it may be turned upon the pivot which carries it. If its rotation were made to equal that of the earth, at its equator, a point of its surface would move at the rate of half a millimeter in the second. This movement would scarcely be visible, but it would, of course, represent an actual movement of the earth over half a kilometer in the same time.

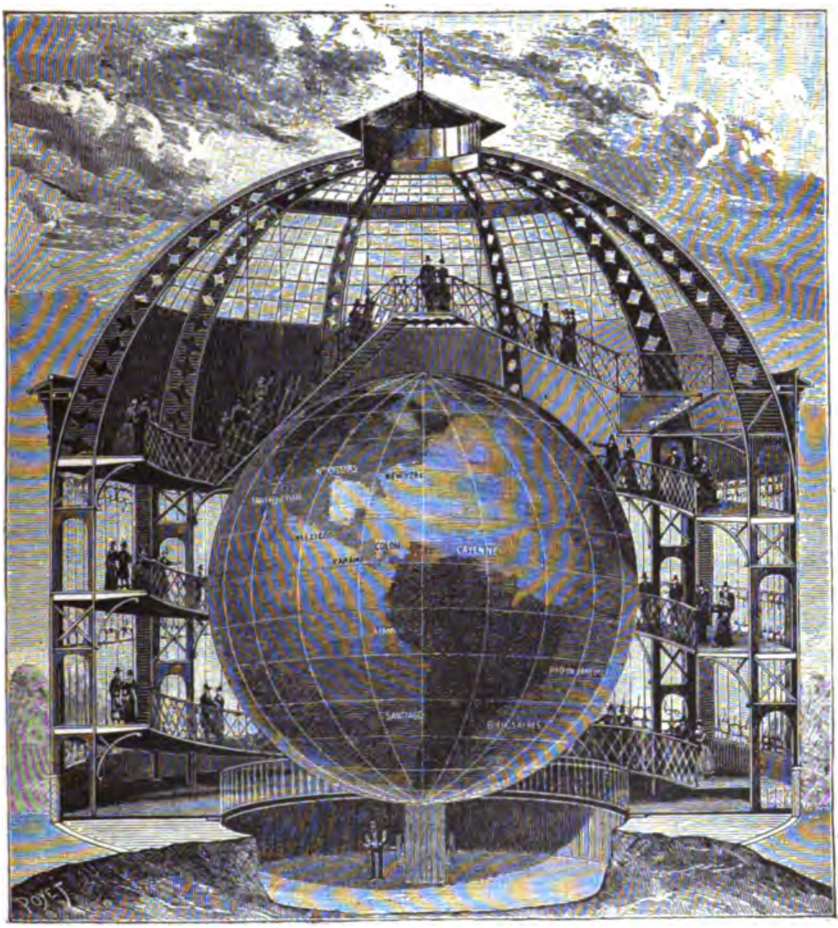

FIG. 2.

A figure of the moon, corresponding to this one of the earth, would have a diameter of 3.50 meters (11½ feet), and would be 384 meters (about a quarter of a mile) distant. A like figure of the sun would have a diameter of 1,400 meters (4,593 feet, or nearly five-sixths of a mile), and be distant about 150 kilometers (93 miles.) The diameter of a globe representing Jupiter on the same scale would be one-half—that of a globe representing Saturn on the same scale would be a little more than one-third—of the height of the Eiffel Tower.

This is not the first occasion on which an attempt has been made to suggest by means of a great globe the size of the earth, and the extent of its oceans and land masses. The globe of the Château of Marly, which is still to be seen in the National Library of Paris, excited much admiration in the age of Louis XIV, but it has only a diameter of about 5 meters, and is much less effective for its purpose than its successor in the Paris Exhibition.

It is significant of the present state of our knowledge of the interior of Africa that the makers of the globe, in preparing their maps, had twice to alter their representation of that continent in order to indicate the results of the most recent geographical discoveries.

GEOGRAPHICAL LATITUDE.

By WALTER B. SCAIFE, PH. D. (Vienna).

Introduction.—The designation of the situation of places on the surface of the earth by their latitude and longitude is such a common occurrence that one rarely stops to ask how these quantities are determined, much less to consider the evolution of the ideas which form the basis of the usage, or to study the slow progress of events through which (even after the theory was perfected) accuracy of observation and measurement was first made possible, while new and improved methods were being invented for representing the results thereof. Though latitude and longitude are so intimately connected in usage and thought, the methods of determining them are different, and each has its own peculiar historical development. Hence they can be separately treated without injury to the whole subject; and this article accordingly is confined to the consideration of the historical evolution of geographical latitude alone.

The word *latitude*, signifying breadth, was adopted by the early geographers to designate situation to the north or south, in contradistinction to east and west, because the then known world was longer from east to west, which was hence called the length, than from north to south, which was then naturally styled the breadth.[1] The fact that the earth is spherical and so can have, accurately speaking, no length or breadth, has not altered the nomenclature adopted in the infancy of the science. The technical meaning of the word latitude now includes however much more than the crude idea of mere distance north or south of a given point. It takes for granted the sphericity of the earth and its division by imaginary lines running east and west, whose distances from each other, though not exactly equal, mark the intersection with the earth's surface of plumb lines forming equal angles, each one to the next.[2]

The very fact of thinking of the earth as a whole shows that an individual or a people has made considerable progress in civilization, for among savages this conception seems to be lacking. But as soon as one forms an idea of the whole and begins to comprehend its vastness, there arises the necessity of systematic division, in order to avoid mental confusion. Even among fairly educated people of to-day, there exists frequently no adequate conception of the size of the various continents, not to speak of the earth as a whole. Thoughtful men at an early period recognized this necessity of division and hit upon a rude method for establishing one; but there was a long distance between the first crude trials and the marvellous accuracy of the methods, instruments, and results of the present. From the conception to the attempt at pictorial representation was a step which was certain sooner or later to be taken, and progress in the art of map-making has held equal pace with the advance of geographical science.

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[1] Ptolemæus, Geographica, lib. i, cap. vi. The origin of the idea is ascribed to Democritus of Abderos. (D'Avezac, Coup d'œil, 286, note 10; Lelewel, I, vi.)

[2] When we speak of the latitude of a place, then, we mean in reality not its distance from the equator, measured on the earth's surface, but the angle which a plumb line at that place forms with the plane of the equator.

Sphericity of earth.—As the theories of the orientals as to the form and nature of the earth generally rested upon fantasy, and not upon a scientific basis, they may be neglected in the consideration of the subject in hand, and the attention be at once directed to the Greeks,[1] who created the science of geography.

The beginnings of astronomy and geography were very closely connected; not as now, when the earth is recognized as a mere atom in the immensity of the universe, but considering the earth as the very center, toward which all was attracted[2] and around which the universe revolved.[3] As to who first taught the doctrine of the sphericity of the earth it is difficult to decide. To Thales,[4] Parmenides,[5] and Pythagoras,[6] respectively, this honor is ascribed. However that may be, there was scientific proof of the doctrine lacking till the great minds of Aristotle and Archimedes took the subject in hand.[7] Before scientific grounds were arrived at, various reasons were given by the several philosophers for their opinions. The well known one of the Pythagorean school, that the earth, being the center of the universe, must have the most perfect form, viz, spherical, is perhaps as good as any. Strabo, although much later, considers it sufficient to maintain that the spherical form of the earth follows, as a matter of course, from the construction of the universe.[8]

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[1] Delambre, Astron. ancienne, I, ix. "C'est donc chez les Grecs, et chez eux seuls, qu'il nous faut chercher l'origine et les monuments d'une science qu'ils ont créée et que seuls ils ont eu les moyens de créer."

Lelewel, vii. "Ils [les Grecs] ont pu voir et examiner les cartes égyptiennes, phéniciennes et des orientaux: mais il n'y trouvaient rien pour leur schème, qu'ils élaboraient sur leur propre terrain."

[2] Grosskurd's Strabo, lib. ii, Abt. iv. § 2. Vol. I. p. 180.

[3] Mannert, I. 98.

[4] Delambre, Astron. ancienne, I, 14.

[5] Sprenger, Ausland, 1867, p. 1045.

[6] Mädler, Gesch. d. Himmelskunde, I. 38, 39.

[7] Günther, Geophysik, I. 130.

[8] Forbiger's Strabo, lib. i, cap. iii. § 3. p. 77, 78.

It was early seen that the earth, with its numerous elevations and depressions, could not be a perfect sphere;[1] which Strabo in truly philosophical manner avoids by remarking that in immensity things so small disappear.[2] Pliny however offers another solution of the subject by maintaining that an ideal circumference resting on the tops of the mountains would form a perfect sphere.[3] Although, as has been shown, a few learned specialists among the Greeks and Romans believed and taught that the earth is spherical, this belief not only never descended to the masses, but was rejected by such scholars as Herodotus[4] and Tacitus.[5]

First circles.—The comprehension of a subject often involves two processes of thought, viz, a conception of the whole, which is then divided into parts, in order by the investigation of the several parts to arrive at length at a just understanding of the whole. In this manner the science of geography has been brought to its present high level. There could be no other divisions of the surface of the earth than those arising out of natural physical features or of political borders, before man had formed an idea of the whole. Having arrived at this point, it was a natural step that the five circles by which Thales had divided the heavens[6] should be applied to the earth's surface.[7] Strabo quotes Poseidonius as authority for regarding Parmenides as the inventor of the division into five zones, who however made the torrid zone extend beyond the tropics, to which limits it was reduced by Aristotle. The latter however is also criticised by Strabo for making the "burned" zone too broad, inasmuch as at least half the distance between the Tropic of Cancer and the equator is known to be inhabited. As it was accepted as fact that the "burned" zone was uninhabitable, its northern limit could not extend farther than the southern boundary of Ethiopia.[8] Later Strabo seems to have forgotten his objection to Aristotle's division and himself adopts the zones as exactly corresponding to the five circles of the heaven.[1] Pliny, on the other hand, takes the names of the zones literally, and considers the whole torrid zone uninhabitable, and so preventing all intercourse between the two temperate zones.[2]

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[1] Forbiger's Strabo, lib. i, cap. iii.

[2] This is finely illustrated by Prof. Albrecht Penck, in his pamphlet on "Theorien über das Gleichgewicht der Erdkruste," Wien, 1889, pp. 11, 12. He says: "Aber jener Beschauer, der sich in den Weltraum begeben könnte, wird bald die Höhen Unterschiede zwischen Berg und Thal verschwinden sehen, die gewaltigen Festlandsplateaux werden ihm allmählich mit dem Meeresboden verwachsen, dem sie aufgesetzt sind, und schliesslich wird sich der ganze Erdball seinem Blicke darbieten. Derselbe würde ihm als Kugel erscheinen."

[3] Strack's Plinius, I. p. 107. L. ii. 64.

[4] Mannert, I. 4.

[5] Peschel, Gesch. d. Erdkunde, 35.

[6] Delambre, Astron. ancienne, I, 15. Bailly, Hist. de l'astron., 196, says, as usual, the idea was not original with him, but suggested by Ulysses (p. 187).

[7] Delambre, *ibid.* I, 257, ascribing the application to Hipparchus, Strabo calls Parmenides the "inventor" of the division of the earth in five zones. Grosskurd's Strabo, lib. ii. Abt. ii. § 1. Vol. I. pp. 154 *et seq.* Lelewel, Géog. d. moy.-âge, vii, ascribes the application to Eudoxus of Cnidos.

[8] Grosskurd's Strabo, lib. ii. Abt. ii. § 1, Vol. I. pp. 154 *et seq.*

The Greeks however did not arrive at one bound at the exact division of the earth's surface. The starred heavens passed over their heads, making it impossible to see and study much while remaining at the same place. Exact knowledge of the world was not thus attainable; practical information gained by observation and travel was necessary as a foundation for the complete application to geography of the lines of astronomy. Though adopting theoretically the five main parallels of the astronomers, the early geographers felt the necessity of having a working basis; and, leaving the equator at one side as a practically unknown quantity, they adopted as a central line, a parallel passing through a place, (Rhodes,[3]) not only important in itself, but also for them, the practical middle between the north and south, inhabited world. This line was proposed by Eratosthenes, and passed from the Pillars of Hercules through the Strait of Messina, Southern Greece, Rhodes, then on through Asia to the mountains forming the (imaginary) northeast boundary of India.[4] This parallel is known as the Diaphragm, and is generally supposed to have been so named by the Greeks,[5] but a modern French investigator says he was unable to find this designation among the ancient Greek geographers.[6] Following the policy of employing an arbitrary line as a center, largely if not mainly because of its local importance, other parallels to the north and south were adopted because they passed through well known places. The spaces thus divided off, had no fixed arithmetical relation, but were supposed to mark climatical differences, and received hence the name *climates*. Eratosthenes made a division of the entire known earth into four great rectangles which he called *Sphragides*,[7] and these in turn into twelve climates.[8] The latter were reduced to eight by Hipparchus,[9] and later increased[10] until they became, in the work of Ptolemy, twenty-three.[11] However, the division of the earth into five zones, as is now customary, was adopted by Parmenides, sanctioned by the authority of

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[1] Grosskurd's Strabo, lib. ii. Abt. iv. § 3. Vol. I, pp. 181–2.

[2] Strack's Plinius, I. iii. lib. ii. 68.

[3] Berger, Frag. d. Hipparch, 72. Grosskurd's Strabo, lib. ii. Abt. I. § 1, note 1. p. 110. Used first by Dicæarchos. Mannert I. 90.

[4] Forbiger's Strabo, lib. ii. cap. i. § 1. Mannert, I. 90, gives the honor of first having proposed this line to Dicæarchus.

[5] Term already employed by Dicæarchus, Sprenger, Ausland, 1867, p. 1045. Grosskurd's Strabo, lib. ii. Abt. i. § 1, note 1, p. 110.

[6] D'Avezac, Coup d'œil, etc., p. 269, note 9.

[7] Grosskurd's Strabo, lib. i. Abt. i. § 13. Vol. I. p. 128.

[8] Lelewel, Bres. Ed. I. x.

[9] Grosskurd's Strabo, I. 215–17, lib. ii. Abt. iv. § 26.

[10] Sprenger, Ausland, 1867, p. 1043.

[11] Mannert, I. 130.

Aristotle,[1] and accepted by Strabo, although the latter expressly says, the whole of the "burned" (torrid) zone is not rendered uninhabitable by excessive heat, "as we conclude from the Æthiopians above Egypt."[2] At the same time the theoretical division of circles into equal parts was not lost sight of. From Babylon the Greeks received the duodecimal division of the Ecliptic, from which was developed the present custom of dividing a circle into 360 degrees. The latter was also borrowed from Babylon,[3] or first proposed by Hipparchus;[4] but for a long time gave way to the Eratosthenian division into 60 degrees,[5] until revived and made popular by the authority of Ptolemy. Cleomedes proposed a division into 48 degrees, allowing 4 to each sign of the zodiac,[6] but seems to have had no following.

Methods and instruments.—In these days of exact scientific research one can scarcely realize the crudity of method and the indifference to exactness which characterize the work of many of the scholars of antiquity, and this is particularly striking in their geographical investigations.[7] Not only did they accept the tales of sailors as facts, and found theories thereon,[8] but they were so easy-going as not to hesitate to change the result of their most careful calculations in order to have round numbers to work with.[9] That the sun changes its position in the heavens at the different seasons of the year and that the length of shadows varies accordingly, were matters of early observation; also that the duration of the longest day is different according to position north or south. These facts combined furnished the earliest basis for determining latitude. The known account of the well at Syene, directly over which the sun stood at noon at the summer solstice, whether true or not,[10] gives an idea of the rude method of determining astronomical, or in this case also geographical points, then in vogue. Accepted as true, it was made the basis of the geographical latitude of antiquity. The parallel of Syene was hence considered the Tropic of Cancer, and from this point distances and degrees were computed north and south.

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[1] Mannert, I. 100.

[2] Grosskurd's Strabo, lib. ii. Abt. ii. § 1. Vol. I. pp. 154–5. Sprenger, Ausland, 1867, p. 1043.

[3] Peschel, Gesch. d. Erdkunde, 43.

[4] Berger, Fragmenta d. Hipparch., 44.

[5] D'Avezac, Coup d'œil etc. pp. 271–2. Peschel, Gesch d. Erdkunde, 43. An. 2. Grosskurd's Strabo, lib. ii. Abt. iv. § 7. Vol. I. p. 186. Günther, Die Erdmessung d. Eratosthenes, Rundschau für Geog. und Statistik, III. Jahrg. p. 327. Bailly, 179, says this was the general division and in use among the Indians, Chaldeans, Persians, and Egyptians.

[6] Delambre, Astron. ancienne, I, 220.

[7] Sprenger, Ausland, 1867, p. 1065. Mädler, Gesch. d. Erdkunde, I, 70. See Berger, Frag. d. Hipparch., 30, 31.

[8] Forbiger's Strabo, lib. ii. cap. i. § 11.

[9] Sprenger, Ausland, 1867, p. 1045. Lelewel, Géog. du moyen-âge, vii. D'Avezac, Coup d'œil, etc., 271, n. 5.

[10] "Ein solches Werk zur Constatirung einer astronomischen Thatsache ist ganz im Geiste der Erbauer der Pyramiden. Wir können sicher sein, es ist von den Pharaonen ausgeführt worden, und zwar etwa 700 Jahre v. Chr." (Unfortunately I have forgotten to note from whom I extracted this remark.)

The only scientific method then known of determining latitude was by observing the length of shadow cast, either at the equinox or summer solstice, by a simple instrument called a gnomon. It has been suggested that the obelisks of Egypt served this purpose.[1] Two varieties of this instrument are mentioned as in use among the ancients. One was formed by a hollow hemisphere having in its center an upright whose length was equal to the radius of the sphere; so that the proportional length of the shadow to the distance between the base of the upright and the periphery of the hemisphere, as observed at the summer solstice, gave the proportion of the distance of the place of observation between the Tropic of Cancer and the north pole. This instrument is said to have been imported from the Chaldeans,[2] and used only by Eratosthenes among the Greeks.[3] The usual form of gnomon had a flat base, and the latitude of the place of observation was indicated by the ratio of the length of the shadow to the upright, as observed at the equinox. Whether this instrument was brought from the East or invented independently in Greece, it seems impossible to decide. There is probably no good reason for doubting that it was already in use among the Chinese eleven hundred years before Christ.[4] But with them directly the Greeks had no intercourse. Bailly, who finds the beginning of all things in the Orient and allows the Greeks no inventive genius whatever, relates that Pherecides erected such an instrument on an island in the Syrian Sea, and that Anaximander perhaps carried the knowledge thereof to Greece,[5] while Pliny ascribes its invention to Anaximenes of Miletus, a pupil of Anaximander,[6] and a modern investigator of great learning maintains that Pytheas "is the first of whom it is historically certain that he determined the altitude of the pole of a place by the length of the sun's shadow."[7]

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[1] Delambre, Astron. ancienne, I, 14. Cassini, Grandeur, etc., 33. One brought from Egypt by order of Augustus, was placed on what is now the Champ de Mars and used for that purpose by Manilius. Wolf, Gesch. d. Astronomie, 124.

[2] Mannert, I. 111.

[3] "C'est l'hémisphère creux de Bérose." Delambre, Astron. ancienne, I, 221. Ideler is of another opinion. "Sie (die Skaphe) war eine Erfindung des *Aristarch* von Samos. Von ihrer Gestalt hiess sie auch *Hemisphaerium*; denn sie bestand aus einem sphärischgekrümmten metallenen Becken, auf dessen Boden in der Richtung und Länge des Halbmessers ein Stift, *Γνώμων*, als Schattenzeiger, errichtet war." Zach, Mon. Cor., May, 1811, citing as authority Martianus Capella, de Nupt., I, vi, p. 194. Ed Grotii.

[4] Delambre, Astron. ancienne, I, xvii. Kirchhoff, Unser Wissen von der Erde, I. 15.

[5] Bailly, Histoire de l'astron., 197, 198.

[6] Strack's Plinius, I. 115; lib. ii. 76–78.

[7] Mannert, I. 5.

Be that as it may, this instrument must be recognized as one of the most important aids to the early development of geography, and, whether borrowed or invented by the Greeks, they deserve the credit of having made the best use of it. In all probability the number of places whose latitude was determined by its use was exceedingly limited,[1] as the geographers employed other means also for attaining the same end. The rising and setting of certain fixed stars served as one basis for calculating the latitude of places, while even untrustworthy data as to climate, wind, color of inhabitants, varieties of animals, vegetable products, etc., were used to supply the lack of better kinds of information. The ancients were right in considering the length of the longest day an important factor in determining latitude, and Hipparchus is said to have calculated what it should be for each degree.[2] However, the want of accurate time-pieces rendered it impossible to make exact observations. As to the instruments employed for astronomical observations we know almost nothing. Hipparchus, who was an ardent advocate of using the results of astronomical observation for geographical purposes, does not mention by name a single instrument which he employed, nor does he use even the generic term instrument.[3] Eratosthenes used the hollow gnomon (mentioned above) to determine the latitude of Alexandria,[4] and at his request the king ordered to be made and placed on the roof of the museum the famous armillary spheres, with which he determined the declination of the Ecliptic to within six minutes of arc, a praiseworthy exactness for that age.[5] To this may be added the known fact of Ptolemy's use of the astrolabe at Rhodes[6] and the invention of the sun-dial by Anaximander.[7] With such simple means were the beginnings of the science of geography made.

Application.—It is difficult for us to realize the length of time required to make such small advances as have been indicated, but the history of other branches of science is parallel. The inertia to be overcome in advancing from utter ignorance of a subject to the commencement of real knowledge, and founding the principles on which investigation should be carried on, is immense. Knowledge does not spring into being, Minerva-like, completely developed, but resembles much more the insignificant acorn, which, slowly growing and battling with the elements, becomes in the course of time the mighty oak. Ages may pass in the almost unheeded dreaming and theorizing of philosophers and investigators before a science is matured ready for prosaic, worldly application. So for example were the calculations (though erroneous) of the old Greeks one day to bear practical fruit by encouraging Columbus to make the famous voyage which resulted in the discovery of America.

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[1] Peschel, Gesch. d. Erdkunde, 44, 45.

[2] Sprenger, Ausland, 1867, p. 1045.

[3] Delambre, Astronomie ancienne, I, 139. At the same time he thinks Hipparchus was possibly the inventor of the astrolabe, I, 154.

[4] *Ibid*, I, 221.

[5] Mädler, Gesch. d. Himmelskunde, I. 57.

[6] Delambre, Astron. ancienne, I, 154.

[7] Mannert, I. 11.

These humble beginnings and the men who made them are then not to be despised because no more was effected, but much rather, to be honored that under the circumstances so much was accomplished. They might perhaps have made more progress, had they theorized less as to how far north and south the earth is inhabitable,[1] a matter necessarily beyond their power of determination, and instead of that have spent their energies in more accurately investigating that which lay at hand. There is however a strong temptation in all men to strive for the attainment of the unknowable; and, whether this tendency leads to religious enthusiasm, spiritualism, or philosophical dreaming, it is but taking on different forms of expression of the same factor in human nature. Accordingly we must not blame the early geographers even if the number of places where the altitude of the pole was actually observed does not exceed half a dozen.[2] There were very few specialists, and they without co-operation; travelling was difficult and expensive, and general interest in geographical theory practically null. Furthermore, they knew their observations did not give perfectly exact results;[3] but thought that lay in the nature of the work to be done, and not in defects of their theories or instruments.

Their base line was the parallel of Syene, which they reckoned in round numbers to be in latitude 24° N., instead of the true latitude of 24° 5′ 23.″ But they made a still greater mistake in accepting the same parallel as the Tropic of Cancer;[4] although Eratosthenes's calculation is supposed to have given 23° 51′ 19″.5 for that line, and Ptolemæus quotes Hipparchus as employing 23° 51′ 20.″[5] After the twenty-fourth parallel, the one most frequently referred to was probably that of Alexandria, the great commercial center of the age, from which voyagers toward all directions calculated distances, which calculations were much relied upon for fixing latitude. It was accordingly recognized as of prime importance to determine its true latitude, the result showing 30° 58′[6] instead of 31° 11,′[7] according to recent observations. Rhodes, through which the "Diaphragm" passed, was said to be on the thirty-sixth parallel,[1] although they knew that that was not exact.[2] Other points whose latitude is given by Hipparchus are Athens, 36°; Alexandria in Troas, 41°; Byzantium and Massilia (Marseilles), 43°; Syracuse, 36° 44′; mouth of the Xanthus in Lycia, also 36° 44′; and Babylon, 33° 30′. One of the easy-going methods of the early geographers was to bring as many places of importance as possible on the same parallel, of which there are several lists.[3] That this at best could be only an approximation to the truth, and that opinions would necessarily vary, is a matter of course. One well known case gives a striking example, viz: Strabo's rather sharp criticism of Hipparchus for placing Byzantium on the same parallel with Massilia and then himself making the still greater error of placing the former to the north,[4] while in reality it is more than 2 degrees farther south.[5] Hipparchus dreamed of the possibility of fixing the locations of all places on the surface of the earth by the use of a common standard, viz, that of latitude and longitude,[6] and did all in his power to show his successors how to attain that possibility. But all were not willing to follow the lines laid down by him, and even so good and late a geographer as Strabo combated his theory, while not being able to propose a better and in fact adopting in great part that which he condemned.[7] First at the hands of Marinus of Tyre[8] and of Ptolemy[9] was made a practical application on a large scale of the ideas of Hipparchus, which even now form the basis of our usage.

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[1] See Grosskurd's Strabo, pp. 117 and 118.

[2] Peschel, Gesch. d. Erdkunde, 44. Mannert, I. 95, seems to be of another opinion when he says: "Hipparchus hat das wichtige Verdienst weit mehreren Orten ihre wirkliche Lage der Polhöhe nach anzuweisen, als es Eratosthenes bei wenigeren Erfahrungen thun konnte."

[3] Sprenger, Ausland 1867, p. 1066.

[4] Mannert, I, 102.

[5] Delambre, I, 87.

[6] Berger, Frag. d. Hipparch., 48.

[7] Peschel, Gesch. d. Erdkunde, 46, n. 4.

Maps.—The use of maps dates from a very early period. Like first essays in general they must have been very crude. In the first place their makers had an extremely imperfect knowledge of the earth, and what is not accurately known can not be pictorially well represented. Moreover, there is an inherent difficulty in the matter, owing to the sphericity of the earth, whose form necessitates distortion of some sort when represented on a flat surface. If drawn in perspective, as one looking at the earth from some distant point in space would see it, the portions toward the circumference of the resulting circle become falsely curved and much too narrow for their length. If drawn according to Mercator's projection, the meridians remain parallel instead of converging at the poles, and so give a false picture of the comparative breadth of bodies of land and water at the equator and to the north and south. So each possible kind of projection on a plane surface has its peculiar accompanying distortion. As to how the earliest maps were drawn, we have no exact information; but they were in all probability mere sketches of the outlines of known lands, with a river and a city here and there indicated, without any thought of perspective.[1] As to who projected the first map of the world based on scientific principles, modern critics are not agreed; but it seems probable that it originated with Eratosthenes and was greatly improved by Hipparchus.[2] This was called a "planisphere" and supposed the known world hollow as seen from a point on the opposite surface of the earth, directly *vis-à-vis* to the center of the part represented.[3] Hipparchus, dividing the distance between the equator and the poles into 90 degrees, according to the division of the entire circle into 360 degrees, drew for each degree a parallel of latitude, and, knowing himself the true latitude of but few places, gave the astronomical conditions for determining it as groundwork for his successors.[4] Strabo, we have seen, did not favor Hipparchus's manifold divisions of latitude;[5] but he himself did not attempt to project a new map, preferring the easier method of making changes in that of Eratosthenes.[6]

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[1] Ptolemæus, Geographia, lib. i, cap. xx.

[2] Hipparchus gave 36° 20′ (Berger's Hip., 72).

[3] Grosskurd's Strabo, I. 219, 220, lib. ii. cap. iv. § 27. Forbiger's Strabo, lib i. cap. 4. § 4. p. 100.

[4] Grosskurd's Strabo., I. 1889, lib. ii. cap. iv, § 8.

[5] Delambre, p. 257, makes it a criticism of Pytheas.

[6] Berger, Frag. d. Hipparch., 20, 21.

[7] Berger's Hipparch., 44.

[8] Peschel, Gesch. d. Erdkunde, 51.

[9] Ptolemæus, Geographia, lib. i, cap. xix.

However, in the text he goes into some detail as to his ideas of map-making, prefers on the whole a globe of not more than 10 feet diameter, and considers the next best thing a drawing of at least 7 feet diameter, on which the lines of latitude would be parallel and the meridians converge;[7] the two "main lines," evidently the parallel and meridian of Rhodes, to be at right angles.[8] Marinus of Tyre, with much better information than Hipparchus, tried to realize the latter's ideal of representing all places by their latitude and longitude.[9] Having nothing direct from him however, we pass at once to his successor and improver, Ptolemy.[10] With this great scholar, classical geography reaches it highest point of development.[1] He not only possessed all the available knowledge of his time bearing upon the subject, but based his work so thoroughly on scientific principles that its revival in modern times gave the starting point to many of the improvements of the new geography. That his maps were but improvements on those of Eratosthenes[2] and Hipparchus,[3] and not absolutely new, is not matter of censure. He recognized the justness of their principles and simply added thereto what his talent and the better facilities for obtaining knowledge afforded by his time and circumstances rendered possible. He not only wrote the principles of map-drawing, but put the same into practice, producing a very fair representation of the then known world, and he added thereto special maps on a larger scale to the number of twenty-six.[4] Here it is not in place to go deeply into the details of Ptolemy's projection, which however is interesting.[5] The ground-plan is that of an open fan, the radii forming the meridians of longitude, the lines parallel to the outermost curve representing the parallels of latitude. Of these there were four principal ones, the outermost being the supposed southern limit of the habitable zone. From the center of this line to the point forming the pivot of the fan was a line to be divided into one-hundred and thirty-one equal parts. Starting from the south, sixteen of those parts bring us to the equator, on which 180 degrees of longitude were marked, forming the extreme limits east and west of the known world. Counting then thirty-six parts toward the north, came Rhodes, whose parallel was the most important line of ancient geography. Twenty-seven parts more bring us to the parallel of Thule, the limit to the north of the habitable world. The other parallels were those of important places, without an attempt to make equal divisions, as is done with the meridians, although at one side the parallels are marked at distances of 10 degrees. At the other side an unequal division into climates is indicated. Theoretically, however, Ptolemy advocates the division into equal parts by parallels of latitude. He gives also rules for another kind of projection in which the meridians should be curved lines, as this would better represent the middle latitudes. At the same time he says that this projection is much more difficult to draw.[6] Here we have the best production of classical geography. As far as latitude is concerned the theory is completely developed, while the practice remains extremely imperfect. Though recognizing the usefulness of equally distant parallels, he makes his drawing more easily by adopting only those of known places. Furthermore, he still clings to the old idea of divisions into climates, the absurdity of which he would have appreciated if he could have seen the modern isothermal lines.

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[1] Mannert and Bailly under "A." Ukert, Geog. d. Gr. & Römer, I. 81, referring to the anecdote of Socrates leading Alcibiades to a map.

[2] Berger, Fragmenta d. Hipp., 29, 73. Mannert gives Anaximander the honor of having made a globe, I. 11; also Bailly, Hist. d'astron, 198. Delambre says of Hipparchus: "C'est d'après la projection dont il est l'auteur que nous faisons encore aujourd'hui nos mappemondes et nos meilleures cartes géographiques" (Astron. ancienne, I. xv).

[3] D'Avezac, Coup d'œil, etc., p. 275, calls it "la nouvelle projection d'Hipparche."

[4] Berger, as note "A."

[5] *Ibid.*, p. 44.

[6] Grosskurd's Strabo, I. Einleitung, § 7. p. xxx.

[7] *Ibid.*, I. 191.

[8] *Ibid.*, 197, 198.

[9] Mannert, I. 7.

[10] *Ibidem*, I. 8: "Der Ruhm der Erfindung (eines neuen Systems) bleibt dem Marinus, aber die Ausbesserung des noch ziemlich rohen Entwurfs, ist das Verdienst seines Nachfolgers Ptolemäus Alexandreia in Aegypten. Mit vorzüglichen mathematischen Kenntnissen ausgerüstet, mit mehreren neuern Reisebeschreibungen versehen, wagte er sich an die Umarbeitung des marinischen Werks. Er ergänzte das Unvollständige, verbesserte die Fehler welche er entdeckte, gab allen Orten eine bestimmte Lage, und zog die zu grossen Messungen seines Vorgängers von der Länge und Breite der bekannten Erde mehr in das Engere.

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[1] Mannert, I. 153: "Nach Ptolemäus wagte sich niemand weiter an die Verbesserung der Geographie. Anstatt manche seiner Angaben zu berichtigen, nahm man dessen Werk für das *non plus ultra* der Wissenschaft, und hielt es fast für Sünde eine seiner Behauptungen nicht gelten lassen zu wollen; und desto mehr, da ein grosser Theil der spätern Gelehrten die Gründe derselben nicht verstunden."

[2] Mannert, I. 96.

[3] Berger, 30, Note 1. "Es (das Verfahren des Ptolemäus) giebt fast die ganze Hipparchische Lehre wieder."

[4] Zach remarks that the special maps are not drawn according to Ptolemy's own proposition (Mon. Cor. Jun., 1805, p. 504).

[5] Ptolemäus, Geographica, lib. i. cap. xxiii.

[6] The first maps based on this projection were drawn by Nikolaus Donis (1482). (Mannert, I. 178, 179).

How much or how little geographical knowledge the Greeks derived from the Orient or from Ancient Egypt, it is at this day impossible to determine. Of one thing we may be certain, viz, that it was at best only fragmentary. With this as a starting point, they, in the course of centuries, developed a sound theory as to the form of the earth, then proved it mathematically; though often going astray, as one does in the beginning of every science, they arrived at a fair, almost an accurate, computation of the earth's size. Though acquainted in reality with only a small portion of the globe, they hit upon the only truly scientific method of indicating position on a body which, having neither breadth nor length, presents a puzzling problem. As far as circumstances permitted they put their theories into practice,[1] and laid down many of the lines on which modern scholarship is still developing the science.

In the mean time a movement had begun in Palestine which was to produce great changes in the civilized world and from whose influence the subject now under consideration was not to remain free. Rejecting almost as a whole the literature and art with which they were familiar, as an inseparable part of that heathenism which they were struggling to overcome, the church fathers sought to find in the Bible the only source of true knowledge.[2] They accordingly rejected the doctrine of the sphericity of the earth, which had become, at least among specialists, firmly established, and with childish religious arguments, settled for themselves and their followers the whole matter, *e. g.*, by asking, "How, on the day of judgment, could the people on the other side of a ball see the Lord descending through the air?"[3] or maintaining that if there were antipodes Christ would have gone to them.[4] They were not united as to what form the earth really has, one believing it to be square,[5] another circular, because the Bible uses the expressions "the four corners of the earth" and "circle of the earth." The latter being the more popular, the so-called "wheel maps" were accordingly much in vogue,[6] while again the Tabernacle was thought to be a mystic symbol of the earth, and that the latter must therefore be rectangular and twice as long as it is broad.[1] In the north was a lofty mountain, behind which the sun took his course at night in order to appear again the next morning in the east.[2] That the earth was the center of the universe could not be doubted by an orthodox Christian.[3] It stands to reason that with such conceptions of the world there was felt no necessity for the use of parallels of latitude, and this branch, together with the whole science of geography, fell into decay during the period of the supremacy of the church. Nothing more is heard of astronomical observations to determine the location of places; the gnomon, the sphere, together with most of the products of Grecian culture, are buried in the dust, awaiting resurrection at the hands of another race, itself now in the bonds of darkness, but soon to start on a course of conquest over men and ideas unprecedented in its rapidity and brilliancy.

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[1] "In fact, when we come to examine their geographical proficiency, we find it in exact relation to the poverty of their geometrical means." Leakes, Journ. R. G. S., 1839, IX, p. 14.

[2] Exceptions to be noted *infra*. As to their rejection of Greek geography, see Latronne, 602.

[3] Kosmas, quoted by Günther, Studien, etc., 3.

[4] Doctrine of Procopius von Gaza, Zöckler, Theol. und Naturwiss., I. 127.

[5] Santarem, L'histoire de la cosmographie, I, 107, 112.

[6] Peschel, Gesch. d. Erdkunde, 100.

Though a wrong conception of the form of the earth was generally accepted by the Christian Church up to the time of the great discoveries and circum-navigation of the globe, there were not wanting at periods a few great minds which had a clearer idea of the truth. Clemens, Origen, Ambrose, Basil,[4] John Scotus Erigena,[5] the Venerable Bede,[6] Virgilus of Juvavo,[7] Adam of Bremen,[8] and some others are mentioned as having testified to a belief in the rotundity of the earth.[9] One fact notwithstanding remains certain, viz, that the church officially opposed the doctrine in spite of the contrary opinion of a few learned fathers, and the science of geography failed to receive any addition "to the knowledge which the ancients had of the globe and the habitable zones."[10]

The Jews believed the earth to be flat and Jerusalem in the center,[11] which idea was adopted by the earlier Christians along with the rest of Jewish doctrine; and we find accordingly a number of crude maps of the world with Jerusalem in the center and Paradise to the east, which on account of its importance was placed at the top of the map.[12] With such fundamental ideas it was not possible to bring into harmony the work of Ptolemy, and we accordingly find other maps preferred;[1] though it is but just to add that the Roman itineraries were more practicable for travelling, and probably also more easily acquired than those of Ptolemy. On the other hand, if we turn to the greatest known geographical work of Christendom in the Middle Ages, that of the unnamed geographer of Ravenna, written in the seventh century, we find "no notion of geographical latitude.[2] The French geographers of the twelfth century paid no regard to the relative position of cities,[3] and the English, even in the following century, gave their entire attention to the itineraries.[4] As navigation increased in activity there came into popular use among the mariners the so-called compass maps, which, disregarding projection, latitude, and longitude, were based on observations of the compass, starting from one or more fixed centers. These maps generally neglected the inland, but in course of time came to picture very accurately the most frequently visited coast lines. Lelewel (II, 16, n. 32) maintains that this kind of map is very ancient, that one in fact served as a model for the world-map of Eratosthenes, but he fails to mention his authority for the statement, and the writer has not seen elsewhere the suggestion of such an idea. Furthermore, as we have no evidence of the Greeks having possessed the compass, how can they have drawn "compass maps," whose foundation is not a general idea of direction in reference to the equator and the poles, as was the case in Greek maps, but rather of direction from given *local centers*, if we may be allowed the expression?

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[1] Peschel, Abhandlungen, I. 76.

[2] Theory of Kosmas. Latronne, 610.

[3] Latronne, 604.

[4] Günther, Kosmog. d. M. A. Rundschau.

[5] Latronne, 317. Für Geogr. und Statis., IV. 313.

[6] La terre "est au milieu de celui-ci [le monde] comme le jaune est dans l'œuf." Quoted by Santarem, L'histoire de la cosmographie, I. 25.

[7] Günther, Studien, etc., 6.

[8] *Ibid.*, 8.

[9] Pérennès, Biog. universelle, XII, p. 387, mentions also as holding this belief St.-Grégoire de Nysse, St.-Grégoire de Nazianze, St.-Athanase; that St.-Hilary and Origen mention the antipodes.

[10] Santarem, L'histoire de la cosmographie, I. 22, 151.

[11] Ukert, I. 7.

[12] "Sachez que la Bible nomme l'Orient le devant; le sud la droite; le nord la gauche." Lelewel, Table xiv, p. 15, quoting Meir al Dabi Hispanus, 1362.

So we see that Christendom, as a whole, remained for centuries in ignorance and neglect of the principles of geographical latitude, though it had conquered the very region where in better days the theory had been perfected and the practice greatly advanced. Preceding the dawn of the Renaissance travelling became more common; the cultivation of geography followed as a matter of course. Already in the thirteenth century works on geography began to multiply,[5] interest in the subject increased, and progress in knowledge of facts was made, though but little in theory until the beginning of the fifteenth century, when Ptolemy was translated into Latin,[6] which act signalled the commencement of a new era in the science.

To understand how Grecian knowledge was preserved during this period and handed on with additions to the later world, it will be necessary to turn the attention for a moment to another field of activity. The Arabs, converted to Mohammedanism, having made immense conquests in arms and established a great realm, turned a part of their energy to intellectual pursuits. Having produced too little of their own to act as a foundation of future development, they drew from a rich fountain ready at hand—Greek literature and science—which through the medium of translations they soon made their own. Not only did the extent of their possessions and trade demand considerable geographical knowledge, and at the same time provide much of the material for it, but a stringent rule of their religion, requiring all prayers to be said with the worshiper's face turned toward Mecca, made necessary the determination in every part of the realm of the exact direction toward the holy city. Although they were at first guilty of the same fundamental errors as other Oriental nations in conceiving the earth as a great plane or as having the form of a shield or a drum,[1] they came into possession of Ptolemy's work, probably as early as the beginning of the ninth century,[2] and from that time the rotundity of the earth was among them an almost universally accepted fact. That geography received a large share of attention is shown by the considerable number of works on the subject written by them, which have come down to us. One author, citing the Koran as authority, calls geography "a science pleasing to God."[3] They not only established by observation with the gnomon the latitude of a considerable number of places, but discovered also the error of one-fourth of a degree inherent in all observations with that instrument, because the shadow measures the angle to the uppermost edge of the sun instead of its center.[4] Having a passion for astrology and a climate favorable for observing the heavens, they made rapid progress in astronomy, and accordingly in the theoretical part of geography connected with it; but the geographers seem to have been unable both to value this knowledge rightly and to use it in the preparation of their maps. To read the praises of their work by one[5] who has made a specialty of it and constructed maps on nineteenth-century lines as an illustration of their production, must give a false idea of their true position in the history of geographical development. He names Ptolemy's great work a "*monument monstrueux*," and finds the purported translation, known as "*rasm*," much better than the original, the translators having adopted its principles without its errors of detail.[6] Though not a single Arabic map now known has a net representing latitude and longitude,[1] Lelewel says that the "produits cartographiques de cette époque prouvent que toutes les cartes arabes furent élaborées sur les latitudes et les distances."[2] He seems to have been unaware of the fact that the geographers regarded as too confusing—and so neglected to use on their maps—the mathematical determination of places fixed by the astronomers.[3] They employed theoretically the division of the circle into degrees and minutes for giving in lists the position of individual places, but seem to have preferred as a general division of their maps that into climates, of which they reckoned only seven, connecting them with the seven planets.[4] Though in the ninth century they were satisfied with a degree of accuracy in determining latitude approaching to one-third or one-sixth of the truth, they improved greatly later, and made some very exact observations, including two correct to the minute, viz, those of Toledo and Bagdad.[5]

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[1] Lelewel, Br. Ed., I., xix.

[2] *Ibid.*, I, 5.

[3] Santarem, I, 188.

[4] Lelewel, II, 4.

[5] Santarem, III, lv, lvi.

[6] Lelewel, Br. Ed., II, 71.

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[1] Günther, Rundschau, 4, 345.

[2] Peschel, Erdkunde, 132.

[3] *Ibid.*, 105.

[4] *Ibid.*, 134.

[5] Lelewel, Géog. du moyen-âge.

[6] *Ibid.*, I, 24. Contradicting himself he speaks of it as "l'ouvrage géographique d'un anonyme intitulé όρισμος, préférable à la géographie de Ptolémée." D'Avezac, 293, 294, is decidedly of another opinion: "Mais les échantillons de cartographie arabe qui sont parvenu jusqu'à nous se bornent en général à de bien grossières esquisses, sans exactitude en proportions d'aucune espèce." Again: "Il faut bien reconnaitre que leur rôle [des cartes arabes] est absolument nul dans l'histoire des projections terrestres," p. 295.

It is to this people that we must look for much influence, direct and indirect, in the renaissance of classical learning in Christian Europe. As early as in the tenth century we catch a faint glimmer of reflected light in the field of geography in a globe made for Pope Sylvester II,[6] who had studied in Mohammedan Spain. In the middle of the twelfth century there was to be found at the Court of Roger a copy of Ptolemy's geography, and there still exists a thirteenth-century copy thereof in Venice;[7] but being in the original tongue, which was as a sealed book to the Italians of that period, it probably had no influence on the development of the science.[8] The first Latin translation of Ptolemy was made in 1405 by a Florentine named Angelo, and gradually found its way into all the countries of Europe.[9] This was contemporaneous with the opening of the period of oceanic discoveries under Henry the Sailor of Portugal. From this time the influence of Ptolemy was great,[10] gradually driving out the itineraries and works of like class, to make way for those constructed on the more reliable method, because based on scientific principles;[1] and his was destined to become the fundamental work to which the maps of modern geographers might be added as a supplement.[2] If this substitution of Ptolemy's method was at first disastrous in producing maps less practicable and perhaps also less accurate than those in vogue for mariners, it was due to the fact that the mariners had in their compass maps exaggerated the size and importance of the bodies of water at the expense of the land.[3] Accordingly it was necessary to take this apparent step backwards once for all, in order to establish for the future correct lines for the development of the science. For some time there continued to be published maps of the Middle Age type, as Fra Mauro's map of the world, 1459, and Bennicasa's nautical map, *circa* 1476;[4] and even at times such were issued in the same work with re-productions of Ptolemy's maps, as in the work of André Bianco,[5] 1436. But at the end of the fifteenth century the work of the great Grecian was so widely circulated, at least among scientists, so generally accepted, that from that time we may consider the rotundity of the earth as scientifically established[6] in Christendom, and therewith the theory of latitude. That was however only the beginning. The real latitude of but comparatively few places had been determined, and that generally inaccurately, while there still remained the great work of measuring exactly a degree of latitude and thence calculating the size of the earth.

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[1] Peschel, Eudkunde, 146.

[2] Bresl. Ed., I, lxxvi.

[3] Peschel, Erdkunde, 146.

[4] Lelewel, Br. Ed., I, xxxviii. Lelewel, Table v, gives the climates of Abraham Bar Haüa Espagnol, 1136, bordered respectively by the equator and the parallels of 15°, 24°, 30°, 36°, 40°, 45°, and 48°, for which he calculated that the longest day of each would be half an hour longer and the shortest half an hour shorter than that of the next one to the south. Aboulfeda, 1331, gives another division as follows: I, from 12⅜° to 20¼°; II, to 27°; III, to 33⅓°; IV, to $38\frac{2}{10}$°; V, to 43¼°; VI, to 47⅓°; VII, to 50⅜°. (*Ibid.*, Table xiii.)

[5] Peschel, Erdkunde, p. 136 n. 1.

[6] Santarem, I, 184.

[7] Lelewel, Br. Ed., II, 122.

[8] In the mean time much had been learned from the Mohammedans; and Lelewel, I, xciv, finds a direct connection between their work and that of Delisle, D'Anville, and Bonne, and says, also, p. lxxxii: "Les notions cartographiques d'Alfragan, d'Albateni, d'Arzakhel, passaient dans la langue latine, par les ouvrages de Gérard de Crémone (1187), de Sacrobosco (1250), de Bacon (1294), de Cecco (1327).

[9] *Ibid.*, II, 123, 124.

[10] Santarem, I, 177. Lelewel, Br. Ed., II, 104.

Herewith we enter upon a new era in the development of geographical latitude. The first essential for the required accuracy was improvement in the instruments and methods of astronomical observation. The gnomon is at best but a crude instrument, and the practice of waiting for noon on the four days of the year when the sun's position was accurately known hindered the accumulation of observations which is necessary to exactness. Not only were new instruments therefor invented, but the astronomers turned to the observation of the pole star, by which it is easier to determine latitude, and later came the tables of the daily position of sun and stars in the heavens, which enables us to determine with accuracy the latitude any day or night, provided the sky is comparatively free from clouds. Not only is the position of each individual place of importance for the science of geography, but the distance between places, as well as their direction one from another, is also of great weight. So long as navigation was confined to coasting, simple compass-directions and compass-maps answered all necessary requirements; but when mariners began to strike out into the ocean, where they might wander for weeks without seeing land, and it was found that the compass did not always point directly north, but is liable to considerable variation, the old method no longer sufficed; a method of determining latitude at all times became a pressing want, which, as is generally the case, resulted in corresponding new inventions. The natural curiosity of civilized man causes a longing to know the size of the planet on which he dwells. This knowledge can be most readily gained by measuring accurately a degree or rather various degrees of latitude. To accomplish this, governments have provided immense sums of money, and scientists have borne—not only without murmur, but with enthusiasm—the heat of tropical summers and the cold of polar winters, the winds and fogs of the mountains, and deluging rains in the plains, have suffered from hunger and thirst, and even met bravely at their post, the one unconquerable, Death. It is to the contemplation of this gigantic work, already continued for more than three centuries and still being vigorously pursued, that we now turn.

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[1] Lelewel, Br. Ed., II, 125.

[2] D'Avezac, 300.

[3] Lelewel, II, 47, draws the contrast as follows: "La géographie des Arabes, savante mais embrouillée, était éminemment continentale; celle des latins d'expérience, mais régulière, exclusivement nautique. Celle-là, suivant les règles de la haute science, sur des bases vicieuses, fournit des produits variés et discordants, s'emplit d'inextricables erreurs; cette autre, marchant vers le grand chemin, par des sentiers étroits mais bien battus, élabora l'unique produit pour toutes les écoles qui se disputaient l'exactitude de son dessin."

[4] Lelewel, Br. Ed., II, 105.

[5] *Ibid.*, 86.

[6] Günther, Studien, 11.

It may be well however to say first something as to the principles which lie at the foundation of this work.

Position of the tropics.—It was matter of very early observation that the sun changes from time to time its position in the heavens in reference to the stars, and that this change is a regular one, and the period of time occupied in returning to the same position determines the length of the year. The path thus formed is called the Ecliptic.[1] An imaginary line on the surface of the earth formed by connecting those points over which the center of the sun passes vertically at noon on each successive day of the year, receives the same name. In connection with this change of the sun's position, it was noticed that the length of the shadows cast on the earth at noon at different seasons of the year is variable,[2] and this fact gave the first means of determining absolute position on the globe. Noting in Egypt, where no shadow was cast, only on one day of the year, gave the position of the northern tropic. Half the difference between this point and that over which the sun stood perpendicularly when farthest south, (accordingly when the shadow at the first point was the longest), gave the position of the equator, or the middle distance between the poles. So there was one point on the earth, viz, that of the Tropic of Cancer, apparently fixed,[1] and a relation was established between the position of the sun at two other points, and the length of shadows cast on the earth. This may be called the first foundation stone of scientific geography; for without such a sure grounding in nature itself, the science is impossible. Presupposing that the rays of the sun come to us parallel to each other, it happens from the nature of a sphere that the ratio between the length of a shadow at noon, cast by an object perpendicular to the plane of the horizon to the object itself, is equal to the ratio of the distance between the place of observation and the point where the sun casts no shadow. Having thought out this principle, the earliest astronomers waited for noon at the summer solstice to make their observations, and somewhat later came to use also the winter solstice and the equinoxes therefor. But the position thus gained was not absolute and unchangeable, as the ancients supposed.

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[1] Brünnow, Astron., p. 75, defines the Ecliptic as "der Kreis den der Mittelpunkt der Sonne, vom Mittelpunkt der Erde gesehen, im Laufe eines Jahres unter den Sternen von Westen nach Osten beschreibt."

[2] That the sun's course is not parallel to the Equator is said to have been discovered by Anaximander of Miletos (Strack's Plinius, lib. ii, 8. 6. Vol. I. p. 73).

In the first place, accuracy of observation was not possible, owing to defective instruments; they supposed the position of the tropic marked by the position of the upper edge of the sun at the summer solstice, instead of by its center. And in the second place, they could not suspect that the ecliptic plane is itself subject to a secular variation of between one and two degrees in about ten thousand years; as a result of which—during the historical period—the earth's axis has been slowly becoming more perpendicular to the plane of its course around the sun. The obliquity is now 24′ less than it was 2,000 years ago. This causes a slight approach of the tropics toward the equator and of the polar circles toward the poles. Syene, whose position was accepted as marking that of the tropic, lay in 24° 5′ 32″ north latitude, and according to modern calculations was under the edge of the sun at the summer solstice about 700 B. C., when the obliquity of the Ecliptic was probably 23° 51′.[2] Though Eratosthenes determined with a fair degree of accuracy the obliquity at 23° 51′ 19″.5, the ancients generally accepted the round sum of 24 degrees. Later classical authors made no improvement on this, and these figures passed with the remainder of Greek learning to the Arabs. Though the latter added little or nothing to the theory of the Greeks, they were much better observers.[3] Accordingly we find Albategnius in the second half of the ninth century observing with carefully divided parallactic rules the distance between the sun and the zenith at the solstices. The difference he found to be 47° 10′, from which he determined the obliquity of the Ecliptic to be 23° 35′. Delambre names this "the most trustworthy ancient observation," and comparing the result with that of his own work in 1800, determines the diminution of the obliquity at 0″.505 per annum. He adds however that one can not say that the Arabian observation was exact to the minute.[1] Remarking that their result did not agree with that of the Greeks, the Arabs thought that the "obliquity of the Ecliptic oscillates."[2]

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[1] Later it will be seen that this point is not stationary, but slightly movable.

[2] Neglected to note author.

[3] Delambre, Astron. du XVIII^{me} siècle, p. 6.

However, it was not until toward the middle of the eighteenth century that the diminution of the Ecliptic obliquity was generally admitted.[3] Louville was the first who attempted to measure the amount of the movement and announced in 1716 the result, 60″ a century.[4] The elder Cassini believed it to be 45″,[5] which is practically that at present accepted.[6] However, opinions have varied much; Ximines in 1756 calculated it to be 30″.[7] Hornsby (1769) believed it to be no more than 32″ or 34″ in the century, although his observations had given ½″ per annum.[8] Lalande gives 33″.33, Delambre, 46″ to 48″, as the result of observations; but the latter accepts as the best those of La Caille, 44″ *per* century.[9]

It belongs also to the childhood of the race to have noticed that some stars never go below the horizon and that one seems to be stationary, round which the others revolve. This star marks the prolongation of the earth's axis to the north, and pre-supposes its rays to come from an infinite distance and fall on the earth parallel to each other. It happens then, from the nature of a sphere, that it can not be seen from the equator, or further to the south; but in proportion to the distance of the observer north from the equator that star appears above the horizon. This fact gave a second principle founded in nature on which to build the science of geography. This star however is not absolutely stationary in the sky, but itself describes daily a small circle, so a simple method was invented to find the center of this circle, which then should be the absolute north pole. This was to observe the altitude of any one of the circumscribing stars when at its highest and lowest points, *i. e.* at its culminations, and halve the result, which gives a truer north point. It was the good fortune of the English astronomer-royal, Bradley (1692–1762), to discover that even this point is not absolutely fixed, but describes periodically a small ellipse. Even this is not a regular curve, for the line is a wavy one.

Still a third method of determining latitude was employed as early as the time of Poseidonius (first century B. C.) which was however at first so crude as to be perfectly unreliable in its results, but which in the end was so far useful as to call attention to the fact that other stars than the circumpolar ones can be used in determining latitude. This method was to observe certain stars which in one place just graze the horizon and in another appear higher. The altitude in the second place gives the difference in latitude of the two places of observation.[1]

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[1] Delambre, Astron. du XVIII^{me} siècle, pp. 13, 14.

[2] *Ibid.*, p. 200.

[3] *Ibid.*, vii.

[4] *Ibid.*, p. 317.

[5] *Ibid.*, p. 262.

[6] *Ibid.*, p. 406.

[7] *Ibid.*, p. 405.

[8] *Ibid.*, pp. 697, 698.

[9] *Ibid.*, p. 594.

Instruments.—No one art or science is so independent that it can stand or fall, advance or retrograde, without influencing and being influenced by the others. Accordingly we find progress in the knowledge of geographical latitude dependent not only on that of general information regarding the earth's surface, but also on that of astronomy, and even of mechanics, the latter being necessary to increase accuracy of astronomical observations, on which all the rest depends. The beginnings with two kinds of rude gnomons have already been mentioned. To the Chinese was known a third sort, more accurate because provided toward the top with a small hole, through which the sun shone, thus terminating the shadow to be measured at the point corresponding to the center of the sun. The Arabs also made use of this sort of gnomon, but it seems doubtful if the Greeks ever came to a knowledge of it. Eratosthenes employed besides the gnomon the armillary spheres already mentioned, with which he observed the obliquity of the Ecliptic. Further progress was marked by the introduction of the astrolabe, the first mention of which is made by Ptolemy, who says he used one at Rhodes, which however may have been invented by Hipparchus.[2] Between this and modern times, the only improved instruments for observation are the similar but more complicated torquetum of Regiomontanus (1436–1476), and the quadratum geometricum, which was known to the Arabs, but is generally ascribed to the invention of Purbach[3] (1423–1461). In about 1600, observations at sea were much improved by the invention of a portable quadrant by the English sea-captain, John Davis.[4] In the mean time the instruments for use on land were being made larger and larger, and, though admitting of more accurate division, they became unwieldy. Then came the application of magnifying glasses, to which Huyghens first applied the cross-threads to mark the mutual focus of the two glasses of the telescope; and in 1667, Picard first applied (in concert with Auzout) the telescope to the quadrant.[1] The great English astronomer, Hadley, worked a revolution in observations of latitude at sea by publishing in 1731 the description of the octant,[2] now known by his name,[3] though it was probably invented by Newton.[4] By means of flat-surface reflectors the observer is enabled to see apparently in the same line the two objects whose angular distance he wishes to measure. The angle between the two objects observed is measured by double the angle formed by the two reflectors of the instrument, according to a well known law of reflected light, which may be expressed as follows: "The angle between the first and last direction of a ray which has suffered two reflections in the same plane, is equal to double the angles formed by the reflecting surfaces."[5] During most of the eighteenth century, the English not only made great progress in astronomy, but were decidedly the leading mechanicians,[6] so that their instruments were greatly sought for, even in foreign countries. The necessity of having large instruments for observing latitude came to an end with the invention of Borda's circles in 1790.[7] This instrument was not only made with extreme care and with very minute divisions, but its form permitted a continuous series of observations, each angle commencing where the preceding ended, instead of the instrument being turned back to the starting point for each new observation, as was the case with the sectors.[8] In the present century there have been constant improvements and alterations in the forms and accuracy of observing instruments, a closer consideration of which lies outside the scope of this article. The latest improvment in principle seems to have been the invention of a reflection *circle* as a substitute for the sextant, which is provided with two nonii, directly opposite to each other, by which method each counteracts the possible error of the other.[1]

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[1] In recent times it has been customary to observe any selected star, measuring its distance on the meridian from the pole or zenith. The latitude is found by comparing the relative position observed with the star's absolute position, which will be found in tables prepared for that purpose. On account of atmospheric refraction stars near the zenith are now generally chosen for such observations.

[2] Delambre, Astron. ancienne, I, 184.

[3] Wolf, Gesch. Astron., p. 126.

[4] *Ibid.*, p. 378. Encyc. Brit., art. "Navigation," speaking of Davis's *The Seaman's Secrets:* "There is a drawing of a quadrant, with a plumb line, for measuring the zenith distance, and one of a curious modification of a cross-staff, with which the observer stands with his back to the sun, looking at the horizon through a sight on the end of the staff, while the shadow of the sun from the top of a movable projection falls on the sight box. This remained in common use till superseded by Hadley's quadrant."

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[1] Delambre, Astron. du XVIII$^{\text{me}}$, siècle, p. 618, note.

[2] The instrument in reality was only an octant, or formed an arc of 45°, but since, through the principle of reflection, it could measure 90°, it is often called Hadley's quadrant.

[3] Delambre, Astron. du XVIII$^{\text{me}}$, siècle, p. 688.

[4] "He (Newton) also invented a reflecting sextant for observing the distance between the moon and the fixed stars, the same in every essential as the instrument which is still in everyday use at sea under the name of Hadley's quadrant. This discovery was communicated by him to Dr. Halley in 1700, but was not published or communicated to the Royal Society till after Newton's death, when a description of it was found among his papers."—Encyc. Brit., art. "Navigation."

[5] Herschel, Pop. Astron., §157, p. 122.

[6] Zach, Mon. Cor., 1804, p. 277.

[7] *Ibid.*, p. 271.

[8] Delambre (Base du système métrique, I, 97, 98), thus speaks of the possible accuracy of this instrument: "Les deux miens [instruments] étaient divisés en quatre cents degrés subdivisés chacun en dix partiés; ce qui faisoit en total quatre mille divisions tracées sur le limbe. Le vernier les partagoit encore chacune en dix partiés, sans la moindre incertitude, et l'on pouvoit même estimer, sans se tromper de deux en trois, les millièmes de degré. Quatre alidades, placées presque à angles droits, divisoient encore l'erreur; en sorte que ce n'est pas trop de dire que l'instrument donnoit les millièmes de degré. Ainsi, faisant abstraction des erreurs de la division, on auroit un angle à trois ou quatre secondes près, par une seule observation."

With the advance toward perfection of the instruments of observation has naturally gone hand in hand the progress in the accuracy of the astronomical observations themselves. Though Hipparchus was far ahead of his predecessors in the kind of instruments he used, his observations were still liable to an error of half a degree.[2] Lelewel gives the latitude of several places according to two Arabian authors, which vary in each case three (!) degrees or more.[3] There is a long difference between this and Picard's results, whose observations, according to his own statement, did not vary one from another more than five seconds.[4] Somewhat more than a century later Delambre made about twelve hundred observations to determine the position of one of his stations, and found them accord to within half a second.[5] Another series gave a variation of only one-third of a second,[6] and still a third, consisting of eighteen hundred observations, made by Delambre and Mechain, showed a difference of but one-sixth of a second in the results of the two observers.[7] These are the achievements of the most careful observations with the finest instruments. About the same time Bohnenberger, writing of ordinary observations with the mirror-sextant, gives, as the greatest possible error in observing the altitude of the sun, 23.5 seconds,[8] a tremendous advance on classical and mediæval results. However, all modern observers do not find the same happy agreement in their observations as did Mechain and Delambre. Even at the beginning of this century a complaint is made that the latitude of the best astronomical observatories is scarcely within three to four seconds certainly fixed, while that of Paris had varied between 1667 and 1721 a quarter of a minute,[9] and that of Berlin still later varied a whole minute, according to the observations of two astronomers.[10]

Tables of positions of stars.—Of great importance for the determination of latitude, are the tables giving the exact position of the sun and stars. Hipparchus calculated the length of the longest day for each degree of latitude, and in so far used the sun's position for geographical purposes. But it is in modern times that man has first felt the necessity of being able to determine each day and even moment his latitude; and with the occasion of the necessity, were invented the means of meeting it. Not only were new instruments of observation placed at the disposal of astronomers and mariners, but tables of the daily positions of sun, moon, and stars, have been worked out with ever-increasing accuracy, so that what was once a matter of impossibility to the best informed astronomer and mathematician, viz, to determine any day his geographical position on the earth, is now an easy matter for any mariner of fair education. Though such tables were constructed by the Greeks and Arabs, we must look to the French astronomer, La Caille, and the German, Tobias Mayer, for the first approach to accuracy in this regard.[1] The former, using alike the work of his predecessors and his own numerous observations, constructed about the middle of the last century a table of 397 stars, which receives the highest praise from Delambre.[2] Mayer took up the work where La Caille left it, and simplified the mathematical formulæ, receiving a prize from the English government for the benefit therefrom to navigation. This work was then collated with the observations of the astronomer royal of England by the mathematician, Mason, whose name in America is so well known in connection with the Mason and Dixon's line. The accuracy of the tables was thereby increased to such an extent that Maskelyne expressed the belief that the greatest error would not surpass thirty seconds. Later laborers in the same field have materially diminished even this small margin of possible error.[3]

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[1] "Besonders bequem sind die von Pistor und Martin's erfundenen Reflexionskreise, bei denen der kleine Spiegel durch ein Prisma ersetzt ist. Diese haben überdies den Vortheil, dass man damit alle Winkel von 8° bis 180° messen kann" (Brümow, Astron., p. 540).

[2] Delambre, Astron. ancienne, I, xii.

[3] Br. Ed., I, xlviii.

[4] Picard, Mesure de la terre, 22.

[5] Delambre, Base du système, I, 77.

[6] *Ibid.*, 72.

[7] Delambre, Base du système, I, 94.

[8] Ortsbestimmung, 145.

[9] Zach., Mon. Cor., April, 1804, p. 270.

[10] *Ibid.*, p. 284.

Refraction —Several causes united to make possible a degree of accuracy which for ages was held to be unattainable. Among the most potent of these factors were the discovery by Bradley of the aberration of light and the nutation of the earth's axis; and the great progress made in determining accurately the amount of refraction in connection with astronomical observations. As the latter was a matter of slow development, it may be advantageous to consider it somewhat more in detail. Astronomical refraction has been defined as the amount which the rays of light are bent from their entrance into the atmosphere to us.[4] The first recorded notice taken of this phenomenon was about the time of the Christian era, when Cleomedes, in his work Circularis Inspectio Meteorum, relates an account which he had received of an eclipse of the moon taking place when both sun and moon were visible above the horizon. Though disposed to doubt the truth of the story, he gives a possible explanation of the phenomenon in remarking: "Even as a ring in a vessel will be raised visibly above the edge by water poured in, so can the sun be seen by refraction when it is in reality still below the horizon."[5] The fact of refraction was known to Ptolemy, and that its quantity decreases from the horizon to the zenith, where it disappears.[1] However, though he had a better idea of the subject than the earliest of modern astronomers, he failed to discover any of the laws of its action.[2] The subject was merely mentioned by Sextus Empiricus, in a work entitled Adversus Astrologus (in the third century);[3] was taken up by Alhazen about 1100, in his work on optics; then remained unnoticed till the Nuremberger astronomer, Walther, about 1500, commenced to estimate, at least near the horizon, the effects of refraction.[4] He was probably the first who ever really observed astronomical refraction, which he had done before becoming acquainted with the ancient works wherein it is mentioned.[5] About a century later Tycho Brahe was lead to a re-discovery of the phenomenon while trying to determine the latitude of his observatory. For greater accuracy he made observations of a circumpolar star and of the sun at the solstices, and found a difference of four minutes in the results. In a new instrument, built with more care for similar observations, showing the same divergence, he believed the difference to be caused by refraction, and fell to making a special study of the subject, from the results of which he constructed the first table of astronomical refractions; but he not only missed an important truth, which had been already guessed by Ptolemy, viz, that refraction ceases only at the zenith, but he believed that refraction for the sun and the stars ceases at different altitudes, and found the latter at an end of 26 degrees above the horizon, while at 45 degrees the sun's rays still suffered a refraction of five seconds.[6] Here was indeed a valuable beginning, but the whole matter rested upon an empirical basis; for as yet there was no conception of the laws of the action of light. The first of these, that of the relation of the angle of incidence to the angle of refraction, was first published by Descartes in his Dioptrice, though the honor of precedent discovery belongs to Snellius, who unfortunately died before he could publish his work.[7] Tycho himself remarked that refraction is not always the same, but offered no explanation of the fact. Cassini and Picard found that it was greater in winter than in summer, by night than by day, and by a happy chance the latter was led to the true explanation of this irregularity; for, observing once at sunrise, he found the horizontal refraction suddenly change twenty-five seconds, which could have no other cause than the increased warmth of the air,[8] caused by the appearance of the sun. Two other laws of nature discovered about the same time aided materially in increasing the knowledge of astronomical refraction, the first being the one discovered by Edmund Mariotte, that the density of the air is proportional to the pressure resting upon it, and the second, that established by Hawksbee's experiment in 1702, viz, that the refracting power of the atmosphere is as its density.[1]

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[1] Delambre, Astron. du XVIII^{me} siècle, viii.

[2] *Ibid.*, 515.

[3] *Ibid.*, 634.

[4] *Ibid.*, 717.

[5] Bruhns, Strahlenbrechung. 6.

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[1] Delambre, Astron. du XVIII^{me} siècle, 774.

[2] Bruhns, 8. [3] *Ibid.*

[4] Delambre, Astron. du XVIII^{me}, siècle, 775.

[5] Delambre, Astron. du moyen-âge, 339.

[6] Delambre, Astron. du XVIII^{me} siècle, 775.

[7] Bruhns, 24, 25. [8] *Ibid.*, 31.

In order to render practical—for determining astronomical refraction—the knowledge gained by these various discoveries it was necessary to have the means of arriving at the amount of pressure on the great body of atmosphere through which the light comes to the observer, and also of measuring the temperature of the atmosphere. These means were provided just at the right time by the invention of the thermometer by Drebbel in 1638, and its perfecting nearly a century later by the labors of Fahrenheit and Réaumur; also by the (if possible) still more valuable invention of the barometer by Torricelli in 1643. Thus at the beginning of the eighteenth century we find all the means at hand, by which the cause and amount of astronomical refraction were to be determined with the greatest accuracy, supplying the last requisite to the exact determination of geographical latitude. Halley, by his observations in 1714–'15, was led to the conclusion that the change of refraction is proportional to the change in the height of the barometer,[2] which was for astronomical refraction only the confirmation of the law already proved for terrestial refraction by Hawksbee, viz, that refraction is proportional to the density of the atmosphere, while later in the century Euler called attention to the fact that refraction is almost exactly in the inverse ratio of the degrees of heat, when the star is not too near the horizon.[3] Thus a large body of facts had been gradually brought together, which was then systematized, and thereby was made possible the establishing of theories as to the action of light on its way through the atmosphere. They in turn became the foundation for calculating accurately the amount of refraction when the conditions of the observations were known. The details of the matter lie outside of the scope of this article; but a few of the most important points are worthy of a moment's attention. Though Kepler brought his great talents to bear on the subject he failed to add anything to its elucidation.[4] Tobias Mayer (already mentioned) was the first to give a rule for calculating the amount of refraction in connection with the variation of the barometer and thermometer.[5] Oriani in 1788 showed that the change of density of the air is without influence on refraction from the zenith to a distance of 70 degrees[6] (though of course the regular increment of density and refraction continues), and Laplace showed that the same is true to 74 degrees,[7] and in fact he takes no account of the variation of density in his formula up to 80 degrees.[8] This probably accounts for the fact that some early tables were quite accurate up to that point, as was the case with Kepler's.[9] The genius of Newton and of Huyghens was brought to bear also on this subject, and though starting with an entirely different hypothesis, their results were both quite accurate. The former, believing light to be a material substance, supposed that the heavier medium attracted more powerfully and thus produced refraction; the latter, having proposed the wave theory of light, taught that it is more difficult for the waves to force their way through a dense medium than through one less dense; that their motion is thereby retarded, and when striking the denser medium at an oblique angle that their direction is changed.

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[1] Bruhns, 40.

[2] *Ibid.*, 41.

[3] Delambre, Astron. du xviii^{me} siècle, 785, note by Mathieu.

[4] Bruhns, 15.

[5] *Ibid.*, 78.

[6] *Ibid.*, 74.

[7] *Ibid.*, 36.

[8] *Ibid.*, 119.

[9] *Ibid.*, 19.

The earliest astronomers who considered the subject supposed the atmosphere of the same density everywhere, and hence presumed that the light-ray was only bent once and formed a straight path through the air; but from the time of Newton and Huyghens this idea was shown to be untenable and that the path is really a curve. What the exact nature of this curve is it is extremely difficult to determine, on account of the presence of a large number of possible disturbing elements. With the gradual increase of accuracy, the older tables of Cassini, Bradley, Berg, and Burckhardt and others have given way, one after the other, to ever newly appearing ones. The present ones of Laplace, Bessel, Schmidt, and Ivory are models of accuracy and fully answer the needs for furnishing the amount of refraction in all cases.[1]

Progress of determining the latitude of fixed points.—At the renaissance of geography, almost the entire political face of Europe was found changed from what it had been when Ptolemy's work was written. In that work but few latitudes had been given, and these were generally seriously lacking in exactness. The Arabs had added considerable thereto, but in the region foreign to those with whom the advancement of the science now rested. At first the modern geographers dared not dispute the authority of the great master, and the first to print a map with corrections offered therewith excuses for breaking with the tradition. The need of accurately fixing latitudes was soon felt. The gnomon was revived and made larger and better. Even as late as the elder Cassini, a gnomon of 20 feet in height was used to determine the latitude of Bologna;[2] and still later (1744) Lemonnier added to his gnomon a burning glass of three inches diameter and 80-foot focus, by means of which latitude could be as accurately determined as by the great quadrants of the time.[3] But the days of gnomons were numbered, and the newer instruments took precedence. There followed a general improvement in the determination of latitude by observation, a few examples of which may be of use to show the rate of progress.

Surveys.—In the history of geographical latitude the most conspicuous rôle has been played by the surveys which have been made to determine the length of a degree, and therefrom, by further deductions, the size and form of the earth. The principal aim of this great work was, to be sure, purely scientific; but that it has a most practical application also is pithily stated by Maupertuis as follows: "Sur des routes de 100 degrés, en longitude, on commettroit des erreurs de plus de 2 degrés, si naviguant sur la sphéroïde de Newton on se croyoit sur celui du livre *De la grandeur et figure de la terre*: and combien de vaisseaux ont péri pour des erreurs moins considérables.[1] For ages those who believed in the rotundity of the earth thought it to be a perfect sphere; but in the seventeenth century the two great physicists of the day, Newton and Huyghens, again starting with different hypotheses, came to a like result, viz, that the earth is not a true sphere, but rather a spheroid, flattened toward the poles. The announcement of these theories, nearly at the same time (1686 and 1688, respectively), caused great excitement among the astronomers and geographers; two camps were formed contending respectively for the old and the new theory and each demanding proof of its theory by surveying: for if the earth is flatter toward the poles, a degree of latitude will be longer near the pole than near the equator. France, which was already the leading country in such work, offered a good field for it; the French astronomers were eager to undertake it, and Louis XIV authorized the survey of a meridian extending throughout the land. The northern and southern stretches being put under leading specialists for survey, the result showed the degree to be longest in the southern section; a repetition only confirmed the result. If the surveys were correct, this proved the earth to be *elongated* toward the poles, instead of flattened. Great was the delight of the French at this defeat of the foreign theorists. But the latter contended that the matter was by no means settled; that the only sure method would be to make surveys near the equator and near the pole, and await the result. This plan was carried out during the first half of the eighteenth century, and resulted in the complete triumph of the defenders of the theory of flattening toward the poles. This point settled, the earth was still supposed to be a perfectly symmetrical figure; but as surveys became more extensive and more accurate there appeared inequalities which were not compatible with that theory, and it is now generally accepted that beyond the irregularities of mountains and valleys, discernible to every eye, the geodetic form of the earth is also irregular.

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[1] Bruhns, 181.

[2] Cassini, Grandeur de la terre, p. 375.

[3] Delambre, Astron. du XVIII^{me} siècle, 180.

We have already seen that astronomy gives us the means of determining accurately and with comparative ease the latitude of any one place. If then the latitude of two places which are on the same meridian is known and the distance between them is accurately measured, supposing the earth to be a perfect sphere, a simple arithmetical calculation will give the earth's circumference; for the latitude being expressed in degrees, of which 360 form the circumference, the distance between the points of observation will be to the entire circumference as the difference in latitude is to 360 degrees. The earth being a spheroid makes the calculation much more complicated, though the same principle of proportion between distance and degrees of arc remains as the basis thereof. But inasmuch as it is known that the earth has considerable irregularities, it is not possible to determine its exact form from one or two measured lines; but for entire accuracy the whole surface of the land should be surveyed. To carry out as far as possible this object, there was formed in 1861, the Middle European Commission for measuring degrees.

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[1] Maupertuis, Œuvres, III, 82.

This commission working steadily from year to year will gradually cover the entire surface of Europe with a net-work of surveyed triangles, and thus be able to add greatly to our knowledge of the true form of the earth.

The improvement in measuring distances on the surface of the earth has kept equal pace with the advance in astronomical accuracy. The ancients accepted the distances as reckoned by travellers or at best the land measurement of government employés on a line not straight. The Arabs measured one or more lines of a degree's length, by what means we do not know, and with so little accuracy that there was a difference in the results of three-quarters of a mile. The first measurement of modern times was made in the sixteenth century by counting the revolutions of a carriage wheel on an ordinary road. Finally in 1615, in The Netherlands, was made the first application of trigonometry to land measurement, and though the result was not nearly so accurate as that of the wheel measurement, a new principle had been introduced, which was destined to revolutionize investigations of this nature, and by gradual improvement in its application, to furnish results so accurate as to leave practically nothing to be desired. This principle is that which forms the foundation of trigonometrical science, viz, that the value of a line and two angles of a triangle being known the other quantities can be determined. From that time this has been the method employed, with the exception of the attempt by Mason and Dixon to measure an entire degree in Pennsylvania, which failed to produce an accurate result, and of Norwood's survey from London to York in 1634–'35. The best method having been discovered, there remained still the possibility of enormous progress in accuracy, especially in two directions: (1) in measuring the angles of the triangles; (2) in measuring the base-line. These have now been brought to such perfection that a leading authority is of the opinion that further improvement in this direction can not increase the accuracy of the result so much as the outstanding uncertainty due to irregularities in the form of the earth.[1]

Before passing to a chronological consideration of the most important surveys made to determine the length of a degree, it may be well to say something of the work in connection with measuring a base line and fixing and measuring the triangles connected therewith. In the first place, choosing the ground for the entire survey is no easy matter. Two points nearly or quite on the same meridian should be sought, which are favorable to the astronomical observations necessary to determine their latitude; near the course of the line there should be a convenient plane, as flat as possible, advantageous for running the base line in a direction favorable for connecting it with the main series of triangles, while the tract as a whole should be of such a nature as to permit the making of triangles as large as possible, *i. e.*, furnishing high points at long distances with uninterrupted view, with all the angles as large as possible, for the measurement of very acute angles is more liable to error than that of large ones.[1] The situation of the base line once selected, its end points are marked with the utmost care by contrivances which vary according to the ideas of the surveyor. In order to keep the true direction with perfect accuracy while measuring, the line is staked out with the utmost care, wooden stakes being driven into the earth at short distances, and nails driven into the top of the stakes to mark the exact point where the line passes. Then follows the actual work of measuring. That this may be accomplished with the most extreme accuracy, a great variety of rules have been constructed from time to time, the standard of length generally being the toise of Paris, or of Peru, as it was called after the completion of the Peruvian survey. The first care is to procure a rule as nearly as possible just so long or double so long as the standard and determine its absolute length. Then, since the substance thereof (of whatever sort) is liable to variation of volume dependent on change of temperature, experiments must be made to establish the amount of this variation, and a thermometer is so attached as to give the temperature of the instrument. There must be an attachment to the rule which enables it to be placed at a perfect level, or if not, then to measure the amount of the declination. An arrangement for placing it firmly on the ground and at the same time for changing its level must also be thought out and constructed. Furthermore, since it is practically impossible to place one heavy rule against another without displacing more or less the one already *in situ*, ingenuity has been taxed to invent methods by which the main rules could be placed in line without touching and the small intervals be accurately measured. This difficulty can be obviated by the application of the principle of wheel measurement, by which, a perfectly smooth way being constructed between the two points whose distance is to be measured, a cylinder, of special construction, the length of whose circumference is accurately known, is rolled over the way, thus producing the effect of a continuous rule. This has been applied with good result, though it has not been used frequently. As the work of measurement can not be completed in one day, it is necessary to mark with care the place of quitting at evening, for commencing in the morning. This is done, when measuring with rules, by dropping a plumb line from the end of the last rule placed at evening and marking the spot where it falls on a plate sunk into the ground, which is then carefully covered, so that neither storm nor wandering beast can change its position during the night. With all these precautions and the most careful noting of every circumstance that can in the least influence the result, the line is generally measured twice, and as a control of the accuracy of the measurement, there is often measured a second line distant from the first.

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[1] Bessel, Gradmessung, 428.

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[1] Bouguer (Figure de la terre., p. 79) says in this connection: "Ces erreurs, quoique les mêmes, produiront cependant ensuite différens effets, selon que les angles seront *plus* ou moins grands; une minute apporte beaucoup plus de différence dans le sinus d'un petit angle que dans le sinus d'un grand; et les côtés qu'on calcule par le moyen des triangles, doivent être sujets à la même erreur que ces sinus, puisqu'ils changent dans le même rapport." See also pp. 83, 85, 88.

Added to this is the labor of running the net of triangles between the ends of the line to be surveyed and connecting it, as accurately as possible, with the base line. The points for the angles of the triangles having been selected, there are the necessary preparations for observations to be made, including, where necessary, the building of wooden towers of greater or less height. As the summits of mountains are frequently selected therefor the difficulties of procuring all necessaries of life and for the work are thereby materially increased, while the violent action of the elements often disturbs and sometimes destroys the work of a long period. The stations established, it remains to measure the angles, not only once, but a number of times; and not only two angles of each triangle (which theoretically would be sufficient), but in order to insure the greatest possible accuracy, all three are measured with extreme care. For this work, the improvement in signals and instruments has been immense.

These surveys have led to some valuable observations on the varying conditions of the atmosphere. Perhaps the most interesting of these after those relating to refraction are those which prove a periodic disturbance or movement in the atmosphere, even on so called still days. There are four phases daily, two of agitation and two of quiet. The period of greatest quiet or of least motion is in the night, for which reason it has often been found advantageous to make the observations at that time, using artificial lights as signals.[1]

After the angles have been measured it is necessary to calculate the difference in altitude of the various observing points, in order to reduce all to the same plane, when these, together with the base line itself, are reduced by elaborate mathematical calculations to the level of the sea, on which level and changed to an arc the length of the line whose value is sought is determined.

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[1] An excellent illustration of this was given in carrying the French-Spanish meridian to the Balearic Islands. "Die Dreyecks-Seite von *Sierra Morella* bis *Silla de Torellas* auf Mayorca beträgt 93080 Toisen. Ein einziges *Reverbere* auf Mayorca hat man auf der spanischen Küste mit Fernröhren drey Stunden lang gesehen." Zach, Mon. Cor. Junius, 1803, p. 569. As early as the time of Picard it was found advantageous to use fire signals at night. See Picard, Mesure de la terre, p. 9.

There still remains the astronomical determination of the latitude of the end points of the surveyed line, which is done by a great number of observations, the mean is accepted as the final result. The difference in latitude compared with the length of the surveyed line, gives the mean length of a degree of latitude in the meridian line surveyed.

More than two thousand years have elapsed since the first scientific attempt to measure the size of the earth. Interest in the matter has increased with the spread of knowledge, while vast improvements in science and art have gradually rendered possible the solving of the problem with great accuracy. The commencement of this grand work was made by Eratosthenes (276–196 B. C.), the librarian of the famous library at Alexandria and a man whose talents found occupation in several fields of science and art. Accepting the common belief that Syene was on the Tropic, he observed at Alexandria with the hollow gnomon at the summer solstice and found the shadow to be one fiftieth of the circumference. The distance between the two places he valued at 5,000 stadii, from which he deduced the earth's circumference, 250,000 stadii. How he arrived at the valuation of 5,000 stadii is matter of dispute. General Baeyer, according to whose calculations there was a failure in the given size of the earth's circumference of only 8 geographical miles,[1] says he measured with rules, but fails to give any authority for the statement; against which we have the authority of Martianus Capella for the much more probable statement that he learned the distance "*per mensores regios Ptolemæi*,"[2] and of Strabo, who says he calculated it from the course of the Nile,[3] while Kiepert is of the opinion that he reached his result by comparing many inaccurate lengths of the ways, gathered from various sources;[4] to all which may be added the assertion of Marcian of Heraclea, that he stole the whole calculation from his predecessor, Timosthenes.[5] However this may be, the result remains the same, that in Eratosthenes's time there was an attempt by scientific method to arrive at the size of the earth, by which the circumference was determined to be 250,000 stadii, to which he added 2,000 stadii, in order to have the convenient number 700 for the length of each degree.[6] One is naturally curious to know how this first essay compares with those made recently with the application of all the means for securing accuracy known to modern science. The comparison is not however so easy as at first thought appears and leading specialists are by no means in unison on the matter. The real difficulty lies in our not knowing the value of the stadium which was employed as the standard of measure.[1] In order to bring harmony into classical accounts of distances, French authorities adopted the idea that there were different standards in use, even to the number of eight.[2] German investigators were rather inclined to scout this method as an easy one for avoiding a dilemma, while an English writer puts all differences down to ignorance on the part of the Greeks, and adds: "The more frequented the route, the more populous the country through which it passed, the more civilized and lettered the people, the more nearly we find the reported distance to approach that standard [600 Gr. feet] of the stadium."[3] But the excavations at Olympia have revealed the fact that the stadium there was 192.27 meters long, instead of 176.76 meters, as was earlier thought to be the case;[4] and Dr. Dörpfeld, one of the directors of the excavations, assures the writer that this length differs from that of all other stadii found in Greece. This proves at least that there existed in Greece itself different standards; but whether there were five or eight, or more or less,[5] it is not within the province of this article to decide. The idea seems to be modern; for, though some classical writers seem to employ different standards, they do not expressly mention the existence of such. Humboldt says he finds the first trace of the opinion broached by Mossen Janne Ferrer in a letter to Columbus on the means of tracing with precision the line of demarkation which should divide the globe between Spain and Portugal.[6]

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[1] Behm's Geog. Jahrb., 1870., III, p. 155.

[2] Zach., Mon. Cor., 1811, p. 469.

[3] Ideler, Zach, Mon. Cor., May, 1811, p. 469–70. "Nach Strabo rechnete er auf den Lauf des Nil von dem kleinen Kataracte in der Gegend von Syene bis an seinen Ausfluss 5,300 Stadien. - - - Denn *Danville* versichert durch ein genaues Studium des ägyptischen Terrains zwischen Syene und Alexandrien nach der Richtung der Wege 640 römische Meilen oder 5,120 Stadien, und in gerader Richtung 560 römische Meilen oder 4,480 Stadien gefunden zu haben."

[4] Alte Geog., 5.

[5] Journal R. G. S., 1839, IX, p. 7, note.

[6] Delambre, Astron. ancienne, I, 221. Lelewel, Géog. du moyen-Âge, ix. Sprenger, Ausland, 1867, p. 1067. Grosskurd's Strabo, I, 214, 215; lib. ii, Abt. iv, § 25.

With these preliminary remarks we turn to the conclusions arrived at, which vary according to the hypothesis of each writer. The two extremes are represented by General Baeyer and Sprenger. The first finds an error of only one seven-hundredth of the distance measured, and says this proves that the ancients went to work with great care and understood quite well how to measure,[7] while Sprenger maintains that Eratosthenes simply re-discovered the method for measuring the size of the earth, and proved it mathematically; that he however made no actual survey, but relied blindly on the map of the world.[1] Between these two extremes are found those who prefer to take the results as they are reported to us, and interpreting them in accordance with what is known as to the then condition of science, come probably nearer the truth. Thus attention is called to the sources of error in the work: that the length of the arc between the end points, Alexandria and Syene, was accepted as much greater than it is, Syene lying north of the Ecliptic; that these places are not on the same meridian,[2] and, furthermore, that no account was taken of the apparent radius of the sun. With these important sources of error at hand it may well be accepted in accordance with the opinion of Lepsius,[3] Günther,[4] and Ideler[5] that the error amounted to about 14 per cent., for which however considering all the circumstances, we are not justified in complaining of the work.

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[1] Quant à sa division du degré en 700 stades, elle n'a pour nous aucun sens, puisque rien ne détermine le stade dont il s'est servi" (Delambre, Base du système métrique, i, 3). "Auch wissen wir nicht ob er ägyptische oder olympische Stadien meinte, oder ob er wie Ptolemäus zwischen beiden keinen Unterschied machte" (Sprenger, Ausland, 1867, p. 1065).

[2] Ideler, Zach, Mon. Cor., May, 1811, p. 456.

[3] Journal R. G. S., 1839, ix, 11.

[4] Lelewel, Géog. du moyen-âge, xxi.

[5] Ideler, Zach's Mon. Cor., May, 1811, p. 456. Grosskurd's Strabo, Vorrede, i. §. 10. p. lxiii.

[6] Humboldt, Géog. du xv^{me} siècle, ii, 327–328.

[7] Behun's Geog. Jahrb., iii, 1870, p. 155. Add quotations from Pt. i, p. 17a.

Though this was the nearest approach to the truth made in ancient times, there were other figures accepted by various classical writers, and one which later received more general acceptance among the ancients and exercised more influence in modern times. This is that which gives to the earth the circumference of 180,000 stadii, generally attributed to Poseidonius, and probably verified by Simplicius. The former enters into considerable detail also as to how he arrived at another result, 240,000 stadii,[6] and then he rejects it without explanation in favor of the much smaller and less accurate estimate, namely 180,000 stadii. Simplicius, on the other hand, describes minutely the manner of arriving at this result. The astronomers sought out, he says, two stars exactly one degree apart, found the places where these stars passed through the respective zeniths, and by "operations" measured the terrestrial distance, finding it to be 500 stadii.[7] Delambre is positive that this was only imagined and never carried out.[8] But why not? According to a calculation of Alexander von Humboldt, this is as nearly accurate within 7,000 toises (though on the other side), as the measurement of Eratosthenes.[9] When one considers the great crudity of the means then at command, it is a perfectly possible result of an actual measurement. If it rested on no good foundation, how came it that the great authority of Eratosthenes and Hipparchus, still followed in other respects, was rejected by Ptolemy and his successors as to this?

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[1] Ausland, 1867, p. 1066.

[2] Grosskurd's Strabo, lib. i. Abt. iv, p. 99, An. 2.

[3] In Zeitsch. für aegypt. Sprach und Alterthums-Kunde, xv. p. 7.

[4] In Rundschau für Geog. und Statistik, III. Jahrg. p. 335, where he gives a masterly résumé of the subject.

[5] Zach, Mon. Cor., May, 1811, p. 474.

[6] Sprenger, Ausland, 1867, p. 1067. This is the value accepted in Babylon three hundred years before Christ. *Ibid.*, 1068.

[7] Ideler, in Zach's Mon. Cor., May, 1811, p. 478, 479.

[8] Astron. ancienne, I, 304.

[9] Humboldt, Géog. du xv^{me} siècle, II, 326, 327.

Aristotle and Archimedes, for instance, thought the earth to be much larger than the measurement of Eratosthenes showed it to be; and their abstract calculations, notwithstanding their general authority, gave way in this matter before the better grounded result. Is it likely that this latter would in turn be rejected without what was at least supposed by the learned to be equally well-founded reasons?

Thus stood the matter at the close of classical times; and the literature of the subject was inherited by the Arabs, when they reached their flourishing period, along with the great mass of Greek learning. One of the early caliphs, Almamoun (813–833), who took an active interest in scientific matters, ordered (A. D. 827) the measurement of an arc of meridian in order to determine independently the size of the earth. Several different accounts of how this order was carried out have come down to us and vary considerably the one from the other. There seem however to have been two separate and distinct measurements made, resulting respectively in 57 and 56¼ Arabic miles for the value of a degree of latitude.[1] As the mean, 56⅝ miles was adopted officially for the length of a degree. But what was the true length of the Arabic mile? It contained 4,000 so-called black cubits; a cubit, 4 palms; a palm, 4 polles; and a polle, 6 barley grains laid side by side. But all barley grains are not equal in thickness and as this variable quantity is the foundation of the numerical system, modern experts have differed greatly in the resulting value of a degree of latitude, the one giving it at 54,563 toises, another at 63,750 toises, etc.[2] The method of procedure was in each case as follows: Two parties were sent to the same starting point and, having there observed the altitude of the sun (by what means is not known), the one party measured[3] toward the north, the other toward the south, till by new observations they found they had advanced 1 degree, where they ceased work and reported the distance measured.[4] Ebn Jounis, who furnished the most detailed account of the operation, describes also the precaution necessary to insure accuracy in the result, but leaves us in the dark as to whether or not these precautions were actually observed.[5] Albategnius, who in most respects was among the most accurate of the Arabic scientists, gives the length of a degree at 85 miles, which however may be the result of a typographic error in the printed translation;[6] while Edrisi is said to have given 75 miles to the degree,[1] and Aboul Hassan, 66⅔ miles.[2] The measurement of 56⅔ miles to the degree, which seems to have been most generally accepted by the Arabs,[3] is nearer the truth than either of the two generally accepted classical ones, as the error is probably only one-tenth.[4]

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[1] Delambre, 66, quotes "Abilfedea" (Aboulfeda) as saying that the astronomers found only 56 miles for the value of a degree.

[2] See Posch, 28 *et seq*. Delambre, Astron. du moyen-âge, 66.

[3] Peschel (133) and Delambre (78) give different accounts of the manner of measuring the line, the one by rules and stakes, the other by long cords, placing each time the end of the last at the middle of the preceding to preserve a straight line. Delambre gives another version, pp. 97, 98.

[4] Delambre, Astron. du moyen-âge, 78; Bauernfeind, Die Bedeutung moderner Gradmessungen, p. 10; Posch, Geschichte und System der Breitengrad-Messungen, 28 ff; Peschel, Erdkunde, 133; Günther, Studien, etc., 59, 60.

[5] Delambre, Astron. du moyen-âge, 78.

[6] Delambre, Astron. du xviii^{me} siècle, 15.

This, then, is the best result of Mohammedan science added to the knowledge of classical antiquity. Christian Europe had as yet done nothing toward solving this problem; was even disposed to regard as heretical a belief in the foundation principles thereof, until circum-navigation of the globe proved beyond the possibility of a doubt the rotundity of the earth. In the same decade in which the first voyage round the world was completed, Jean Fernel, a French physician, undertook to measure a degree of latitude, whose result, by a marvellous series of compensating errors, turned out to be extremely accurate. The actuality of this measurement also has been doubted;[5] but his relation is so circumstantial in all its details that it seems difficult to doubt his having in reality carried out the plan he describes. Remarking that different authors give varying lengths of the degree of latitude and that even among the best, one did not know which to choose, he determined himself to make the experiment, and he found by a careful calculation the length of a degree to be 68 Italian miles, 95¼ feet, which equal 544 Roman stadii, 45$\frac{17}{40}$ feet.[6] Taking from Paris a carriage on which he had made an attachment for counting the revolutions of one of the wheels, he drove on the road toward Amiens which led directly north, until by observation he found he had passed a degree of latitude. His carriage wheel had made 17,024 revolutions. The diameter of the wheel was 6 feet and a little more than 6 digits; hence the circumference 20 feet or 4 paces; which gave for the whole distance, 68,096 paces or 68 Italian miles, 90 feet,[7] which, reduced to toises and taking into account the alteration made in the length of the standard toise in 1668, gives 57,070 toises for the length of a degree in the latitude of Paris.[8] Bessel calculates that the true length is 57,055 toises.[9] According to Picard's calculation, Fernel measured a line of only 56′ 36,″ which shortcoming was compensated by calculating the direct distance too long.[10] Peschel[11] calls attention to the fact that he made an error of twelve minutes of arc in his latitude of Paris, and Bauernfeind[1] calls it "einen völlig werthlosen Versuch die Grösse der Erde zu messen." Fate was kinder to Fernel than his critics, and he cannot be deprived of the unique honor of having first given to the world the nearly exact length of a degree of latitude.[2]

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[1] Lelewel, Bres. Ed., I, lviii–ix.

[2] Delambre, Astron. du moyen-âge, 188.

[3] *Ibid*, 66.

[4] Peschel, Gesch. der Erdkunde, 135, following Boeckh's calculation. Bauernfeind thinks only 6 to 7 per cent. Die Bedeutung moderner Gradmessungen, p. 11.

[5] By Snellius; and Peschel thinks the suspicion "nur allzu begründet" (Erdkunde, p. 394, n. 3).

[6] Lalande, quoting Fernel, Histoire de l'Académie des Sciences, 1787, p. 217.

[7] *Ibid.*, p. 219. This is 5¼ ft. less than above given on p. 217.

[8] *Ibid.*

[9] Kalender, etc., von Sachsen, 1876, p. 58, or 57,057 toises, Peschel, Erdkunde, 394, An. 2.

[10] Picard, Mesure de la terre, 28.

[11] Erdkunde, 394, n. 3.

Almost a century later (1615) Willibrord Snellius, a famous Dutch physicist, made his celebrated trigonometrical survey of a line between Bergen-op-Zoom, Leyden, and Alkmaar, which line, according to his calculation, has a length of 34,018.20 perches of the Rhine. The distance in latitude he determined at 1° 11½′; and this, compared with the distance surveyed, gives a mean value of the degree of latitude of 28,500 perches=55,100 toises of Paris.[3] The result was, according to more recent calculations, 2,000 toises too short, an error of nearly 3½ per cent.[4] He himself was persuaded that the result was not accurate and therefore undertook a second survey in 1622, but was prevented by sickness and death from completing it with the necessary calculations.[5] The base line was only 326.43 perches long (=631 toises and about 1 foot), an extremely short scale, considering the imperfection of his instruments and the fact that his triangles contained very many acute angles.

The first English survey of a degree of latitude was made by a sailor, Richard Norwood, who determined astronomically the latitude of London and York, then measured with a chain the intervening distance, allowing in his calculations for the unevenness of the land, the windings of the way, and also for refraction, declination, and parallax. He found the difference in latitude to be 2° 28,′ instead of 2° 25′. His measurement gave 367,176 feet for the degree of latitude, which, allowing 2.1315 English yards to the toise, gives 57,420 toises as the value of a degree.[6] He had been led to make the survey from the practical need of information as a mariner, and published the results thereof in 1637 in a work entitled "The Seaman's Practices."[7]

About the middle of the seventeenth century (1645) Father Riccioli, an Italian Jesuit, ran a series of triangles in the neighborhood of Bologna, measured a base of 1,094 paces 2½ feet, and by observations of a number of stars for determining the distance in latitude between the end points of his line, arrived at very different results, so that taking them separately the value of a degree would vary, according to Cassini, from 56,130 toises to 62,000.[1] Riccioli's own calculations, combining all the elements, gave 62,650 toises for the value of a degree, an error of 10 per cent.[2] He seems not only not to have observed all three angles of each triangle, but also to have employed very acute angles, even as small as two degrees, which practice, as before remarked, increases greatly the liability to error.

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[1] Bedeutung, etc., 39 n. 22.

[2] Lalande, Mém. de l'Acad. Roy., 1787, p. 222; Jordan, Vermessungskunde, II, 4.

[3] Cassini, Grandeur, etc., 364.

[4] Peschel, Erdkunde, 396. Bauernfeind (as above, p. 39, n. 20) says ½ of the error was in the geodetic work, ½ in the astronomical observations.

[5] Bauernfeind, 15, 16.

[6] Peschel, Erdkunde, 395, n., quotes Maupertuis, Figure de la terre, p. viii, as giving as the result of this survey 367,196 feet=57,300 toises for the degree of latitude, an evident arithmetical error in reducing to toises, even if the number of feet given were correct.

[7] The writer adopts the account given in the Encyc. Brit., art "Navigation," and Jordan, Vermessungskunde, II, p. 78, for the value of the toise.

The year 1669 is memorable as that of the first survey which is used as a starting point for the best of modern work. It was conducted by Jean Picard, a French physicist, who, by improvements in instruments and methods, showed a decided advance over any of his predecessors. The line extended from Malvoisine to Sourdon in Picardy, which points were connected by a series of thirteen triangles, two of which were principally for verification.[3] Cassini complains of him that he observed only two angles in each two triangles, and only one angle of a third triangle.[4] This serves to show that up to that period even the most careful savant had not arrived at the recent conception of extreme accuracy. Cassini himself, as we shall see later, made a survey which was by no means faultless. Picard based his calculations on the measurement of two base lines, each of which he measured twice. The principal one was on the highway from Villejuive to Juvisy, the advantage of which for the base line was one of the reasons for choosing this region for the survey.[5] Here he measured with two wooden rules, each 4 toises long, a line which, according to the first measurement, was 5,662 toises 5 feet long, the second, 5,663 toises 1 foot. He adopted the mean of 5,663 toises. His second base had a length of 3,902 toises.[6] His calculations gave as a result 57,057 toises for the length of a degree; but he adopted the more convenient number of 57,060.[7] Later he remarks that on the ocean the length would be 8 feet less, but thinks this unworthy of consideration.[8] It may be well to add that though he was acquainted with the fact of refraction, and discovered the influence of temperature thereon, he takes no notice of it in his calculations, nor did he make allowance for the precession of the equinoxes or the aberration of light, and was still ignorant of the flattening of the earth at the poles; consequently calculated on the basis of the absolute sphericity of the earth. Later Maupertuis made a calculation of the length of a degree, based on Picard's geodetic work, but taking into account the precession of the equinoxes and the aberration of light, and found it to be 57,183 toises;[9] or according to another calculation, including the effect of refraction also, he finds the value of a degree to be 56,925 toises.[10] As Picard was the first to apply the telescope to the quadrant, he was enabled to make more accurate observations than his predecessors; but it is not to be wondered at that he committed an error of a few seconds. He acknowledged himself that he could not be responsible for errors of 2 seconds, notwithstanding his exactitude, which error would make a difference of 32 toises on each observation.[1] This error however was happily compensated by his toise being about one-thousandth shorter than the standard.[2] Recent authorities find the length of a degree at this latitude to be 57,011.825 toises,[3] so that Picard's error amounted to 45.175 toises, or 0.79 per cent.

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[1] Cassini, Grandeur, etc., 365–8.

[2] Jordan, Vermessungskunde, II, 4.

[3] Picard, Mesure de la terre, 7.

[4] Cassini, Grandeur, etc., 331.

[5] Picard Mesure de la terre, 3.

[6] *Ibid.*, 3.

[7] *Ibid.*, 22.

[8] *Ibid.*, 23.

[9] Maupertuis, Œuvres, IV, 330.

[10] *Ibid.* III, 167.

To settle a point of such importance Louis XIV ordered (1700) a survey of a meridian line stretching throughout the entire length of France. As early as 1680 a survey had been commenced with the idea of giving it this extent, but had been discontinued without coming to any result. Now the line was divided into two parts, the one extending from Paris north to Dunkerque, the other from Paris south to Collioure. The survey of the first was intrusted to La Hire, and was found to have an amplitude of 2° 12′ 9″ 30‴[4] and a length of 125,454 toises, which, reduced to sea-level and calculating on the basis of the sphericity of the earth, gives to the degree 56,960 toises. Jacques Cassini conducted the survey of the second and much longer line, found its amplitude to be 6° 18′ 57″ and its length 360,614 toises, which gives for the degree 57,097 toises.[5] Combining the two surveys, one has a line with an amplitude of 8° 31′ 11″$\frac{5}{8}$, with a length of 486,156 toises; this gives as the mean length of a degree 57 ,061 toises, which approaches so nearly to that determined by M. Picard that it was thought it should conform to it.[6] A base of 7,246 toises 2 feet was measured[7] for the southern division, which varied 3 toises from "the calculated length continued from Paris," in consequence of which various corrections in the observed angles were made, by which the result was brought very near (à très peu près) to that measured on the ground.[8] For the northern division, Picard's old base-line from Villejuive to Juvisy was adopted without re-measuring, and a second base-line near Dunkerque was measured for purposes of verification. This had a length of 5,464 toises 3 feet,[9] which was within almost 1 toise of the length found by calculating the line from Picard's base. All the angles of each triangle of the entire survey were actually observed,[10] and, for purposes of comparison, nine triangles of Picard were also inclosed in the net.[1] Great was the exultation of the French, for the results of this unprecedentedly great undertaking showed not only that the earth was not flattened at the poles, as Newton and Huyghens taught, but that it was actually elongated in that direction. Doubts were naturally expressed by the Newtonians as to the accuracy of the operations by which the result had been obtained. To satisfy these there were repetitions with different instruments and different methods, the entire work continuing at intervals from 1701 to 1736, and always with the same result, viz, that the more southerly part gave the greatest length of a degree.[2] Still the party of theory was not to be quieted, objecting that the portions of the meridian were too near together to afford incontrovertible proof of the earth's form, and maintaining that the true solution could only be furnished by surveying one meridian line near the equator and another near the poles and comparing the results. This opinion gradually gained force, till finally the French government undertook to provide the means for carrying out this project, and the French Academy of Sciences to furnish the specialists to conduct with commanding ability the operations.

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[1] Cassini, Grandeur de la terre, 183.

[2] Mechain, et Delambre, Base du système, etc., I, 7.

[3] Bessel's calculations in H. Struve, Landkarten, p. 61.

[4] Cassini, Grandeur de la terre, 292.

[5] *Ibid.*, 178–181. Quoted all together in Maupertuis, Œuvres, IV, 327, with 9″ 30″ false.

[6] *Ibid.*, 302.

[7] *Ibid.*, 123.

[8] *Ibid.*, 125, 126.

[9] *Ibid.*, 270, p. 237. He gives 5,564 toises.

[10] *Ibid.*, 331.

For this purpose two expeditions were fitted out with all the care possible at that date, to secure accuracy in the result of their respective surveys. The first set out for Peru in 1735, but did not return till 1744. In the mean time a second expedition had been sent to Sweden, where it finished its work and returned to France in 1737; the result of the latter survey as compared with those in France showed beyond a doubt the flattening of the earth at the poles, without awaiting returns from the equator. The instigator and chief of this enterprise was the famous mathematician, Maupertuis, whose quarrel with Voltaire at the court of Frederick the Great is probably much better known than his perils and hardships[3] in surveying a meridian line which crosses the polar circle. His line was a short one, extending from Tornea to the mountain Kittis, with an amplitude of only 57′ 28¾,″ the mean result of the observations of the same two stars at both end stations. The mountainous nature of the region favored large triangles, generally of a form advantageous to the ease and accuracy of observation. The frozen surface of the river Tornea offered an excellent opportunity to measure a base line; so good in fact that Maupertuis neglected to take any account of its fall. This action has been censured by later mathematicians, who have consequently corrected his result by 5.355 toises.[1] His base line was 7,406.86 toises long, the two measurements differing only 4 inches[2] from each other. Calculating from this basis, he found his meridian line to be 55,023½ toises long, which, compared with the amplitude,[3] gives the length of a degree of latitude crossing the Polar circle 57,438 toises.[4] Comparing this result with Picard's measurement and leaving out of the reckoning the aberration of light, which was unknown to the latter, Maupertuis's arc would have an amplitude of 57′ 25,″ and this, compared with the length, would raise the value of a degree at the polar circle to 57,497 toises or 437 toises greater than Picard's result in France; and, taking the aberration into account, Maupertuis's result differs 950 toises from that which it ought to be, following Cassini's calculations based on the supposition of the earth being elongated at the poles.[5]

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[1] Cassini, Grandeur de la terre, 237.

[2] Maupertuis, Œuvres, III, 37.

[3] He gives (Œuvres, III, 146) a graphic description of some of his trials. He says: "Je ne dirai rien des fatigues ni des périls de cette opération (mesurer la base); on imaginera ce que c'est que de marcher dans une neige haute de 2 pieds, chargés de perches pesantes, qu'il falloit continuellement poser sur la neige et relever; pendant un froid si grand que la langue et les lèvres se géloient sur-le-champ contre la tasse, lorsqu'on vouloit boire de l'eau-de-vie, qui étoit la seule liqueur qu'on pût tenir assez liquide pour la boire, et ne s'en arrachoient que sanglantes; pendant un froid qui gêla les doigts de quelques-uns de nous, et qui nous menaçoit à tous momens d'accidens plus grands encore. Tandis que les extrémités de nos corps étoient glacées, le travail nous faisoit suer. L'eau-de-vie ne put suffire à nous désaltérer; il fallut creuser dans la glace des puits profonds, qui étoient presque aussitôt refermés, et d'où l'eau pouvait à peine parvenir liquide à la bouche; et il falloit s'exposer au dangereux contraire que pouvoit produire dans nos corps échauffés cette eau glacée."

The expedition to Peru met with a series of difficulties, which, combined with party strife and the length of the line surveyed, detained the experts nine years; so that the principal question, which caused their going, viz, as to the form of the earth, was already settled forever before their return, and the results achieved only served to add another factor toward arriving at exactness in the solution of the matter. A couple of Spanish representatives also took part in the operations. The company was provided with several quadrants, on one of which was the first micrometer ever so applied.[6] Their astronomical observations were made with a sector of 12-foot radius, and their base line was measured with wooden rules 20 feet long. On the ends of these were fastened projecting copper plates, so arranged that, in measuring, those of neighboring rules stood at right angles to each other. In measuring, three rules were laid on the ground in a straight line, level being secured by means of wedges. The line was maintained by means of a stretched cord, and the surveyors had to lie on the ground "*pour les disposer*".[7] They knew that these rules were subject to variations according to changes in the humidity and temperature of the atmosphere, and found themselves "obliged to examine each day and often several times the little equation or correction that was necessary to apply to them."[8] For this purpose they had a bar of iron whereon was marked a toise. This was kept in the shade in the guard tent, but no regard seems to have been paid to the fact that the iron also was subject to constant changes in length. In computing the final result an allowance was made for its expansion by heat, probably for the average temperature of Peru above that of Paris. This toise was afterward adopted as the standard of measure under the name of toise of Peru, and with it all subsequent surveys have been compared. All the angles of each triangle were actually observed, many of them twice, with different instruments and by different observers, and some of them even three times.[1] Experiments were made to determine as nearly as possible the constant failure of the quadrants, which of course was taken into account in the observations, besides which other corrections were made to reduce the sum of all three angles to 180°, the amount of this last correction seldom reaching 30 seconds.[2] The first base line was measured by two different parties starting at the opposite ends, and had an extent of 6,272 toises. The second line, for verification, was 5,259 toises long, which according to Bouguer was within 3 to 4 feet, according to La Condamine, within one toise, of the trigonometrically calculated length. The entire line had an amplitude of 3° 7′ 3.″3,[3] crossed the equator, and measured 176,940 toises, according to Bouguer,[4] or 176,930, according to Condamine.[5] The operations were carried on at an altitude of more than 1,000 feet above sea level, to which all measurements must be reduced. According to actual measurement the first degree from the equator is 56,767 toises long, from which 21⅝ toises were subtracted to reduce to sea level, and 6 to 7 toises added for expansion of the standard in the heat, giving for the true value of a degree of latitude at the equator 56,753 toises.[6] The entire operation was subjected to a searching criticism at the beginning of the present century, in the light of more recent researches, with the following result: "Wenn wir daher nach sorgfältiger Erwägung aller vorher erörterten Umstände der Ungewissheit in der Grösse eines Breiten-Grades am Æquator noch auf 80–100 Toisen festsetzen, so glauben wir keine Ungerechtigkeit gegen die französischen und spanischen Messkünstler zu begehen, deren Arbeiten keineswegs aus Mangel an Geschicklichkeit, sondern enizig wegen Unvolkommenheit der damaligen Instrumente nicht den Grad von Genauigkeit haben konnten, der zu einer Gradmessung erfordert wird."[7]

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[1] Zach, Mon. Cor., Januar, 1806, p. 20.

[2] Maupertuis, Œuvres, IV, 301.

[3] *Ibid.*, III, 152, adopts amplitude of 57′ 27,″ that given by observation of only one star.

[4] *Ibid.*, IV, 331.

[5] *Ibid.*, III, 167, 168.

[6] Bouguer, Figure de la terre, 60, 61.

[7] *Ibid.*, p. 40.

[8] *Ibid.*, 40, 41.

The next great survey was also the work of French savants, but was undertaken for a different purpose. It was now admitted on all sides that the earth is flattened at the poles; but there was in France at least a strong desire for a more correct determination of the size of the earth than had yet been made, in order to deduct therefrom a standard of measurement founded in nature, so that if ever lost it could be recovered; and further that there might be a standard which was in its character not national, but universal. This was one of those plans for universal improvement so rife at the beginning of the French Revolution, and the one perhaps of all which has been most permanent and wide-spread in its results. The work of carrying out the project was intrusted to Delambre and Mechain, two prominent scientists of the day. They surveyed a meridian line extending from Dunkerque to Barcelona, with an amplitude of 9° 40′ 24″.75, and a length of 551,584.72 toises. The line was later extended to Formentera in the Balearic Islands, but too late to change the result, which had been accepted for the standard of measurement. For this survey the most careful preparations were made to secure the utmost accuracy. Instead of the old-fashioned quadrants and sectors, with their unwieldly bulk and subject to a variety of changes from temperature, position, their own weight, etc., the then newly invented repetition circles of Borda were used with excellent result. The same expert also provided rules for measuring the base line, which were of a pattern entirely new and capable of an accuracy hitherto impossible. The measuring part was formed of platinum, whose relations to the toises of Peru and Lapland were accurately determined. Upon this a rule of copper 16 inches shorter was fastened securely at one end. The ratio of expansion of the two metals and the difference of length at a fixed temperature being known, an observation of the temperature and of the difference of their lengths by means of a vernier provided for that purpose, which was fixed to the metal and protected from the sun's rays, gives the amount of expansion at the moment of observation. Here was also adopted for the first time a plan which afterwards became universal in such operations, namely in measuring the base, to place the rules at a distance from each other to prevent the effect of the shock of contact, and measure carefully the interval. For this purpose there was attached at one end of each rule a small slide, accurately divided into hundred-thousandths for measuring minute distances, and provided with a microscope for reading them. After both rules were placed in line this was moved forward with the greatest care till it covered the interval between the rules, and the distance was at once read off and noted.

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[1] Bouguer, Figure de la terre, 100, 101.

[2] *Ibid.*, 104.

[3] According to Zach's calculations, 3° 6′ 0.″9. Mon. Cor., Oct., 1807, p. 320.

[4] *Ibid.*, 153.

[5] Posch, 47.

[6] Bouguer, 272.

[7] Zach, Mon. Cor., Oct., 1807, p. 325.

In reference to the actual labor in determining a meridian line, Delambre remarks:[1] "De toutes les opérations qui concourent à la mesure des degrés du méridien, les observations de latitude sont celles qui demandent plus de précautions, plus de soins et plus de temps." As an example of the extreme care taken in this work, may be cited the fact that to determine the latitude of the Panthéon at Paris, Mechain and Delambre each made eighteen hundred astronomical observations, the results agreeing to the sixth of a second.[1] This was to establish the exact position of one point in the middle of the surveyed line; and similar observations were necessary at both ends of the same. Two base lines were measured, 6,075,90 and 6,006,25 toises long, respectively. In fixing the permanent ends thereof and in the work of placing the rules in the true direction, etc., this survey furnished the model for the future; and though more recent experts have changed details of practice, they have offered nothing new in principle. In reducing the length actually measured to the true basis of the survey, certain slight alterations were necessary, which were made with the greatest nicety. For instance, the line of Melun was not perfectly straight, but was broken at one point by an angle of 179° 10′ 49″.09, which necessitated calculations to give the length of the corresponding straight line; even the thickness of the cord bearing the plummet was subtracted, and corrections for temperature, for inevitable errors in tracing the line, and for the thickness of the rules, were added.[2] When all this was done it remained to reduce this to the arc of a circle and then to sea-level. Delambre says that the greatest error to be feared is that of the "vernier des languettes," or slides for measuring the intervals between the rules, which error will not surpass one inch in 6,000 toises, $\frac{1}{432000}$ of the whole length.[3] Work of such nicety is necessarily slow, and it need not surprise one to learn that it took forty-one days of actual work from 9 o'clock A. M. to sun-down to measure one line, and that the greatest attainable speed was to place in a day ninety rules end to end, or in other words—measure 360 meters.[4]

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[1] Base du système, II, 158.

Equal care was taken in locating the triangles, as witness a search of six days for a place from which at one time three important points might be seen or the measuring an angle 170 times (!), because of the peculiar effect of the sun's light at different times of the day on a *belvédère*, which formed the point of a neighboring angle.[5] With Borda's circle was observed not only the angle between the two lines of the triangle, but also the zenith distance of each point. The size of the angle was determined by the use of a series of twenty angles in favorable cases, and of repeating doubtful cases at different hours of the day.[6] The result of the survey was to give to the forty-fifth degree of latitude the value of 67,027 toises.

Thus the eighteenth century proved conclusively what the genius of the seventeenth had only made probable on theoretical grounds, namely, the flattening of the earth at the poles. It remained for the nineteenth not only to determine exactly the quantity thereof, but to bring to light another fact not dreamed of heretofore, *i. e.*, that the earth, even in its geodetic lines, has no regular geometrical figure whatever. This was the finishing touch to the dream of the Pythagorean school. First, it was proven that the earth is not only not the center of the universe, but that it is merely as a grain of sand in the illimitable ocean of space. Then its sphericity gave way before the genius of Newton and the work of French enthusiasts. Finally the gradual increase in accuracy in all branches of science deprived us of the last poetic idea as to its form, struck the death knell of the Pythagorean theory, and left us with the bald fact that our beautiful earth cannot lay claim to any ideal form.

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[1] Base du système I, 94.

[2] *Ibid.*, II, 41–45.

[3] *Ibid.*, III, 165.

[4] *Ibid.*, 85, 86.

[5] *Ibid.*, I, 75.

[6] *Ibid.*, I, 117.

INDEX.

A.

Page.

Abnaki Indians, pictographs of, study of ... 56
Aboriginal pottery collections of National Museum, statistics of accessions ... 40, 41
  presented by Dr. Featherstonehaugh ... 42
Academic publications received by library ... 25, 84
Academy of Sciences at Berlin, account of ... 89
Accessions to collections of Bureau of Ethnology ... 63
  National Museum, list of ... 71
  library of National Museum ... 47
  National Museum collections, statistics of ... 40, 41
  Smithsonian library ... 23, 25, 83
Act of Congress establishing Zoological Park ... xxxiv
  incorporating American Historical Association ... xxxv
  making appropriations ... xxi, xxii, xxiv, xxvii, xxviii, xxxiii, xxxiv, xxxv
Adams, Herbert B., lecture, The State and Higher Education ... 695
Addresses:
  Adams, Herbert B., The State and Higher Education ... 695
  Burdon-Sanderson, J. S., Elementary Problems in Physiology ... 623
  Lovering, Joseph, Michelson's Recent Researches on Light ... 449
  Roscoe, Henry E., The Life Work of a Chemist ... 491
  Thiselton-Dyer, H. T., Botanical Biology ... 399
  Turner, Sir William, On Heredity ... 541
  Virchow, Dr. Rudolph, Anthropology in the Last Twenty Years ... 555
Aerial locomotion, paper on, by F. H. Wenham ... 303
Africa, explorations in, by W. Selcott Williams ... 8, 9
Agassiz, Louis, and Spencer F. Baird, Natural History Illustrations, prepared under the direction of ... 69
Agents, foreign, salaries of ... 74
  general, of Colonial governments, act as exchange agents ... 80, 81
Agricultural Department, co-operation of ... 45
  High School at Berlin, account of ... 121
Agronomic-pedological Institute of the Agricultural High School of Berlin, Germany, account of ... 124
Alabama, ethnological collections from ... 42
Alaska, Copper River, natives of, paper on, by Henry T. Allen ... 70
  explorations in, by W. L. Howard ... 51
Albatross collections transferred to National Museum ... 42
Alfaro, Anastasio, gold ornaments received from ... 63
  loaned specimens to Bureau of Ethnology ... 63
Algae collections presented by F. S. Collins ... 44

Page.

- Algonquin bibliography by J. C. Pilling, 62
- Allen, Dr. Harrison, delivered lecture on clinical study of the skull, 50, 52
- Aluminum, alloys of, paper on, by J. H. Dagger, 725
    - paper on, by H. C. Hovey, 721
- American Geological Society, Council of, met at National Museum, 50
    - Historical Association, incorporation of, xxxv
    - Institute of Mining Engineers' collection added to Museum, 5
    - Ornithologists' Union met at National Museum, 50
- Anatomy of Astrangia Danæ, 69
- Anderson, William, paper on molecular structure of matter, 711
- Angell, James B., acts of, as regent, xii, xiii
- Angel, G. W. J., presented series of dried coleoptera, 43
- Animal physiology, Institute of, of the Agricultural High School at Berlin, Germany, account of, 125
- Anthropology, bibliography of, 621
    - in the last twenty years, address by Dr. Rudolph Virchow, 555
    - prehistoric, collection of National Museum, statistics of accessions, 40, 41
    - progress in, by Otis T. Mason, 591
- Antiquities, spurious Mexican, paper on, by William H. Holmes, 70
- Apparatus collection of National Museum, 40, 41
    - the late William Shaw loaned to institution, 7
    - shed erected in Smithsonian Grounds, 7
- Appropriation by Congress for widow of Professor Baird, 32
    - for American Historical Association, xxxv
    - insufficient, effect of, 20, 79
    - International exchanges, xxi, xxxi, xxxiii, 3, 4, 17, 18, 19, 20, 21, 74, 75, 76
    - National Museum, xxiv, xxvii, xxviii, xxix, xxxiv, 3, 4
        - heating and lighting, xxviii, xxx, xxxii, xxxiv, 3, 4
        - Fish Commission, for repair of armory, xxxiv
        - furniture and fixtures, xxvii, xxix, xxxiii, xxxiv, 3, 4
        - living animals, 3, 4
        - postage, xxxiv, 3, 4
        - preservation of collections, xxiv, xxvii, xxix, xxxi, xxxiv, 3, 4
        - printing and binding, xxxiv, 3, 4
    - North American Ethnology, xxii, xxxi, xxxiii, 3, 4
    - Ohio Valley Centennial Exposition, 52
    - required for purchase of exhibition material, 54
    - Smithsonian building, xxiv, xxxi
    - statue to Professor Baird, 32
    - Zoological Park, xxxiv, 31
- Aquarium at Berlin, Germany, account of, 121
- Archæological collections, principal accessions to, 42
- Archæology, Scandinavian, paper on by M. Ingwald Unsot, 571
- Argentine Republic, exchanges with, 77, 78
- Arietidæ, Genesis of, memoir on, by Prof. Alpheus Hyatt, 69
- Arlington National Cemetery, a proposed site of astro-physical observatory, 33
- Armory building assigned by Congress to Fish Commission, 5, 6, 7
- Art collections presented to Smithsonian Institution, 32
    - work of Ojibwas, studied by Dr. W. J. Hoffman, 57
- Assistant Secretary placed in charge of exhibit at Marietta Exposition, 53
- Assyrian objects, casts of, presented by Prof. Paul Haupt, 42
- Astrangia Danæ, anatomy of, 69
- Astronomical Journal, subscription for twenty copies, 36

Page.
Astronomical obervatories, report on, for 1886, by George H. Boehmer 70
Astro-physical Observatory at Potsdam, Germany, account of 133
  proposed erection of 33
  observations, temporary shed for 33
Australian Museum at Sydney, contributed collection of birds 43

B.

Bailey, H. B. & Co., grant free freight 21, 79
Baird, Spencer F., annual report of, for 1886 70
  and Louis Agassiz, natural history illustrations prepared under the direction of 69
  statue to 32
  widow of, appropriation for relief of 32
Baldwin Locomotive Works, present model of locomotives 44
Base line established by United States Geological Survey in Smithsonian Grounds 33
Batrachia of North America, Professor Cope's work on 10
Beck, Senator, bill for establishment of Zoological Park 27
Bequest of James Hamilton, amount of xix, 2, 3
  Simeon Habel, amount of xix, 2
  Smithson, amount of xix, 2
Berlin, Germany, scientific institutions at, account of 89
Bern, University of, sends dissertations 25, 84
Bibliographical work of J. C. Pilling 62, 65
Bibliography of anthropology, 1889 621
  astronomy for 1887, by William C. Winlock 70
  chemistry for 1887, by H. Carrington Bolton 70
  meteorology for 1889, by O. L. Fassig 271
  National Museum 71
  North American Indian languages, by J. C. Pilling 65
Bisons, American, described by Dr. V. F. McGillycuddy 25
Blanford, H. F., lecture, "How rain is formed" 287
Bleismer, Rev. C. A., translation of Virchow's address, "Anthropology in the last twenty years" 555
Blytt, A., paper on the movements of the earth's crust 325
Board of Regents. (See Regents.)
Boas, Dr. Franz, explorations by 60
Boehmer, George H.:
  Additions and corrections to list of foreign correspondents by 70
  Report on astronomical observatories for 1886 by 70, 71
  Report on Smithsonian exchanges for 1887 by 70, 73
  Systematic arrangement of list of foreign correspondents by 70
  Translation by 89
Bonn, University of, sends dissertations 25, 84
Books deposited in Library of Congress 22, 25, 83
  inaccessibility of, in Library of Congress 22, 23
  retained for Museum library 25
  transferred to Surgeon-General's library 25, 83
Boscovich's theory, by Dr. William Thompson 435
Botanical biology, address by W. T. Thiselton-Dyer 399
  Garden in Berlin, Germany, account of 113
  Institute of the Royal University at Berlin, Germany, account of 117

Page.
Botanical Museum of the Royal University at Berlin, Germany, account of ... 114
Boulton, Bliss & Dallett, grant free freight ... 21, 80
Breckinridge, Hon. W. C. P., introduced bill relative to establishment of Zoological Park ... 27, 29
remarks relative to Zoological Park ... 31
British Colonies, the crown agents in London the exchange agents for ... 80
Brown, Vernon H., & Co., grant free freight ... 21, 80
Brussels exchange treaty of 1886 ... 18, 20, 76
Building, additional, required for Museum ... 5, 6, 7
expenditures for ... xx
material, testing station for, at Berlin, Germany, account of ... 133
Building-stone collection of National Museum, report on, by George P. Merrill ... 71
Bullay, H. J., grants free freight ... 21, 80
Bulletins of Bureau of Ethnology ... 65
National Museum, account of ... 13
history of publications of ... 47, 48, 49, 71
Burdon-Sanderson, J. S., address "Elementary problems in physiology" ... 423
Bureau of Engraving and Printing at Berlin, Germany, account of ... 143
Bureaus of the Government, co-operation of ... 45
receive appropriation for exchanges ... xxxiii, 17, 18, 75

C.

Calhoun County, Illinois, mound explorations in ... 55
California, ethnological collections from ... 42, 63
perforated stones from, paper on, by H. W. Henshaw ... 65
Cameron, R. W., & Co., grant free freight ... 21, 80
Carbon process prints, presented by J. H. Osborne ... 44
Casts presented by Prof. Paul Haupt ... 42
Catalogue entries made in the Museum ... 42
of exhibit for Ohio Valley Centennial Exposition ... 47
minerals and their synonyms, by T. Egleston ... 47, 71
publications of the Smithsonian Institution, by William J. Rhees ... 70, 71
Cazaux, H., grants free freight ... 21
Centennial Exposition of the Ohio Valley ... 47, 51
Central America, correspondents in ... 76
exchanges with ... 77, 78
Central Telegraph Bureau at Berlin, Germany, account of ... 139
Chandler, Prof. Charles F., presented collection of photo-mechanical process work ... 44
Chemical Institute of the Agricultural High School at Berlin, Germany, account of ... 123
institutes of the Royal University at Berlin, Germany, account of ... 112
laboratories connected with Technical High School at Berlin, Germany, account of ... 131, 132
Chemist, the life work of a, address by Dr. Henry E. Roscoe ... 491
Chili, exchanges with ... 77, 78
China, ethnological collections from ... 42
exchange agency in ... 77, 78, 80
explorations in, by W. W. Rockhill ... 9
Cincinnati Exposition ... 45
Classified service of the Museum ... 34, 35, 36, 37, 38
Clay, Col. Cecil, presented full-grown moose ... 43

Page.

Clerical force of library 24

staff of National Museum, salary schedule for 35, 36, 37

Clinical study of the skull, lecture on by Dr. H. Allen 32

Coast and Geodetic Survey continues pendulum experiments in Smithsonian building 32

Superintendent of, co-operation of 45

Cody, Hon. W. F., presented American elks 25, 43

Coins, collection of, presented by Hon. W. T. Rice 44

Coleoptera presented by G. W. J. Angell 43

Collections, accessions to 40, 44

preservation of, Congressional appropriation for xxiv, xxvii, xxix, xxxi, xxxiv, 3, 4

estimate of cost submitted to Congress 4

Colombia, exchanges with 77, 78

Colorado, ethnological collections made in 63

Columbium, index to literature of, by Frank H. Traphagen 69

Commander Islands, bones of *Pallas cormorant* collected at 43

Commensals in the pearl oysters, paper on, by R. E. C. Stearns 70

Compagnie Générale Transatlantique, grant free freight 21, 80

Compensation of employés 74

Computation Institute of the Royal Observatory at Berlin, Germany, account of 109

Congress of orientalists, aid given to 15

Congressional appropriations. (See Appropriations and Acts of Congress.)

Contributions to Knowledge, account of 11, 69

published during the year 69

North American ethnology, published by Bureau of Ethnology 65

Convention of Brussels, of 1886 18, 20, 76

relative to international exchanges 76

Co-operation of Departments and Bureaus of the Government 45

Cope, Prof. E. D., work on reptilia and batrachia of North America 10

Copenhagen, Denmark, exchange agency in 77, 80

Coppée, Dr. Henry, acts of, as regent xii, xiii

eulogistic remarks on Dr. Asa Gray xiii

member of committee on eulogy of Dr. Asa Gray xiii

member of executive committee xii

Correspondence, character of, and attention to 34

Correspondents of exchange service 75

Costa Rica, exchange agency in 43, 80

Courtesies extended to museums, etc 50

Cox, Hon. Samuel S., acts of, as regent xii, xiii

death of 1

motion, electing Chancellor of Institution xv

offered resolution to appropriate income xvi

remarks on Zoological Park xvi

Cox, W. V., appointed to take charge of exhibit at Marietta Exposition 53

Crawford, Dr. John M., explorations by 9

Crown agents for the colonies, London, act as exchange agents 80

Cuba, exchange agency in 80

Cullom, Hon. Shelby M., acts of, as regent xii, xiv

offered resolution relative to deposit of books by American Historical Association xvii

remarks on Zoological Park xxx

term as regent expired 1

Page.

- Cunard Royal Mail Steamship Line, grants free freight 21, 80
- Curators of exchanges 16
- Curtis, George E., report on progress in meteorology 205

D.

- Dagger, J. H., paper on alloys of aluminum 725
- Dallas, W. S., translation of paper on the movements of the earth's crust 325
- Dead Letter Office, co-operation of 45
- Deficiency, estimated, for exchange service 20
- Denmark, exchanges with 77, 78, 80
- Denison, Thomas, grants free freight 21, 80
- Department of Agriculture, co-operation of 45
  - living animals 25, 26, 27
  - State, co-operation of 45
- Departmental exchanges explained 17, 18, 19
- Departments of Government, co-operation of 45
  - receiving appropriation for exchanges 17, 18, 75
- Depositories of parliamentary exchanges 77
- Dibble, Hon. S., letter to, relative to establishment of Zoological Park 27, 28, 29
  - remarks relative to Zoological Park 31
- Disbursing clerk of Institution appointed 5
- Disbursements of public moneys 3
- Distribution, geographical, of correspondents 76
  - of duplicate specimens 46
  - of Museum publications, plan recommended 47
  - of publications, plan for 13, 47
- District of Columbia bill, amendment by Senator Edmunds 29, 30, 31
  - ethnological collections made in 63
- Documents, official, international exchange of 76
- Domestic correspondents of Smithsonian Institution 75, 76
  - exchange packages sent 73, 74
- Dorpat, University of, sends dissertations 25, 84
- Dresden, Saxony, depository of United States official publications in 77
- Drugs, collection of, obtained from Kew Gardens 44
  - presented by Dr. J. W. Jewett 44
- Dunedin, New Zealand, Otago Museum at, sent collection of New Zealand fishes 43
- Duplicate specimens, distribution of 46
- Dutch Guiana, exchange agency in 80

E.

- Earll, R. E., placed in charge of exhibit at Ohio Valley Centennial Exposition 53
- Earth's crust, movements of the. Paper on, by A. Blytt 325
- Earthworks at Fort Ancient, Ohio. Paper on, by William M. Thompson 70
  - Paper on, by C. Thomas 65
- Eastman Dry Plate Company presented kodak camera 45, 46
- Ecuador, exchanges with 77, 78, 80
- Eddy, William A., paper on the Eiffel Tower 736
- Edmunds, Senator, amendment to bill relative to Zoological Park 29, 30, 31
- Eells, Myron, paper on the stone age of Oregon 70, 71
- Egleston, T., catalogue of minerals and their synonyms 47, 71

Page.
Egypt, ethnological collections from ... 42
exchange agency in ... 80
Egyptian objects, casts of, presented by Prof. Paul Haupt ... 42
Eiffel, G., paper on the Eiffel Tower ... 729
Eiffel Tower. Paper on, by William A. Eddy ... 736
Paper on, by G. Eiffel ... 729
Electrical oscillations, Hertz's researches on. Paper on, by G. W. de Tunzelmann. 145
Elementary problems in physiology. Address by Prof. J. S. Burton Sanderson. 423
Elks, American, presented by Hon. W. F. Cody ... 25, 43
Endicott, William C., letter to Secretary of Institution relative to site for astrophysical observatory ... 34
Erlangen, University of, sends dissertations ... 25, 84
Estimates presented to Congress for cost of—
International exchanges ... 4, 18, 75
North American ethnology ... 4
National Museum ... 4
Ethnography, Museum of, at Berlin, Germany, account of ... 136
Ethnological collections, principal accessions to ... 42
Ethnology, Bureau of, publications of ... 71
North American, Congressional appropriation for ... xxii, xxxi, xxxiii, 3, 4
contributions to, published by Bureau of Ethnology 65
estimate of cost of submitted to Congress ... 4
Eulogy of Dr. Asa Gray, resolutions of Board of Regents ... xiii, xv
Evarts, Secretary, letter to Secretary of Smithsonian Institution ... 17, 76
Exchanges of Institution, expenditures for ... xx
Exchanges, system of—
a means of diffusing knowledge ... 15
appointment of John Quackenbush as agent ... 16
appointment of W. C. Winlock as curator ... 16
appropriations and expenditures ... xxi, xxxi, xxxiii, 3, 4, 17, 18, 19, 20, 21, 74, 75, 76
assumed by Government ... 15, 16, 17
companies granting free freight ... 21, 79, 80
Congressional appropriations for ... 17, 18, 19, 74, 75, 76
convention at Brussels ... 20, 76
death of Dr. J. H. Kidder, curator ... 16, 66, 73
cost of service ... 18, 74, 75, 76
correspondents of ... 16, 75, 76
estimates presented to Congress ... 18, 75, 76
expenses of ... 16
foreign agencies ... 80, 81
geographical distribution of correspondents ... 77
of official documents ... 20, 76
packages sent abroad ... 19, 78
parliamentary exchanges ... 77
pay of employés ... 20
proposed new plan ... 19
report of curator ... 73
report by Dr. J. H. Kidder ... 70
report of Secretary on ... 15
rules for transmission ... 81
shipments to foreign countries ... 77
statistics of ... 73, 74, 75, 76, 77, 78, 79
transactions of office ... 73

Page.
Executive committee, members of ... xii
statement of expenditures for International Exchanges ... xxi, xxxi
statement of expenditures for North American Ethnology ... xxii, xxxi
statement of expenditures for Smithsonian Building repair ... xxiv, xxxi
statement of expenditures for National Museum ... xxiv, xxxi
to advise Secretary as to expenditure of income ... xix
Exhibit at Marietta Exposition ... 53
at Ohio Valley Centennial Exposition ... 53
for Ohio Valley Centennial Exposition, catalogue of ... 47
Expenditures of Smithsonian Institution ... xx
of exchanges, American, etc. (see Executive committee.)
Explorations by Dr. James Grant Bey ... 9
Boas, Dr. Franz ... 60
Crawford. Dr. John M ... 9
Curtin, Jeremiah ... 9, 59
Emmert, John W ... 55
Fowke, Gerard ... 55
Gatschet, A. S ... 59
Henshaw, H. W ... 57
Hewitt, J. N. B ... 59
Hillebrand, W. F ... 51
Hoffman, Dr. W. J ... 56, 57
Howard, W. L ... 10, 51
Jenkins, O. P. ... 10, 51
Mallery, Col. Garrick ... 56
Merrill, George P ... 51
Middleton, James D ... 57
Mindeleff, Victor ... 60
Mooney, James ... 58
Moser, Lieut. J. F ... 10
Post, Frederick H ... 10
Reynolds, Henry L ... 55, 56
Rockhill, W. W ... 9
Stephen, A. M ... 60
Thomas, Cyrus ... 55
Williams, A. Talcott ... 8, 9
Wilson, Thomas ... 51
Expositions ... 54
unsafe condition of buildings ... 54
appropriations required for new material ... 54
loss to collections on account of ... 54

F.

Fernow, Dr. B. E., appointed honorary curator ... 45, 51
Fewkes, J. Walter, explanation of plates by ... 69
Fire-proofing of Smithsonian building, continuation of ... 7
Fish Commission added to collection of National Museum ... 42
Congressional appropriations to repair Armory building ... xxxiv
occupies Armory building ... 5, 6, 7
Flora of North America, copies of, presented to Mrs. Gray ... xii, xv

Page.
Florida, archæological specimens from ... 42, 43, 63
Flügel, Dr. Felix, acknowldgment of services rendered ... 79
exchange agent of the Institution ... 80, 81
Foreign correspondents of Smithsonian Institution ... 75, 76
Foreign exchange agents, salaries of ... 74
Governments, official publications sent to ... 77
returns made by ... 78
offices acting as exchange agents ... 80, 81
Forget, A., grants free freight ... 21, 80
Fort Ancient, Ohio, earth works at, paper on, by William M. Thompson ... 70
France, exchanges with ... 77, 78
Free freight granted by transportation companies ... 21, 79
Freiburg, University of, sends dissertations ... 25, 84
Freight, cost of ... 20, 74, 75
free, granted by transportation companies ... 21, 79
Fuller, Chief Justice Melville W., elected Chancellor of Institution ... xv, 1
president of Board of Regents ... xii, xiii
Funch, Edye & Co., grant free freight ... 21, 80
Funds administered by Institution ... xix, xxi, xxii, xxiv, xxxi
Furniture and fixtures, Congressional appropriation for .xxvii, xxix, xxxiii, xxxiv, 3, 4
estimate of cost submitted to Congress ... 4
of Institution, expenditures for ... xx
of National Museum, salary schedule for ... 35, 37, 38

G.

Garden, Botanical, at Berlin, Germany, account of ... 113
of University at Berlin, Germany, account of ... 116
Zoological, at Berlin, Germany, account of ... 120
Gardening, Royal School of, at Berlin, Germany, account of ... 127
Gem collection of National Museum, report on, by George F. Kuntz ... 71
Genealogy of man, the last steps in. Lecture by Dr. Paul Topinard ... 609
Genesis of the Arietidæ, memoir by Prof. Alpheus Hyatt ... 69
Geodetic Institute, Royal, at Berlin, Germany, account of ... 135
Instruction, Institute of, of the Agricultural High School at Berlin, account of ... 126
Geographical distribution of correspondents ... 76
latitude, paper on, by Walter B. Scaipe ... 749
Geological Congress, International, American Committee of, met at National Museum ... 50
Institute at Berlin, Germany, account of ... 128
Survey added to collections of National Museum ... 42
established base line in Smithsonian grounds ... 33
Geology collection, principal accessions to ... 44
Georgetown, British Guiana, exchange agency in ... 80
Germany, exchanges with ... 77, 78
Gibson, Hon. Randall L., acts of, as Regent ... xii, xiii
appointed Regent ... 1
Giessen, University of, sends dissertations ... 25, 84
Gifts to department of living animals ... 25
Goode, G. Brown, assistant secretary of the Institution ... xi
Göttingen, University of, sends dissertations ... 25, 84
Government, charge of exchange service assumed by ... 15, 16, 17

Page.
Government, bureaus, appropriations for exchanges xxxiii, 17, 18, 75
Departments and bureaus, co-operation of 45
establishments acting as exchange agents 80, 81
should pay for Smithsonian building 2
Governmental exchanges 16
Gray, Dr. Asa, death of, announced, and resolutions of Board xiii
work of, printed at expense of Institution 14
Great Britain, exchanges with 77, 78
Greece, exchanges with 77, 78
and Rome, time-keeping in: Address by F. A. Seely 377
Guadeloupe, exchange agency in 81
Guatemala, exchange agency in 81
Guttstadt, Prof. Albert, paper on the national scientific institutions at Berlin 89

H.

Habel bequest, amount of xix, 2
Hague, The, Netherlands, depository of United States official publications 77
Hayti, exchanges with 77, 78
Hamburg, exchanges with 77, 78
Hamburg-American Packet Company grant free freight 21, 80
Hamilton bequest, amount of xix, 2
Haupt, Prof. Paul, designated representative to Eighth International Congress of Orientalists 51
Hawaii, exchanges with 77, 78
Heating and lighting, Congressional appropriations for xxviii, xxx, xxxii, xxxiv, 3, 4
estimate of cost submitted to Congress 4
service of National Museum, salary schedule for 35, 38
Heidelberg, University of, sends dissertations 25, 84
Helsingfors, University of, sends dissertations 25, 84
Hemenway expedition, exchange privilege granted 15
Helmholtz, Robert, memoir of Gustav Robert Kirchhoff 527
Henderson & Brother grant free freight 21, 79
Heredity: Address by Dr. William Turner 541
Hertz's researches on electrical oscillations, by G. W. de Tunzelmann 145
High School, Technical, at Berlin, Germany, account of 130
Hillier, George, death of 16
Historical Association, American, Congressional act respecting incorporation of xxxi
Hobart Town, Tasmania, exchange agency in 77, 81
Horn, Dr. George H., work of, printed at expense of Institution 15
Hornaday, William T., paper on how to collect mammal skins 71
proposed plan for Zoological Garden 26
Hough, Walter, translation of Topinard's lecture on the last steps in the genealogy of man 669
House of Representatives, floor of, privilege of, pending 33
Hovey, H. C., on aluminum 721
Hungary, exchanges with 77, 78, 81
Hupa Reservation, Ray collection from, paper on, by Otis T. Mason 70
Hyatt, Prof. Alpheus, memoir by, on Genesis of the Arietidæ 69
Hydrographic office at Berlin, Germany, account of 138

I.

Illustrations, list of ix
Index to the literature of Columbium, by Frank H. Traphagen 69

Page.
India, exchanges with ... 77, 78, 81
Indian languages, study of, paper on, by J. W. Powell ... 65
    system of medicine investigated ... 56, 57, 58, 59, 61
    vocabularies collected by Mr. J. Curtin ... 61
        obtained by Mr. H. W. Henshaw ... 57, 58
Ingalls, Hon. John J., acts of, as Regent ... xii, xiii
    letter to, relative to classified service ... 34, 35, 36
Inman Steamship Line grant free freight ... 21, 80
Inorganic chemistry laboratory of the Technical High School at Berlin, Germany, account of ... 131
Insect collection, extent of ... 6, 43, 46
Institutions, the national scientific, at Berlin, paper on ... 89
Instructions in photography given by Museum ... 50
    taxidermy given by Museum ... 50
Instrument shelter erected in Smithsonian grounds ... 7
International Congress of Orientalists, aid given to ... 15
    exchanges (see Exchanges).
    Geological Congress, American Committee of, met at National Museum ... 50
Italy, exchanges with ... 77, 78

J.

Japan, exchanges with ... 77, 78
Java, exchange agency in ... 81
Jena, University of, sends dissertations ... 25, 84
Jordan, David Starr, explanation of plates by ... 69
Journal of proceedings of the Board of Regents ... xiii

K.

Kidder, Dr. Jerome H., bequest by ... 3
    curator of exchanges ... 3, 16
    death of ... 3, 16, 66
    report on exchanges ... 70
Kiel, University of, sends dissertations ... 25, 84
Königsberg, University of, sends dissertations ... 25, 84
Kunhardt & Co., grant free freight ... 21, 80

L.

Laboratories connected with Technical High School at Berlin, Germany, account of ... 131, 132
Langley, S. P., annual report of, to Regents ... 1
    Secretary and Director of the Institution ... xi
    letter to Hon. S. Dibble ... 27, 28, 29
        President of Senate relative to classified service for National Museum ... 34, 35, 36, 37, 38
    member of committee on eulogy of Dr. Asa Gray ... xiii
    report of, for 1888 ... 70
    report on Bureau of Ethnology ... 55
        Exchanges ... 15
        National Museum ... 34
Languages, bibliographies of, by J. C. Pilling ... 65
Lea, Isaac, collection of shells and books added to Museum ... 5, 47

Page.
Lecture delivered by Dr. Harrison Allen 32
  by Blanford, H. F., How rain is formed 257
    Lodge, Oliver J., The modern theory of light 441
    Topinard, Paul, The last steps in the genealogy of man 669
Lectures in National Museum 50
Leipzig, University of, sends dissertations 25, 84
Letter of Secretary of Smithsonian Institution to Hon. S. Dibble 27, 28, 29
  Secretary Evarts to Secretary of Smithsonian Institution 17, 76
  to Hon. John J. Ingalls, relative to classified service 34, 35, 36
Liberia, exchange agency in 81
Librarian, report of 83, 84
Library of Congress, books deposited in 22, 25, 83
  "Smithsonian Hall," in suggested xvii, 22
  National Museum, accessions to 47
Library of Smithsonian Institution 21
  academic publications of universities 25, 84
  accessions to 23, 25, 83
  books retained for Museum library 25
  books transferred to Surgeon-General's library 25, 83
  expenditures for xx
  in charge of Mr. J. Murdoch 24
  increased by means of exchange system 21, 23
  plan for increase of 23, 24
  reading room 23
  report of librarian 83, 84
  report of secretary 21
  resolution of Board of Regents relative to accommodations 22
  serials received by 83
Light, Michelson's recent researches on, address by Joseph Lovering 449
  modern theory of, lecture by Oliver J. Lodge 441
Lighting National Museum, Congressional appropriation for xxviii, xxx, xxxii, xxxiv, 3, 4
  service of National Museum, salary schedule for 35, 38
Lima, Peru, exchange agency in 77, 81
Lisbon, Portugal, exchange agency in 77, 81
List of illustrations ix
Literary exchanges. (See Exchanges.)
Living animals, collection of National Museum, statistics of accessions 40, 41
  Congressional appropriation for 4
  department of 25
  insufficient accomodations for 26
Locomotion, aerial, paper on, by F. H. Wenham 303
Locomotives, model of, presented by Baldwin Locomotive Works 44
Lodge, Prof. L. D., translation of paper on Scandinavian archaeology 571
Lodge, Prof. Oliver J., lecture: Modern theory of light 441
London, England, exchange agency in 77, 81
Louvain, University of, sends dissertations 25, 84
Lovering, Joseph, address: Michelson's recent researches on light 449
Lund, University of, sends dissertations 25, 84

M.

Madeira, exchange agency in 81
Madrid, Spain, exchange agency in 77, 81
Malta, exchange agency in 81

Page.
Mallery, Col. Garrick, work of 60
Man, genealogy of, the last steps in, lecture by Dr. Paul Topinard 669
Manila, Philippine Islands, exchange agency in 81
Marietta centennial exposition 53
Mason, Prof. Otis T., mission to Europe 51
Matter, molecular structure of, paper on, by William Anderson 711
Mauritius, exchange agency in 81
Medical Congress, met at National Museum 50
Medical Institutes at Berlin, Germany 121
Meetings and letters in National Museum 50
  of Board of Regents xiii
Meigs, General Montgomery C., acts of, as Regent xii, xiii
  member of executive committee xii
  offered resolution relative to Smithsonian Hall in new Library building xvii
  remarks on Zoological Park xvi
Melbourne, Victoria, exchange agency in 77, 81
Members *ex officio* of the establishment xi
Memoir of Gustav Robert Kirchhoff, by Robert von Helmholtz 527
  Heinrich Leberecht Fleischer. By Prof. A. Müller 507
Merchant S. L. Company grant free freight on exchanges for Egypt 21, 80
Meteorological Institute of the Royal University at Berlin, Germany, account of 109
  Observatory of the Agricultural High School at Berlin, Germany, account of 123
Meteorology, Bibliography of. By O. L. Fassig 271
  report on progress of. By George E. Curtis 205
Mexico, ethnological collection from 42
  exchanges with 77, 78, 81
Michelson's recent researches on light. Address by Joseph Lovering 449
Military medical institutes at Berlin, Germany 121
Mining Academy at Berlin, Germany, account of 128
Miscellaneous Collections, account of 11, 69, 70
Models of locomotives presented by Baldwin Locomotive Works 44
Modern theory of light. Lecture by Professor Oliver J. Lodge 441
Molecular structure of matter. Paper on, by William Anderson 711
Monrovia, Liberia, exchange agency in 81
Montevideo, Uruguay, exchange agency in 81
Montreal, Canada, exchange agency in 80
Morrill, Hon. Justin S., acts of, as regent xii, xiii
  bill for establishment of Zoological Park 29
  introduced bill relative to fire-proofing of Smithsonian building 7
Morton, Hon. Levi P., became *ex officio* regent 1
Mound explorations by Bureau of Ethnology 55, 65
Movements of the earth's crust. Paper on, by A. Blytt 325
Mozambique, exchange agency in 81
Müller, A., memoir of Heinrich Leberecht Fleischer 507
Muñoz y Espriella grant free freight 21, 80
Murdoch, John, in charge of library 25
  report on library 83, 84
Murray, Ferris & Co. grant free freight 21, 80
Museum, Botanical, of the Royal University at Berlin, Germany, account of 114
  Minerological, of the Royal University at Berlin, Germany, account of 111

Page.

Museum, Natural History, of the Royal University at Berlin, Germany, account of ... 119
  of Ethnography at Berlin, Germany, account of ... 136
  of Natural History, Paris, specimens of marble received from ... 44
  of the Agricultural High School at Berlin, Germany, account of ... 125
  Postal, at Berlin, Germany, account of ... 142
  Zoological, of Royal University at Berlin, Germany, account of ... 118

N.

National Academy of Sciences met at National Museum ... 50
  Dental Association met at National Museum ... 50
  Educational Association met at National Museum ... 50
National Museum:
  accessions to ... 42, 43, 44
    library ... 25, 47, 84
  annual report for 1886 ... 71
    appropriations and expenditures ... xxix, xxxiv
      Fish Commission, to alter Armory building ... xxxiv
      furniture and fixtures ... xxvii, xxix, xxxii, xxxiv
      heating and lighting ... xxviii, xxx, xxxii, xxxiv
      preservation of collections ... xxvii, xxix, xxxi, xxxiv
      postage ... xxxiv
      printing and binding ... xxxiv
  bibliography of ... 71
  Centennial Exhibition of Ohio Valley ... 51
  catalogue entries ... 42
  classified service, tables of ... 34, 35, 36, 37, 38
  Congressional appropriations for ... 3, 4
    disbursed by Smithsonian ... 5
  distribution of duplicate specimens ... 46
  co-operation of governmental Departments ... 45
  estimates submitted to Congress ... 4
  expenditures on account of ... xx
  explorations, account of ... 51
  governmental aid required for exhibitions ... 54
  increase of collections ... 39
  labels in use ... 46
  library of ... 25, 47, 84
  Marietta Centennial Exhibition, display at ... 53
  meetings and lectures ... 50
  personnel ... 51
  photographic exhibit ... 45
  principal accessions to collections ... 42, 43, 44
  publications ... 13, 47, 71
    Bulletins ... 47, 48, 49, 71
    Proceedings ... 47, 48, 49, 71
  report of Secretary ... 34
  requires additional building ... 5, 6, 7
  special researches ... 50
  statistics of accessions ... 40, 41
  students are granted access to collections ... 49
  visitors to ... 50

Page.
National scientific institutions at Berlin, paper on ... 89
Natural History illustrations by L. Agassiz and S. F. Baird ... 69
Natural History Museum of the Royal University at Berlin, Germany, account of. 119
Necrology ... 65
  Jerome H. Kidder ... 66
  James Stevenson ... 67
Netherlands, exchanges with ... 77, 78, 81
Netherlands American Steam Navigation Company, grant free freight ... 21, 80
New Caledonia, exchange agency for ... 81
New Foundland, exchange agency in ... 81
New South Wales, exchanges with ... 77, 78
New York and Brazil Mail Steamship Line, grant free freight ... 21, 80
New York and Mexico Steamship Company, grant free freight ... 21, 80
New Zealand, exchanges with ... 77, 78, 81
North American ethnology, appropriations and expenditures ... xxii
    Congressional appropriation for ... 34
    contributions to, published by Bureau of Ethnology. 65
    estimate of cost submitted to Congress ... 4
  Indian languages, bibliography of, by J. C. Pilling ... 65
  reptilia and batrachia, Professor Cope's work on ... 10
North German Lloyd, grant free freight ... 21, 80
Norway, exchanges with ... 77, 78, 81
Nursery, National, at Berlin, Germany, account of ... 127

O.

Observatories acting as exchange agents ... 80, 81
Observatory, astro-physical, near Potsdam, Germany, account of ... 133
  astro-physical of the Smithsonian Institution ... 7
  meteorological of the Agricultural High School at Berlin, Germany, account of ... 123
  of the Royal University at Berlin, Germany, account of ... 108
Ocean freight, estimated cost of ... 20, 75
Oelrichs & Co., grant free freight ... 21, 80
Official documents, international exchange of ... 76
Ohio Valley Centennial Exposition ... 47, 51
Organic chemistry laboratory of the Technical High School at Berlin, Germany, account of ... 131
Orientalists, Congress of, aid given to ... 15
Oscillations, electrical, Hertz's researches on, paper on by G. W. de Tunzelmann 145
Ottawa, Canada, exchange agency in ... 77, 80

P.

Pacific Mail Steamship Company, free freight by ... 21, 80
Panama Raiload Company, free freight by ... 21, 80
Packing boxes, cost of ... 74
Paraguay, exchanges with ... 77, 78, 81
Paramaribo, Dutch Guiana, exchange agency in ... 80
Paris, France, exchange agency in ... 77, 80
Parliamentary documents, immediate exchange of ... 20, 21, 76
  exchanges, condition of ... 17, 18, 19, 77
Pedological Institutes of the Agricultural High School at Berlin, Germany, account of ... 123, 124

Page.
Pendulum experiments of Coast Survey continued in Smithsonian building 32
Perott, de, Joseph, translation of memois of Gustav Robert Kirchhoff 527
Peru, exchanges with 42, 77, 78
Phelps, Hon. William W., acts of as Regent .... xii, xiii
Philippine Islands, exchange agency in 81
Photo-chemical laboratory of the Technical High School at Berlin, Germany, account of 132
Photographic exhibit in National Museum 45
Photography in the service of astronomy, by R. Radau 469
Physical cabinet of the Agricultural High School at Berlin, Germany, account of. 123
Physical Institute of the Royal University at Berlin, Germany, account of 110
  Observatory, plans for 7
  science collection of Institution, condition of 7
Physico-mathematical class of the Royal Academy of Sciences at Berlin, account of 89
Physiology, animal, Institute of, of the Agricultural High School at Berlin, Germany, account of 125
  elementary problems in, address by J. S. Burdon-Sanderson 423
  vegetable, institute of, of the Royal University at Berlin, Germany, account of 118
    Agricultural High School at Berlin, Germany, account of 124
Pilling, James C., bibliographical work of 62, 65
Pim, Forwood & Co., grant free freight 21, 79
Polynesia, exchanges with 76, 78, 81
Port-au-Prince, Hayti, exchanges with 77, 81
Porter, Dr. Noah, acts of, as Regent .... xii, xiii
Port Louis, Mauritius, exchange agency in 81
Portugal, exchanges with 77, 78, 81
  party to exchange convention 76
Postage of Institution, expenditures for .... xx, 4
Postal museum at Berlin, Germany, account of 142
Postmaster-General, co-operation of 45
Potsdam, Germany, astro-physical observatory near, account of 133
Powell, J. W., Director of the Bureau of Ethnology 55, 71
  collections received from 42
  introduction to the study of Indian languages 65
  report on work of Bureau of Ethnology 55, 71
Preservation of collections, Congressional appropriations for 3, 4
  estimate of cost submitted to Congress 4
Printing Bureau at Berlin, Germany, account of 143
  expenditures of Institution xx
  for exchange office, cost of 74
Prize questions of the Royal Frederick William's University at Berlin 105
Problems, elementary, in physiology. Address by J. S. Burdon-Sanderson 423
Proceedings of Board of Regents, journal of .... xiii, 70
  National Museum, account of 13, 48, 71, 89
Progress of anthropology in 1889, by Prof. Otis T. Mason 591
  meteorology in 1889, by George C. Curtis 205
Prussia, exchanges with 77, 78
Publications of the Bureau of Ethnology 13, 65, 71
  National Museum 13, 47, 71

Page.
Publications of the Smithsonian Institution 10, 69, 70, 71
    Annual reports 11, 12, 71
    Contributions to Knowledge 11, 69
    Distribution of 13, 47
    Miscellaneous Collections 11, 69
    Stereotype plates, storage of 15

Q

Quackenbush, John, appointed agent 16
Queensland, exchanges with 77, 78
Quito, Ecuador, exchange agency in 80

R

Radau, R., paper on photography in the service of astronomy 469
Rain, how formed, paper on, by H. F. Blanford 287
Reading room in Smithsonian library 23
Reception given by Secretary 34
Red Star Line grant free freight 21, 80
Regents of the Smithsonian Institution xii
    Board of, annual report for 1886 70
        journal of proceedings of xiii
            for 1886 70
        meetings of xiii
        report of Secretary to i
        executive committee of xii
    resolutions by xiii, xv, xvi, xvii, 22
Report of curator of exchanges 73
    executive committee of Board of Regents xix
    Samuel P. Langley, Secretary, for 1888 70
    Secretary to Board of Regents 1
    astronomical observatories for 1886, by George H. Boehmer 70
    exchanges and library (incorporated with Secretary's report).
    Smithsonian exchanges for 1887, by George H. Boehmer 70
Reptilia and Batrachia of North America, Professor Cope's work on 10
Researches on electrical oscillation, paper on, by G. W. de Tunzelman 145
Resolution by Regents relative to library 22
    Congressional, respecting Ohio Valley Centennial Exposition 52
    of Congress to print extra copies of the report ii
Resolutions of Board of Regents—
    on death of Dr. Asa Gray xiii, xv
    to appropriate income xvi
    to provide a Smithson hall in new building for Library of Congress xvii
    relative to deposit of books, etc., by American Historical Association xvii
Resolutions of Congress (see Acts and Appropriations).
Reykjavik, Iceland, exchange agency in 81
Rhees, William J., catalogue of Smithsonian publications by 70, 71
        chief clerk of the Institution xi
Rio de Janeiro, Brazil, exchange agency in 77, 80
Rock Creek valley the site for proposed Zoological Park 27, 28, 29
Rockhill, W. W., explorations by 9, 42
Rome, Italy, exchange agency in 77, 81
    time-keeping in Greece and, address by T. A. Seely 377

Page.

Rooms assigned for scientific work 32
Roscoe, Dr. Henry E., address: The life work of a chemist 491
Roumania, exchanges with 77, 78, 81
Russia, exchanges with 77, 78

S.

St. Helena, exchange agency in 81
St. John's, New Foundland, exchange agency in 81
St. Petersburg, Russia, exchange agency in 77, 81
Salaries of foreign agents 74
Institution, expenditures for xx
Salary schedule for National Museum 34, 35, 36, 37, 38
San José, Costa Rica, exchange agency in 80
San Salvador, exchange agency in 81
Santiago, Chili, exchange agency in 77, 80
Saturday lectures at National Museum 50
Saxony, exchanges with 77, 78, 81
Scaife, Walter B., paper on geographical latitude 749
Scandinavian archæology, by M. Ingwald Unset 571
School for gardening at Berlin, Germany, account of 127
Schumacher & Co. grant free freight 21, 79, 80
Scientific institutions at Berlin, paper on 89
Seals, oriental, presented by Mrs. Anna Randall Diehl 42
Secretary of Agriculture, co-operation of 45
Secretary, letter of, submitting annual report to Congress iii
remarks on Zoological Park xvi
report on exchanges 15
statement relative to statue for Professor Baird xvi
to expend income with advice of Executive Committee xvi
Secretary of State, letter of, to Secretary of Smithsonian Institution 17, 76
Seely, F. A., address by, on time-keeping in Greece and Rome 377
Seminary, Mathematical, of the Royal University at Berlin, Germany, account of 106
Servia, exchange agency for 76, 81
Shanghai, China, exchange agency in 80
Shed, temporary, for astro-physical observations 33
Skinner, Aaron N., translation by, of paper on photography in the service of astronomy 469
Skull, clinical study of, lecture on, by Dr. H. Allen 32
Smithson bequest, amount of xix, 2
Smithsonian building, repairs, appropriation, and expenditures for xxiv
repairs required by 7
exchanges, report for 1887, by George H. Boehmer 70
fund, money paid by, on account of exchanges 17, 18
Grounds, base line established by Geological Survey 33
Hall in new Library of Congress building 22
Institution, the agent of the Government for international exchanges 15, 16, 17
disburses appropriation for National Museum 5
to be custodian of the Zoological Park 31
publications 70, 71
stereotype plates, storage of 15
Sonrel, A., drawings by 69
South Australia, exchanges with 77, 78

Page.
Spain, exchanges with 77, 78
Spofford, A. R., co-operation with Smithsonian library 24
Standard Measures Commission at Berlin, Germany, account of 137
State Department, co-operation of 45
letter to secretary of Smithsonian Institution 17, 76
Statue proposed to Professor Baird 32
Stearns, Silas, death of 51
Stereotype plates of Smithsonian Institution now stored in Smithsonian building. 15, 33
Stevenson, James, necrology of 67
Stockholm, Sweden, exchange agency in 77, 81
Students, assistance given to 49
Stuttgart, Würtemberg, depository of United States official publications in 77
Surgeon-General's library, books transferred to 25, 84
Sweden, exchanges with 77, 78, 81
Switzerland, exchanges with 77, 79, 81
Sydney, New South Wales, exchange agency in 77, 81
Szold, Henrietta, translation of memoir of Heinrich Leberecht Fleischer 507

T.

Tasmania, exchanges with 77, 79, 81
Technical High School at Berlin, Germany, account of 130
Technological Institute of the Royal University at Berlin, Germany, account of. 113
Telegraph Bureau, Central, at Berlin, Germany, account of 139
expenditures of Institution xx
Telephone service at Berlin, Germany, account of 139
Terrestrial Globe at the Paris Exhibition, paper on 745
Testing station, royal, for building material, at Berlin, Germany, account of 133
Thaw collection of apparatus loaned to Institution 7
Theory, modern, of light, lecture by Oliver J. Lodge 441
Thiselton-Dyer, W. T., address, Botanical Biology 399
Thompson, Sir William, on Boscovich's theory 435
Time-keeping in Greece and Rome, address by E. A. Seely 377
Tokio, Japan, exchange agency in 77, 81
Toner lecture fund, condition of 32
Topinard, Dr. Paul, lecture, The last steps in the genealogy of man 669
Toronto, Canada, depository of United States official publications in 77
Traphagen, Frank W., index to literature of Columbium 69
Translations by—
Bleismer, C. A., Anthropology in the last twenty years 555
Boehmer, George H., The national scientific institutes at Berlin, Germany 89
Dallas, W. S., Movements of the earth's crust 325
Hough, Walter, The last steps in the genealogy of man 669
Lodge, L. D., Scandinavian archæology 571
de Perott, Joseph, Memoir of Gustav Robert Kirchhoff 527
Skinner, Aaron N., Photography in the service of astronomy 469
Szold, Henrietta, Memoir of Heinrich Leberecht Fleischer 507
Transportation companies, acknowledgment to, for free freight 21, 79
Treaty of Brussels, of 1886 .... 18, 20, 76
Tübingen, University of, sends dissertations 25, 84
Tunzelmann, G. W., paper on Hertz's researches on electrical oscillations 145
Turkey, exchanges with 77, 79, 81
Turner, Sir William, address on heredity 541

Page.

U.

University publications received by library 25, 84
garden, at Berlin, Germany, account of 116
library, at Berlin, Germany, account of 106
Unset, M. Ingwald, paper on Scandinavian archaeology 571
Uruguay, exchanges with 77, 81
Utrecht, University of, sends dissertations 25, 84

V.

Vanden Toorn, H. W., grants free freight 80
Vasey, Dr. George, appointed curator of botany 51
Vegetable division of the Museum of the Agricultural High School at Berlin, Germany, account of 125
physiology, Institute of, of the Agricultural High School at Berlin, Germany, account of 124
Royal University at Berlin, Germany, account of 118
Venezuela, exchanges with 77, 79
Victoria, exchanges with 77, 79, 81
Vienna, Austria, exchange agency in 77, 80
Virchow, Dr. Rudolph, address: On anthropology in the last twenty years 555
Visitors to National Museum, number of 50, 51

W.

Washington, Lawrence, deposited a number of articles belonging to General Washington 44
Waves, electrical, Hertz's researches on 145
Welling, Dr. James C., acts of, as Regent xii, xiii
member of committee on eulogy of Dr. Asa Gray xiii
executive committee xii
presented report of executive committee xv
Wellington, New Zealand, exchange agency in 77, 81
Wenham, F. H., paper on aerial locomotion 303
Wesley, William, & Son, acknowledgments of services rendered 79
exchange agents of the Institution 80, 81
West Indies, exchanges with 76, 79
Wheeler, Hon. Joseph, acts of, as Regent xii, xiii
White, Dr. Andrew D., acts of, as Regent xii, xiii
elected member of Board of Regents xv
White, Charles A., recommended acceptance of Hyatt's memoirs 69
White Cross Line, grant free freight 21, 80
Wilson & Asmus, grant free freight 21, 80
Wilson, Thomas, mission to Europe 51
Winlock, W. C., appointed curator of exchanges 16
bibliography of astronomy for 1887 70
report on exchanges 73
Wright, Peter, & Sons, grant free freight 21, 80
Würtemberg, exchanges with 77, 79, 81
Würzburg, University of, sends dissertations 25, 84

Page.

Y.

Yana vocabulary, collected by Mr. J. Curtin 61
Yarrow, Dr. H. C., collections received from 42
Introduction to the study of Mortuary customs 65

Z.

Zoological Garden at Berlin, Germany, account of 120
Institute of the Agricultural High School at Berlin, Germany, account of 125
University at Berlin, Germany, account of 119
Museum at Florence, Italy, presented specimens 43
of Royal University at Berlin, Germany, account of 118
Park .... xxxiv, 27
action of Board of Regents relative to xvi
amendment to bill establishing 30
Congressional action relative to establishment of 27, 28, 29, 30, 31
report of Congressional Committee on Public Buildings and Grounds 27
Secretary's letter to Congress 29
report on 29
commission appointed 30
occupies rooms in Smithsonian building 32
Zoo-technical Institute of the Agricultural High School at Berlin, Germany, account of 126

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